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# Contents

## ARTICLES

- Morton Weinfeld, John J. Sigal, William W. Eaton*  
Long-Term Effects of the Holocaust on Selected Social  
Attitudes and Behaviors of Survivors: A Cautionary Note 1

- Kristin A. Moore, Linda J. Waite*  
Marital Dissolution, Early Motherhood and Early Marriage 20 ✓

- Tim B. Heaton, William B. Clifford, Glenn V. Fuguitt*  
Temporal Shifts in the Determinants of Young and Elderly  
Migration in Nonmetropolitan Areas 41

- John R. Logan, Linda Brewster Stearns*  
Suburban Racial Segregation as a Nonecological Process 61

### 2 on Crime & Delinquency

- Dennis W. Roncek*  
Dangerous Places: Crime and Residential Environment 74

- David W. Mann*  
Age and Differential Predictability of Delinquent Behavior 97 ✓

### 4 on Aspects of Work

- Mary L. Fennell, Miriam B. Rodin, Glenda K. Kantor*  
Problems in the Work-Setting, Drinking, and Reasons  
for Drinking 114 ✓

- Brian L. Pitcher*  
The Hawthorne Experiments: Statistical Evidence for a  
Learning Hypothesis 133

- Joan Huber, Glenna Spitze*  
Wives' Employment, Household Behaviors, and  
Sex-Role Attitudes 150 ✓

- Lynn K. White, David B. Brinkerhoff*  
The Sexual Division of Labor: Evidence from Childhood 170

### 3 on Movements & Associations

*Carol M. Mueller, Charles M. Judd*  
Belief Constraint and Belief Consensus: Toward an Analysis  
of Social Movement Ideologies—A Research Note 182

*Bonnie H. Erickson*  
Secret Societies and Social Structure 188

*Michael Hanks*  
Youth, Voluntary Associations and Political Socialization 211 ✓

### Commentaries

*Richard S. Bobys*  
Another Methodological Confounder: Comment on Witt,  
Lowe, Peek, and Curry 224

*David D. Witt, George D. Lowe, Charles W. Peek, Evans W. Curry*  
Presence of Significant Others and the Avowal of  
Happiness Among the Elderly: Reply to Bobys 227

### BOOK REVIEWS

*Jerome L. Himmelstein*  
Review Essay—The Two Nisbets: The Ambivalence of  
Contemporary Conservatism  
(*Robert Nisbet—History of the Idea of Progress*) 231

*David Thomas*  
Naturalism and Social Science *Ernst Nagel* 236

*Robert Alun Jones, Henrika Kuklick (eds.)*  
Research in Sociology of Knowledge, Sciences and Art: II  
*Kurt Lang* 238

*Keith Dixon*  
The Sociology of Belief *Patrick H. McNamara* 239

*Robert Wuthnow (ed.)*  
The Religious Dimension *Hart M. Nelsen* 241

*Lesley Johnson*  
The Cultural Critics *Jeffrey C. Goldfarb* 243

---

<i>Piotr Sztompka</i> Sociological Dilemmas <i>Robert J. Antonio</i>	245
<i>Franco Ferrarotti</i> An Alternative Sociology <i>Andrea Fontana</i>	247
<i>Ralf Dahrendorf</i> Life Chances <i>Guenther Roth</i>	248
<i>Graham Room</i> The Sociology of Welfare <i>Kirsten A. Grønberg</i>	250
<i>Paul Blumberg</i> Inequality in an Age of Decline <i>Nancy Esteb</i>	251
<i>Paul Bernstein</i> Workplace Democratization <i>Curt Tausky</i>	253
<i>Samuel Clark</i> The Social Origins of the Irish Land War <i>Karl Bottigheimer</i>	254
<i>James Bertram Haugh</i> Power and Influence in a Southern City <i>Anthony Andrew Hickey</i>	256
<i>Franklin D. Wilson</i> Residential Consumption, Economic Opportunity, and Race <i>John Edwards</i>	257
<i>Sandra Perlman Schoenberg, Patricia L. Rosenbaum</i> Neighborhoods that Work <i>Michael D. Grimes</i>	258
<i>Shirley Bradway Laska, Daphne Spain</i> Back to the City <i>Michael D. Grimes</i>	258
<i>Carol D. H. Harvey, Howard M. Bahr</i> The Sunshine Widows <i>Patricia MacCorquodale</i>	260
<i>Andrew T. Scull</i> Museums of Madness <i>Virginia Aldigé Hiday</i>	261
<i>David Robinson (ed.)</i> Alcohol Problems <i>Samuel E. Wallace</i>	263
<i>James V. Spotts, Franklin C. Shontz</i> Cocaine Users <i>James T. Carey</i>	263

<i>Vincent Mosco</i> Broadcasting in the United States <i>Haluk Sahin, John P Robinson</i>	265
<i>National Research Council</i> Privacy and Confidentiality as Factors in Survey Response <i>Richard C. Rockwell</i>	267
<i>M. N. Srinivas et al. (eds.)</i> The Fieldworker and the Field <i>John B. Stephenson</i>	269
<b>TAKE NOTE</b>	271
<b>CORRESPONDENCE</b>	280

## Contents

### ARTICLES

#### Part I: The Sociology of Leisure

Special Editor: *Robert N. Wilson*

*Robert N. Wilson*

Introduction 281

*Robert N. Wilson*

The Courage To Be Leisured 282

*John R. Kelly*

Leisure Interaction and the Social Dialectic 304

*Stanley Parker*

Change, Flexibility, Spontaneity, and  
Self-Determination in Leisure 323

*Mihaly Csikszentmihalyi*

Leisure and Socialization 332

*Nancy Theberge*

A Critique of Critiques: Radical and Feminist  
Writings on Sport 341

*Susan Birrell*

Sport as Ritual: Interpretations from Durkheim to Goffman 354

*Lars Björn*

The Mass Society and Group Action Theories of  
Cultural Production: The Case of Stylistic Innovation in Jazz 377

*Dale Dannefer*

Neither Socialization Nor Recruitment:  
The Avocational Careers of Old-Car Enthusiasts 395

#### Part II

*Irving L. Webber*

Sociology: Parochial or Universal? 416

*J. David Lewis, Andrew J. Weigert*

The Structures and Meanings of Social Time 432

## **2 on Pelf & Power**

*Ann R. Tickamyer*  
Wealth and Power: A Comparison of Men and  
Women in the Property Elite 463

*Michael Patrick Allen*  
Managerial Power and Tenure in the Large Corporation 482

## **3 on Religion & Its Correlates**

*Whitney Pope, Nick Danigelis*  
Sociology's "One Law" 495 ✓

*Philip K. Hastings, Dean R. Hoge*  
Religious Trends Among College Students, 1948-79 517 ✓

*John P. Marcum*  
Explaining Fertility Differences Among U.S. Protestants 532

## **3 on Mobility & Its Measures**

*Keith Hope*  
The New Mobility Ratio 544

*K. I. Macdonald*  
On the Formulation of a Structural Model of  
the Mobility Table 557

*Robert M. Hauser*  
Hope for the Mobility Ratio 572

## **COMMENTARIES**

*Warren Handel*  
The Danger of Committing the Ecological Fallacy Persists:  
Comment on Gove and Hughes 585

*Michael Hughes, Walter Gove*  
Theory and Research in a Dangerous World: Reply to Handel 589





<i>Christopher Bagley, Gajendra K. Verma</i> Racial Prejudice, The Individual and Society	<i>Alvin W. Rose</i>	621
<i>Pat Lauderdale (ed.)</i> A Political Analysis of Deviance	<i>Julian B. Roebuck</i>	624
<i>Lonnie H. Athens</i> Violent Criminal Acts and Actors	<i>June Kress</i>	626
<i>Robert Dreehan, J. Alan Thomas (eds.)</i> The Analysis of Educational Productivity, Vol. I: Issues in Microanalysis	<i>Maurcen Hallinan</i>	627
<i>Charles E. Bidwell, Douglas M. Windham (eds.)</i> The Analysis of Educational Productivity, Vol. II: Issues in Macroanalysis	<i>Maurcen Hallinan</i>	627
<i>Jeffrey P. Rosenfeld</i> The Legacy of Aging	<i>Fred C. Pampel</i>	630
<i>Elise Boulding</i> Children's Rights and the Wheel of Life	<i>Florence Karlstrom</i>	631
<i>Robert A. Stebbins</i> Amateurs	<i>Louis Kutcher</i>	633
<i>David G. Bromley, Anson D. Shupe, Jr.</i> "Moonies" in America	<i>William Sims Bainbridge</i>	634
<i>Irving Louis Horowitz (ed.)</i> Science, Sin, and Scholarship	<i>William Sims Bainbridge</i>	634
<i>Fred Davis</i> Yearning for Yesterday	<i>William S. Fox</i>	636
<i>Joseph P. Morrissey et al. (eds.)</i> The Enduring Asylum	<i>Robert J. Gregory</i>	637
<i>T. J. Scheff</i> Catharsis in Healing, Ritual, and Drama	<i>Graeme Newman</i>	639
<i>Gila Hayim</i> The Existential Sociology of Jean-Paul Sartre	<i>Mark J. LaFountain</i>	641

*Gunnar Boalt, Ulla Bergryd*  
Sociologists in Search of Their  
Intellectual Domain *Robert McAulay* 642

*Guy Benton Johnson, Guion Griffis Johnson*  
Research in Service to Society *Richard Robbins* 644

**TAKE NOTE** 647

# AUTHORS' GUIDE (revised 1981)

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## Contents

### ARTICLES

- Carol Mueller, Thomas Dimieri*  
The Structure of Belief Systems Among  
Contending ERA Activists ✓ 657

- Isaac W. Eberstein, W. Parker Frisbie*  
Metropolitan Function and Interdependence in the  
U.S. Urban System 676

- Robert McGinnis, Paul D. Allison, J. Scott Long*  
Postdoctoral Training in Bioscience: Allocation and Outcomes 701

- Harold Fallding*  
G. H. Mead's Orthodoxy 723

- John F. Zipp, Joel Smith*  
A Structural Analysis of Class Voting ✓ 738

#### 4 Bearing on Crime

- Allen E. Liska, Joseph J. Lawrence, Andrew Sanchirico*  
Fear of Crime as a Social Fact ✓ 760

- David F. Greenberg, Ronald C. Kessler*  
The Effect of Arrests on Crime: ✓  
A Multivariate Panel Analysis 771

- Charles H. Logan*  
Problems in Ratio Correlation: The Case of ✓  
Deterrence Research 791

- David F. Luckenbill*  
Compliance Under Threat of Severe Punishment ✓ 811



- Willard Rodgers*  
Trends in Reported Happiness Within  
Demographically Defined Subgroups, 1957-78 826

<i>James R. Marshall, George W. Dowdall</i> Employment and Mental Hospitalization: The Case of Buffalo, New York, 1914-55	843
---	-----

### 3 Research Notes

<i>Michel Tousignant, H. B. M. Murphy</i> The Epidemiological Network Survey: A New Tool for Surveying Deviance and Handicaps	854
---	-----

<i>Ralph R. Sell</i> A Note on the Demography of Occupational Relocations	859
--	-----

<i>Elizabeth Mutran, Linda K. George</i> Alternative Methods of Measuring Role/Identity	866
--	-----

### COMMENTARIES

<i>Thomas M. Gannon, Elizabeth A. Freidheim</i> "Structuralism" or Structure: A Comment on Mayhew	877
--	-----

<i>Marietta Morrissey</i> The Dua' Economy and Labor Market Segmentation: A Comment on Lord and Falk	883
--	-----

<i>George W. Lord, III, William W. Falk</i> Dual Economy, Dual Labor, and Dogmatic Marxism: Reply to Morrissey	891
--	-----

<i>Armand L. Mauss</i> Salvation and Survival on Skid Row: A Comment on Rooney	898
---	-----

<i>James F. Rooney</i> Reply to Mauss	905
--	-----

<b>AUTHORS' GUIDE</b>	908
-----------------------	-----

### BOOK REVIEWS

<i>Hubert M. Blalock, Jr. (ed.)</i> Sociological Theory and Research <i>Neil J. MacKinnon</i>	909
--	-----

<i>Charles A. Goldsmid, Everett K. Wilson</i> Passing on Sociology <i>Reece McGee</i>	913
<i>Neil J. Smelser, Robin Content</i> The Changing Academic Market <i>Lionel S. Lewis</i>	916
<i>Carol H. Weiss, Michael J. Bucuvalas</i> Social Science Research and Decision-Making <i>George H. Conklin</i>	919
<i>Rosemary Crompton, Jon Gubbay</i> Economy and the Class Structure <i>Larry J. Griffin</i>	920
<i>Arthur Marwick</i> Class <i>Robert V. Robinson</i>	922
<i>Kay Lehman Scholzman, Sidney Verba</i> Injury to Insult <i>Paul Burstern</i>	924
<i>David Caplovitz</i> Making Ends Meet <i>J. Herman Blake</i>	925
<i>Gunnar Boalt, Ulla Bergryd et al.</i> Professionalization and Democratization in Sweden <i>Lars Björn</i>	927
<i>Gino Germani</i> Marginality <i>Pat Lauderdale</i>	928
<i>Roland Robertson, Burkart Holzner (eds.)</i> Identity and Authority <i>Alvin Rose</i>	930
<i>Samuel B. Bacharach, Edward J. Lawler</i> Power and Politics in Organizations <i>Peter V. Marsden</i>	932
<i>Jeffrey Pfeffer</i> Power in Organizations <i>Peter V. Marsden</i>	932
<i>Thomas M. Guterbock</i> Machine Politics in Transition <i>Joseph Galaskiewicz</i>	936
<i>Graeme R. Newman (ed.)</i> Crime and Deviance <i>Gary LaFree</i>	937
<i>Robert Prus, Styllianoss Irini</i> Hookers, Rounders, and Desk Clerks <i>Charles A. Sundholm</i>	939

<i>Helen F. Antonovsky</i> <i>Adolescent Sexuality</i> <i>J. Richard Udry</i>	941
<i>William B. Sanders</i> <i>Rape and Woman's Identity</i> <i>Barbara F. Reskin</i>	942
<i>Murray Milner, Jr.</i> <i>Unequal Care</i> <i>Dennis Milet</i>	943
<i>Ida Harper Simpson</i> <i>From Student to Nurse</i> <i>Madelene H. Schmitt</i>	944
<i>James A. Christensen, Jerry W. Robinson, Jr. (eds.)</i> <i>Community Development in America</i> <i>Carlton R. Sollie</i>	946
<i>Dan A. Chekki (ed.)</i> <i>Community Development; Participatory Democracy in Action</i> <i>Carlton R. Sollie</i>	946
<i>Robert Miles, Annie Phizacklea (eds.)</i> <i>Racism and Political Action in Britain</i> <i>John Stone</i>	949
<i>John R. Salter, Jr.</i> <i>Jackson, Mississippi</i> <i>Jay Weinstein</i>	951
<i>Harold E. Quinley, Charles Y. Glock</i> <i>Anti-Semitism in America</i> <i>Celia S. Heller</i>	952
<i>Thomas Robbins, Dick Anthony (eds.)</i> <i>In Gods We Trust</i> <i>William M. Newman</i>	954
<i>Anson D. Shupe, Jr., David G. Bromley</i> <i>The New Vigilantes</i> <i>William Sims Bainbridge</i>	955
<i>Robert N. Thomas, John M. Hunter (eds.)</i> <i>Internal Migration Systems in the Developing World</i> <i>Joachim Singelmann</i>	957
<i>Robert F. Bales, Stephen P. Cohen</i> <i>SYMLOG</i> <i>John Stephen Brennan</i>	959
<b>TAKE NOTE</b>	962
<b>REFEREES</b>	966



## Contents

### ARTICLES

*Frederick D. Weil*

- Tolerance of Free Speech in the United States and  
West Germany, 1970–79: An Analysis of  
Public Opinion Survey Data 973

*John Hagan*

- The Corporate Advantage: A Study of the Involvement of  
Corporate and Individual Victims in a Criminal Justice System 993

*Peter V. Marsden, John Shelton Reed, Michael D. Kennedy,  
Kandi M. Stinson*

- American Regional Cultures and Differences in  
Leisure Time Activities 1023

### 6 on Polity & Economy

*William Form*

- Self-Employed Manual Workers:  
Petty Bourgeois or Working Class? 1050

*Kenneth A. Bollen, Scott T. Jones*

- Political Instability and Foreign Direct Investment:  
The Motor Vehicle Industry, 1948–65 1070

*Philip Janson, Jack K. Martin*

- Job Satisfaction and Age: A Test of Two Views 1089

*Melissa A. Hardy*

- Social Policy and Determinants of Retirement:  
A Longitudinal Analysis of Older White Males, 1969–75 1103

*Kenneth M. Johnson*

- Organization Adjustment to Population Change in  
Nonmetropolitan America: A Longitudinal Analysis of  
Retail Trade 1123

*John F. Zipp, Richard Landerman, Paul Luebke*

- Political Parties and Political Participation: A Reexamination of  
the Standard Socioeconomic Model 1140

## COMMENTARIES

*Brian Powell, Lala Carr Steelman*

Fundamentalism and Sexism: A Reanalysis of Peek and Brown 1154

*Charles W. Peek*

Deficient Methods or Different Data? Another  
Interpretation of Divergent Findings on Fundamentalism  
and Political Sexism: Response to Powell and Steelman 1159

*Gary D. Sandefur, Nancy J. Finley*

Specifying Models of Status Inconsistency:  
A Comment on Wilson and Zurcher 1168

*Kenneth L. Wilson*

Multiple Regression with Dummy Variables:  
Response to Sandefur and Finley 1171

*David F. Gillespie, Dennis S. Mileti*

Differentiation in Organizations: A Comment on  
Miller and Conaty 1172

*George A. Miller, Joseph Conaty*

Further Differentiation: Response to Gillespie and Mileti 1176

*L. Neil Guppy*

On Intersubjectivity and Collective Conscience in  
Occupational Prestige Research: A Comment on  
Balkwell-Bates-Garbin and Kraus-Schild-Hodge 1178

*James W. Balkwell, Frederick L. Bates, Albeno P. Garbin*

Does the Degree of Consensus on Occupational Status  
Evaluations Differ by Socioeconomic Stratum?  
Response to Guppy 1183

*Robert W. Hodge, Vered Kraus, E. O. Schild*

Consensus in Occupational Prestige Ratings:  
Response to Guppy 1190

## BOOK REVIEWS

*Seymour Martin Lipset (ed.)*

The Third Century John H. Faris 1197

<i>Immanuel Wallerstein</i> The Modern World-System II <i>Guenther Roth</i>	1199
<i>Ellen Kay Trimberger</i> Revolution From Above <i>Quee-Young Kim</i>	1201
<i>Eva Etzioni-Halevy</i> Political Manipulation and Administrative Power <i>Peter Abell</i>	1204
<i>Morton Schoolman</i> The Imaginary Witness <i>Lawrence E. Hazelrigg</i>	1205
<i>Hans P. M. Adriaansens</i> Talcott Parsons and the Conceptual Dilemma <i>Jere Cohen</i>	1207
<i>Nicholas Abercrombie et al.</i> The Dominant Ideology Thesis <i>David Zaret</i>	1208
<i>Julius I. Loewenstein</i> Marx Against Marxism <i>David Zaret</i>	1208
<i>Tom W. Goff</i> Marx and Mead <i>Robert L. Stewart</i>	1210
<i>Julia A. Sherman, Evelyn Torton Beck (eds.)</i> The Prism of Sex <i>Cynthia Fuchs Epstein</i>	1211
<i>Cynthia Fuchs Epstein, Rose Laub Coser (eds.)</i> Access to Power <i>Gwen Moore</i>	1213
<i>John Scanzoni, Maximiliane Szinovacz</i> Family Decision-Making <i>Spencer Cahill</i>	1215
<i>Kurt W. Back (ed.)</i> Life Course <i>Sarane Spence Boocock</i>	1216
<i>Elizabeth S. Johnson, John B. Williamson</i> Growing Old <i>Arnold Arluke</i>	1218
<i>Richard P. Coleman, Lee Rainwater</i> Social Standing in America <i>William H. Sewell</i>	1219
<i>Vincent T. Covello (ed.)</i> Poverty and Public Policy <i>David Elesh</i>	1220

<i>David J. Hunter</i> Coping With Uncertainty <i>Mary L. Fennell</i>	1222
<i>Bruce J. Biddle</i> Role Theory <i>Francis M. Sim</i>	1224
<i>E. Gordon Ericksen</i> The Territorial Experience <i>Daniel J. Monti</i>	1226
<i>David L. Brown, John M. Wardwell (eds.)</i> New Directions in Urban-Rural Migration <i>Kenneth M. Johnson</i>	1227
<i>Lee-Jay Cho, Kazumasa Kobayashi (eds.)</i> Fertility Transition of East Asian Populations <i>Fred W. Reed</i>	1229
<i>Clarence E. Glick</i> Sojourners and Settlers <i>Robert Seto Quan</i>	1231
<i>Darrel Montero</i> Japanese Americans <i>Guy Dalto</i>	1233
<i>Edna Bonacich, John Modell</i> The Economic Basis of Ethnic Solidarity <i>Guy Dalto</i>	1233
<i>William R. Beer</i> The Unexpected Rebellion <i>Edward A. Tiryakian</i>	1235
<i>Raymond L. Hall</i> Black Separatism in the United States <i>James H. Laue</i>	1237
<i>Loretta J. Williams</i> Black Freemasonry and Middle-Class Realities <i>H. Viscount Nelson, Jr.</i>	1238
<i>LaFrances Rodgers-Rose (ed.)</i> The Black Woman <i>Marguerite E. Bryan</i>	1240
<i>Paul C. Higgins</i> Outsiders in a Hearing World <i>Gary L. Albrecht</i>	1241
<i>H. Stith Bennett</i> On Becoming a Rock Musician <i>Robert M. Khoury</i>	1242
<i>Peter K. Manning</i> Police Work <i>James P. Lynch</i>	1244

<i>Marshall B. Clinard, Peter C. Yeager</i> Corporate Crime <i>Richard C. Hollinger</i>	1245
<i>Pat Carlen, Mike Collison (eds.)</i> Radical Issues in Criminology <i>Michael Gottfredson</i>	1247
<i>Michael Pratt</i> Mugging as a Social Problem <i>Alden D. Miller</i>	1248
<i>Benjamin Zablocki</i> Alienation and Charisma <i>George V. Zito</i>	1249
<i>David Martin</i> The Breaking of the Image <i>Donald L. Noel</i>	1251
<i>Gary Schwartz, Don Merten</i> Love and Commitment <i>Morris Friedell</i>	1252
<b>TAKE NOTE</b>	1255
<b>INDEX TO VOLUME 60</b>	1262

## Notes on the Authors

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Along with his co-authors, James **Balkwell** carries on the conversation with colleagues reacting to the paper "On the Intersubjectivity of Occupational Status Evaluations." He is studying the subjective views that invest occupations with differentiated meanings; and pursues the problem of distinguishing between socialization and allocation effects on status attainment.

Unlike most of his clan, Kenneth **Bollen** works outside the groves (in the Societal Analysis Department of GM's research laboratories). He has studied imitative suicides and suicidal motor vehicle fatalities (with David Phillips). His article with Scott Jones is one of his studies in international development.

Joseph **Conaty** continues his collaboration with George Miller on the comparative analysis—cross-national and longitudinal—of factors affecting the structure of organizations.

Disruptions in Guatemala are hindering Frederick **Bates'** study of the earthquake in that country in 1976, the social conditions defining the crisis and shaping responses to it, both for the victim community and the rehabilitation agencies. He is also working on an introductory textbook while he continues his longstanding research into differing patterns of social organization.

Nancy **Finley** is doing work on her dissertation, at the University of Oklahoma, an inquiry into conditions and consequences of domestic violence. She also collaborates with Gary Sandefur on research on status inconsistency and class identification.

In this issue, William **Form** continues to mine a rich vein in his studies of the organization of work. He has under way a long-term study of working class stratification in the United States and France. He is also investigating the coherence of ideologies of economic justice, by stratum.

Albeno **Garbin** responds, with his co-authors to a comment on their study of occupational status evaluation. His own work poses problems about factors affecting mid-career changes, the work patterns of spouses of professional persons, and the link between leisure behavior and various occupations.

With his co-author, Dennis Mileti, **David Gillespie** is studying the effects of increasing and decreasing government regulations on major organizations in American industry. They continue to explore the issues, in methods and theory, that can enlarge our understanding of the dimension of organization size.

One of his research projects is reflected in L. Neil **Guppy's** comment on Balkwell et al., his pursuit of two questions bearing on occupational prestige. One is the issue of shared agreement among respondents; the other, the determinants of occupation prestige. Along with five colleagues he has just begun a four-year study of the fishing industry in British Columbia, its impact on political, industrial and environmental features of coastal communities; and changes in the fishing labor force.

**John Hagan** continues his work on criminal justice decision-making and reform. A recently published paper discusses "Race, Class and the Perception of Criminal Injustice in America" and a book on *Crime, Criminal Behavior and its Control* is in preparation (McGraw-Hill).

**Melissa Hardy's** productive work in gerontology includes an investigation of occupation and industry differences in retirement behavior of older men (funded by the National Institute on Aging). Her future work, informed by an interest in structural (including stratification) variables will entail a longitudinal treatment of retirement behavior and policy.

Although he is working on a book on causal models, **Robert Hodge** is simultaneously engaged in a large-scale study of postwar fertility decline in Japan—i.e., a late stage in Japan's demographic transition.

**Philip Janson** also has a major interest in gerontology and is pursuing various questions touching on age and cohort variations in job satisfaction and alienation.

In addition to continuing his research on the impact of population change on the nonmetropolitan infrastructure, **Kenneth Johnson** is studying rural migration and population patterns in the 1930s and 1940s. In cognate studies, he is working out the historical context needed to analyze antecedents and consequences of the resurgent growth of nonmetropolitan areas.

**Scott Jones** is a senior planner for Anaconda Aluminum where his responsibilities include research on international markets, competing materials and price forecasts. Formerly Staff Research Scientist at GM, where the work with co-author Bollen was begun, he is developing measures of the political environment as the aluminum industry expands its interests abroad.

A doctoral student at UNC-Chapel Hill, Michael Kennedy is studying the relationship between the makeup of municipal budgets and urban fiscal strain. He is also studying the evolution of political processes and urbanization patterns in Eastern Europe.

Vered Kraus is a faculty member both at Haifa University and Hebrew University of Jerusalem. Although she continues her work on occupational prestige, she is also studying people's evaluation of units other than occupations such as, for example, neighborhoods.

Richard Landerman who works at the Center for the Study of Aging and Human Development at Duke is investigating the structure and determinants of prejudice toward old people; and the nature of anti-government and anti-business sentiment in the United States.

With his co-authors, Landerman and Zipp, Paul Luebke continues his reexamination of conventional sociological explanations of political attitudes and behavior among the working class in the United States. His own main line of inquiry asks about the social bases of elite and mass politics in contemporary North Carolina.

His article (with Reed, Kennedy, and Stinson) reflects Peter Marsden's interest in adapting methods to substance of sociological inquiry. Marsden is editor of the just published book (Sage), *Linear Models in Social Research*. He works on problems of stratification, formal organizations, and network analysis.

Over the past several years, Jack Martin has been investigating aspects of occupations and organizations that influence worker satisfaction. His most recent work examines alternative models that include sex of worker and family status in accounting for worker satisfaction.

Dennis Mileti comments in this issue, with co-author David Gillespie, on the paper, "Differentiation in Organizations . . ." by Miller and Conaty (*SF* 59:1[September 1980]). Mileti has had a special interest in the response of organizations to stressful environments. He has recently done a stint with the California Seismic Safety Commission, working on the development of statewide plans for response to disasters.

George Miller is studying (with Joseph Conaty) the determinants and outcomes of variations in organizations' structure—both cross-nationally and longitudinally.

Charles Peek responds to others' responses to his paper (with Sharon Brown) on "Sex Prejudice Among White Protestants . . ." His ongoing



work includes studies of the linkages between athletics and deviance; and sex roles and deviance.

One of Peek's responders is **Brian Powell** who is conducting research on sex-based variations in the prestige ratings of occupations.

**John Shelton Reed** has, for a decade, published studies of regionalism in the United States. He is editor (with Merle Black) of the annual publication, *Perspectives on the American South* and is author of the just-published work, *One South* (LSU Press). He is also working on a study of the Anglo-Catholic movement in the nineteenth-century Church of England.

While continuing his work on problems of job mobility and migration, **Gary Sandefur** is also investigating (with Nancy Finley) the effects of status inconsistency on subjective class identification.

**E. O. Schild**, co-author with Hodge and Kraus, is Rector of the University of Haifa. He has a continuing interest in problems of stratification but his work reflects a special commitment to inquiries in the sociology of education.

**Lala Carr Steelman** is working on the problem of explaining variations in cognitive ability. Together with Brian Powell she is evaluating maternal effects on sex role attitudes.

**Kandi Stinson** is a doctoral student in sociology at UNC-Chapel Hill. She does research in sex roles and gerontology. At the moment she is examining regional differences in the sex role-related attitudes of women. In future she plans to study the division of household labor by married couples in their retirement years.

His paper in this issue stems from **Frederick Weil's** dissertation, "Post-fascist Liberalism: The Development of Political Tolerance in West Germany Since World War II," now being revised for publication. He plans to continue his research on factors promoting liberal and democratic values in several Western countries. He is also investigating the changing bases of support for social democratic and labor parties in Western Europe.

**Kenneth Wilson** responds in this issue to a late hit, a response to his (and Louis Zurcher's) 1979 papers on status inconsistency. With **Alejandro Portes**, Wilson is investigating the economic development of the Cuban enclave in Miami, part of a larger inquiry into the economic development of minority enclaves in America.

**John F. Zipp** continues work with Luebke and Landerman on the economic attitudes of Americans. He is also beginning work on a study of the social costs of a plant closing.

# AUTHORS' GUIDE (revised 1982)

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### EXAMPLE.

"After Mills wrote *The Power Elite*, other writers (Bell, Clinard, b) stated that . . ." [Separate authors by semicolon. For authors with more than one work cited, use only "a," "b," "c," etc. Use "and" for two authors, "et al" for three or more, but include all names in references following text.]

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Miller, W. A. Miller, and G. Kline 1975 *The CPS 1974 American National Election Study* [MRDF] Ann Arbor: Center for Political Studies, University of Michigan [producer]; Ann Arbor: Inter-University Consortium for Political and Social Research [distributor]

U.S. Bureau of the Census 1979 *1970 Census of Population and Housing, Fourth Count Population Summary Tape* [MRDF] Washington: U.S. Bureau of the Census [producer]; Rosslyn, Va.: DUALabs [distributor]

## Notes on the Authors

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In this issue Richard **Bobys** raises a question about methods used by Witt, Lowe, Peek, and Curry in their study of "The Changing Association Between Age and Happiness" (*SF* 58:4, June 1980). Bobys does research in deviant behavior, medical sociology, and social psychology and is currently investigating social problems of the handicapped, factors contributing to the stigmatization of ex-mental patients, and academic fraud.

His paper with Lynn White reflects David **Brinkerhoff's** continuing studies of sex roles, including the relative biological and social contributions to sex-specific dominance behaviors. He is also doing research on the position of children in the family power structure.

Research reported in the paper on "Temporal Shifts in the Determinants of Young and Elderly Migration . . ." was conducted while William **Clifford** was visiting professor at the University of Wisconsin-Madison. He is investigating migration of the elderly, with special attention to metropolitan-nonmetropolitan variation.

Evans **Curry** is doing research on the relationships between religion and delinquency; and between family size and intelligence.

William **Eaton** ("Effects of the Holocaust") is Assistant Chief, Center for Epidemiologic Studies at NIMH. He works on methodological developments in psychiatric epidemiology, including quantitative longitudinal modeling, studies of classification, diagnosis and validity, and the calculation of true rates for mental disorder. He has a special interest in the social epidemiology of schizophrenia.

With her co-investigator, T. A. Nosanchuk, Bonnie **Erickson** is conducting a study of a network of competitive bridge players in Ottawa-Hull. Topics of investigation include the effects of relational structure on attitudes, information flow, and access to resources; the effects of participation and social ties on political participation and stereotyping; and problems of social comparison in a natural setting.

Mary **Fennell** is inquiring into the effects of structural problems in the

work setting on drinking practices and attitudes, a study that also investigates the influence of organizational factors on the adoption of alcohol programs in Illinois. She is also involved in a study of structural characteristics of hospital clusters, their sources and their effects on the cost and quality of hospital care. She is also collaborating on a monograph with Richard B. Warnecke on the structure, processes, and effectiveness of hospital networks.

Glenn **Fuguitt** is doing research on various aspects of population change in metropolitan and nonmetropolitan areas, including growth and decline of cities and villages, residential preferences, and temporal trends in migration patterns.

Michael **Hanks** ("Youth, Voluntary Associations and Political Socialization") has special interests in the outcomes of education and the social aspects of physical disability.

At the Family and Demographic Research Institute, Brigham Young University, Tim **Heaton** is investigating aspects of population redistribution in the United States. He is currently busy with a study of internal migration in Thailand, Egypt, and Colombia.

Recently sentenced to administrative duties as Head of her Department, Joan **Huber** nonetheless pursues research on the long-term effects of the decline in fertility and the rise in women's labor force participation. With her co-author, Glenna Spitze ("Wives' Employment, Household Behaviors, and Sex-Role Attitudes") she is working on a book using data generated by a grant from NSF.

Charles **Judd**, a psychologist and co-author with Carol Mueller ("... Toward an Analysis of Social Movement Ideologies") does research on attitudes and public opinion; and on methods of applied social research.

A doctoral student in sociology at the University of Illinois, Glenda **Kantor** is studying the effects of sex role perceptions, social support systems and parental role on the life stability of narcotic-dependent women. A related research interest is the evaluation of the effects of women's self-help health care groups on members' health.

John **Logan** continues his research on patterns of stratification (and their causes) in metropolitan communities. He is also working on a monograph on working-class politics in contemporary Spain and Portugal.

George **Lowe's** major interest is in secondary analysis, especially as it can be applied to problems in rural and psychiatric sociology.

With Linda Waite, Kristin **Moore**, a social psychologist, writes in this issue on a problem that reflects her specialty at the Urban Institute on women and family policy. She does research on the determinants and consequences of teenage childbearing and is now conducting a study of the public sector costs of teenage childbearing. Other research bears on the marital and family effects of women's employment.

As a Mellon Fellow at the Murray Research Center, Radcliffe College, Carol **Mueller** is engaged in research on women in public office. She is studying the part played by public opinion in mediating the relationship between social movements and electoral politics.

Charles **Peek** who responds to Bobys' comment is studying the link between deviance and sex roles; and between deviance and participation in athletics.

His paper on the Hawthorne experiments does not precisely reflect Brian **Pitcher's** three sets of research interests. They are analytical models of collective violence, including evaluation of mathematical models of diffusion and collective learning and comparison of structural, conflict and diffusion theories of collective behavior; role transitions through the life cycle, including cohort patterns of role-taking and adaptation to role loss; and migration patterns among aging males, including correlates of migration propensity, types, direction and distance.

Miriam **Rodin** is studying those features of organizations that influence perceived job stress and alcohol consumption. Her other interests include the study of organizational factors favoring the adoption of alcohol programming among Illinois employers, referral outcome studies of public inebriates, cultural factors in alcohol treatment clinics, and descriptive epidemiology of anorexia nervosa.

Dennis **Roncek** is a devoted urbanist. His research interests grow out of his concern with improving the quality of life in central cities. He is investigating problems of public housing and processes of urban change, including urban redevelopment.

As part of a series of studies of the consequences of parental preoccupation, John **Sigal** is currently engaged in a study of adult children of survivors in order to examine second- and possibly third-generation effects of the Holocaust, and is completing a study of the children of parents who spent some

*of their childhood in an orphanage. He is also examining the correlates and possible determinants of selective perception of male and female voices.*

In addition to her ongoing research with Joan Huber, **Glenna Spitze** is investigating the relationship between female employment and family migration, and is collaborating with Steve Rytina on a study of equity norms and household labor.

**Linda Brewster Stearns** is a graduate student in sociology at SUNY, Stony Brook. She is co-author of *The Politics of Privacy* and is now doing research for her dissertation on capital markets and corporate control.

The timing of the transition to wedlock and parenthood among young women is the subject of research that **Linda Waite** is conducting in collaboration with Glenna Spitze. With Ross Stolzenberg, she is studying local labor market effects on the labor force participation of married women.

**Morton Weinfeld** is collaborating with John Sigal in research on the effects of the Holocaust on adult children of survivors. In the realm of ethnic relations, he is studying the links between ethnic diversity and public policy in Canada; and is doing theoretical and empirical work on the political economy of minority groups.

**Lynn White** is Director of the Bureau of Sociological Research at the University of Nebraska and for four years has been Director of the Nebraska Annual Social Indicators Surveys. Her current work includes research on the impact of female employment opportunities on marriage and marital stability.

**David Witt** is a graduate student whose interest in aging and happiness has led to research on types and conditions of family interaction. He is now directing a study of domestic violence as it affects marital and life satisfaction.

# Long-Term Effects of the Holocaust on Selected Social Attitudes and Behaviors of Survivors: A Cautionary Note

MORTON WEINFELD, *McGill University*

JOHN J. SIGAL, *Sir Mortimer B. Davis-Jewish General  
Hospital, Montreal*

WILLIAM W. EATON, *National Institute of Mental Health*

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## ABSTRACT

*A random sample of Jewish survivors of the Holocaust in Montreal is compared with two Jewish control groups. Modest or insignificant differences were found on measures of perceived anti-Semitism, economic and political satisfaction, social segregation, economic achievement, and propensity to migrate from Quebec. The findings caution against overgeneralization of a clinical construct, the survivor syndrome, and point to the need for further research into the remarkable capacities of human beings to overcome the most severe forms of victimization.*

This study seeks to identify effects of the Holocaust (attempted destruction of European Jewry) on attitudes and behaviors of Jewish survivors in Montreal some 33 years after the end of the war. The scientific literature on Holocaust survivors is predominantly psychiatric and psychoanalytical and relies heavily on clinical studies (Hoppe). These studies have documented various dysfunctional behaviors and mental disorders, of differing severity, which have been labeled the "survivor syndrome" (Krystal and Niederland) and include such symptoms as anxiety, depression, guilt, paranoia, psychosomatic disease, disturbances in cognition, as well as occupational and interpersonal maladjustment (Antonovsky et al.; Chodoff; Eitinger; Krystal; Matussek; Niederland).

Those studies which have used controls have also found persisting effects of the Holocaust on survivors. Shuval found Jewish survivors in Israel (in 1950) to be generally more pessimistic about the future, somewhat hardened to additional strain, but not more distrustful of the outside world. Eitinger and Strøm found that non-Jewish concentration camp survivors in Norway had less successful occupational careers compared to controls and

were in poorer health. These findings tend to support the clinically derived findings of continuing effects.

From the perspective of the sociology of ethnic and race relations, Jewish survivors of the Holocaust can be seen as victims of extreme out-group hostility and persecution. Any study of the long-term effects of such trauma must take into account the subsequent setting, particularly in cases where victims are members of racial or ethnic minority groups, and where this very minority group status was a core reason for the victimization. We might well expect the subsequent behavior of such victimized group members, in another *minority setting*, to differ from behavior in settings in which they are a dominant majority (e.g., Jews in Israel, Norwegians in Norway).

Jewish survivors living in North America would be subject to a threefold strain: as minority members of a society; as immigrants facing problems of adjustment and integration; and as victims of a prior, extreme form of specific persecution.

The Montreal setting adds a unique dimension to this study of survivors. One might argue that the characteristics of the Montreal setting would increase the likelihood of finding behavioral and attitudinal differences in our sample of survivors.

Briefly, a reawakening of French nationalism in Quebec, begun in the 1960s, culminated in the victory of the pro-independence Parti Québécois in the provincial elections of 1976. The apprehension of Jews included fears about the potential for anti-Semitism, which has been associated historically with nationalist movements (including right-wing nationalist movements in Quebec in the 1930s) and with periods of economic and political uncertainty. These fears existed in some quarters, even though the Parti Québécois government itself had provided no evidence, in deed or in declaration, to support even a hint of anti-Semitism (Waller; Weinfeld).

For various reasons, the Jewish community in Montreal has evolved as an English-speaking community, and shared a general misgiving with other English-speakers regarding the increase in Québécois nationalism and the possibility of Quebec's independence from Canada. An immediate consequence of the uncertainty has been a publicized exodus of anglophones, including Jews, from Quebec. It is in this context of uncertainty that one might more readily anticipate that survivors in Montreal would exhibit the sorts of behaviors described below.

An exhaustive computer-assisted search of the literature has revealed no previous study of long-term effects of extreme victimization on members of any minority group (not only Jews), using non-clinical, randomly selected control and experimental groups, in a setting of persisting minority status. (The few non-clinical studies which exist lack one or more of these characteristics; but we refer to these studies when relevant.) This study meets these characteristics, and in addition has a sufficiently



large sample of Holocaust survivors to permit a more confident statistical analysis.

This paper investigates such effects on five variables: perception of anti-Semitism; satisfaction with economic and political conditions; segregation within the Jewish community; economic achievement; probability of emigration from Quebec.

## Methods

The data for this study were drawn from a 1978 survey of a sample of 657 Jewish household heads in Montreal.<sup>1</sup> The survey was designed to provide a data base for Jewish community and welfare planning in Montreal. The sample was selected randomly from a master list of Jewish households, which included between 85 and 90 percent of the estimated number of Jewish households in Montreal, according to the 1971 census.<sup>2</sup>

One question was: "Excluding service in the Allied Armed forces, were you in Europe during World War II?" Of our 657 respondents 135 answered "Yes" to that question and it is this group which comprised the sample of survivors.<sup>3</sup> These persons included those whose primary experience was in a concentration camp (39), labor camp (17), in hiding (25), in armed resistance or in a regular army (13), and 41 who did not specify their primary experience. It was decided to create two control groups. (Non-Ashkenazim were not chosen for the controls since all survivors were Ashkenazim; in addition, only respondents over 40 were included in both control groups and the survivor group, since most survivors were over 40.) The first control group (N = 120) consisted of foreign-born respondents who had not experienced the Holocaust; the vast majority had emigrated to Canada before the 1930s. The second control group (N = 196) consisted of Canadian-born respondents.

These three groups were compared for differences in distributions of the dependent variables. The comparisons of major interest are those between the survivor group and immigrant control group, since both those groups share a European background (with probably some taste of European anti-Semitism), as well as the need to cope with the difficulties of immigration. The data are presented in Tables 2 through 6. Chi square was used to determine the level of significance of between-group differences. A regression analysis, presented in Table 7 (looking at the survivor group and the immigrant control group) isolates the independent effects of surviving the Holocaust, while controlling for other background variables.<sup>4</sup>

Table 1 presents the distribution of several demographic variables for the three groups. The survivor group is younger than the immigrant control group, but older than the native group (remember all respondents

**Table 1.** DEMOGRAPHIC CHARACTERISTICS OF ONE SUBJECT AND TWO CONTROL GROUPS

Demographic Characteristics	Categories		
	Survivor	Immigrant	Native
Percent 60 and over*	54	83	39***
Percent male	75	68	73
Percent highly educated**	49	46	76***
Percent high on religiosity†	39	43	25***

\*All respondents were 40 and over.

\*\*Completed 11 or more years of education.

\*\*\*Chi square significant at .01.

†See Appendix: V.

are over 40). The groups do not differ significantly in their sexual composition. The native-born control group has a higher level of education than that of the two other groups, which have similar distributions. Finally, the survivor and immigrant groups display similar levels of religiosity (an ordinal index including synagogue attendance and ritual observances; see Appendix. Most of the differences are between the native group and the two others; the survivor and immigrant control groups differ only in age distribution, the survivor group being younger; this difference is controlled in the regression analysis. (All interaction effects for survivors and the background variables were found to be insignificant.)<sup>5</sup>

## Results and Discussion

### 1. PERCEPTION OF ANTI-SEMITISM

There seem to be no major empirical investigations of the long-term effects of a previous episode of severe victimization due to minority group status, on victims' perceived discrimination in the present. One possible effect of such a prior episode might be an oversensitivity to possible victimization, leading to continuing perceptions of prejudice and discrimination after a negative stimulus has been removed, and a transfer of such perceptions from a threatening to non-threatening environment. Such hypersensitivity is a popular stereotype of both Jews and blacks, and might be considered as a component of the paranoia described as an element of the psychological profile of survivors analyzed in previous clinical and other studies (Krystal and Niederland; Matussek).

Shuval studied “mistrust, suspicion and perception of hostility to the outside world” of her Jewish survivors in Israel—to the physical environment and to difficulties in immigrant adjustment in Israel. Her finding of no difference between survivors and controls is suggestive, but does not measure an ethnic-racial dimension, such as the level of anti-Semitism which survivors perceive to be held by a gentile majority.

Table 2 presents the distributions for an index of perceived anti-Semitism. (See the Appendix for details on the construction of this and other indices used in the analysis). Proportions of those who perceive a

**Table 2.** LEVEL OF PERCEIVED ANTI-SEMITISM COMPARISON OF SURVIVORS, IMMIGRANTS, AND NATIVES

	Categories			
	Survivor	Immigrant	Native	Total
A. <u>Level of Perceived Anti-Semitism</u>				
Low	54	54	51	53
High	46	46	49	47
N	(105)	(113)	(189)	(407)
<u>Survivor-Immigrant</u>		<u>Three Groups</u>		
$\chi^2 = .00$ (d.f. = 1)		$\chi^2 = 0.4$ (d.f. = 2)		
p = .99		p = .79		
B. <u>Extent Change in Anti-Semitism in Past Years</u>				
No anti-Semitism	3	4	2	3*
Decreased	5	6	12	9
Same	45	55	59	55
Increased	47	35	27	34
N	(75)	(83)	(156)	(314)
<u>Survivor-Immigrant</u>		<u>Three Groups</u>		
$\chi^2 = 2.4$ (d.f. = 3)		$\chi^2 = 11.9$ (d.f. = 6)		
p = .48		p = .063		
C. <u>Possibility of Anti-Semitism</u>				
Not mentioned	64	57	69	64
Mentioned	36	43	31	36
N	(111)	(111)	(190)	(412)
<u>Survivor-Immigrant</u>		<u>Three Groups</u>		
$\chi^2 = 1.13$ (d.f. = 1)		$\chi^2 = 4.436$ (d.f. = 2)		
p = .29		p = .115		

\*Percentages do not sum to 100, owing to rounding.

high level of anti-Semitism are similar in survivor and control groups. Survivors did perceive an increase in anti-Semitism in Quebec over the past five years (Table 2:B). Yet this perception of increasing anti-Semitism by survivors has not produced a significantly higher level of perceived anti-Semitism in the present. (Perhaps five years ago, survivors may have perceived lower levels of anti-Semitism than others.) From the regression equation we note the low and insignificant beta (.04) for the survivor variable, suggesting no greater level of perceived anti-Semitism for survivors compared to other foreign-born respondents.

Respondents were also asked to select from among several possible reasons, those for which they would (hypothetically) leave Quebec. Survivors were not found to be more likely than native control groups to select "possibility of anti-Semitism" as a first or second reason (Table 2:C).

Why do survivors not seem to perceive higher levels of anti-Semitism? One explanation may be that their experiences have inured them to subsequent developments. This explanation is roughly analogous to Shuval's claim that survivors were more hardened than controls to new sources of strain in their environment. (In her study, hardening was operationalized to mean a smaller shift from optimistic to pessimistic attitudes when confronted with strain). A similar pattern emerged in a recent study of crime victims, which found that victims and non-victims do not seem to differ in the degree of their concern about crime (Thomas and Hyman). In any event, we find in our study no evidence of greater paranoia-like fear of majority-group hostility on the part of the survivors.

## 2. SATISFACTION WITH ECONOMIC AND POLITICAL CONDITIONS

The clinical literature on survivors has identified the symptoms of anxiety, worry, and generalized dissatisfaction as typical (Krystal and Niederland). Antonovsky et al., in a study done some 25 years after the war, found that female survivors in Israel were more given to worry than a control group. As indicated earlier, Shuval found survivors in Israel to be generally more pessimistic about their future (when not subject to particular strain) than controls.

Table 3 presents the distribution for an index of satisfaction with economic and political conditions in Quebec, for our three groups (see Appendix:II). To be sure, this index does not measure personal worry or anxiety, but sociopolitical attitudes. Yet given the uncertainties in the political and economic environment in Quebec, these are of interest. We find no evidence to suggest that the survivors are less satisfied than the controls. Thus even if survivors do suffer from personal anxieties as suggested by the clinical literature, they do not seem to have been generalized to affect assessments of sociopolitical conditions.

This is confirmed in Table 7 where the unique effect of being a

**Table 3.** POLITICAL AND ECONOMIC SATISFACTION COMPARISON OF SURVIVORS, IMMIGRANTS, AND NATIVES

Index of Level of Satisfaction	Categories			
	Survivor	Immigrant	Native	Total
Low	47	53	58	54
High	53	47	42	46
N	(107)	(116)	(193)	(416)
Survivor-Immigrant		Three Groups		
$\chi^2 = .80$ (d.f. = 1) $p = .38$		$\chi^2 = 3.9$ (d.f. = 2) $p = .14$		

survivor is modestly and insignificantly associated with greater, not less satisfaction ( $\beta = .10$ ). Among some survivors, evaluations of present conditions may involve comparison with the far greater misery or danger of the past.

3. SEGREGATION WITHIN THE JEWISH COMMUNITY

Sociological theory has argued that the experience of out-group hostility contributes to development of solidarity among the in-group (Coser; LeVine and Campbell; Sherif and Sherif; Simmel). This teleological formulation may be based either on seeing self-segregation as an adaptive response to hostility (or a rational strategy in cases of conflict) or on the fact that out-group discrimination eliminates opportunities and restricts the freedom of in-group members, imposing the segregation by design. Wirth applied both perspectives to his study of American Jewry, stressing the effect of anti-Semitism in sustaining Jewish identity. This view of ethnic segregation has been challenged of late by an emphasis on a voluntaristic approach on the part of the minority groups. Thus we find there are adaptive functions of ethnic communities for the integration of immigrants (Breton) and even apparent preferences among native-born members of minority groups for residential segregation (Kantrowitz; Richmond).

Jews have remained highly segregated both residentially and socially in North American cities, including Montreal (Richmond; Sklar; Weinfeld and Eaton). One might reasonably expect that, whether by choice or by necessity, survivors might be more segregated within the Jewish community (being both immigrants as well as victims of extreme out-group hostility).

Table 4:A presents the distribution for an index of segregation which combines data on both social (friendship networks) and residential seg-

Table 4.

A. Segregation (Social and Residential). Comparison of survivors, Immigrants, and Natives

Index of Segregation	Categories			Total
	Survivor	Immigrant	Native	
Low	26*	29*	28	28
Medium	39	43	38	39
High	35	28	34	33
N	(119)	(119)	(133)	(431)
Survivor-Immigrant		Three Groups		
$\chi^2 = 1.35$ (d.f. = 2)		$\chi^2 = 2.0$ (d.f. = 4)		
$p = .52$		$p = .73$		

B. Perceived Anti-Semitism by Segregation

Segregation	Low	High	Total
Low	29*	26*	27
Medium	38	40	39
High	34	33	34
N	(210)	(191)	(401)
$\chi^2 = .544$ (d.f. = 2)			
$p = .77$			

\*Percentages do not sum to 100, owing to rounding.

gation (see Appendix: III). Survivors do not seem more segregated within the Montreal Jewish community than other Jews. The regression analysis reveals, however, a modest and statistically significant ( $\beta = .16$ ) relation between survivors and greater social segregation, when compared to other immigrants. Thus having experienced the Holocaust may lead to a somewhat greater segregation.

As demonstrated, survivors do not perceive more anti-Semitism than non-survivors. Levine and Campbell have suggested that the perception of threat may be as effective as real threat in developing feelings of in-group solidarity. A brief digression is revealing. Table 4:B presents data for both the survivor group and the two controls combined, relating perceived anti-Semitism and social segregation. We find no relation between the two. Those perceiving little anti-Semitism are as likely to be segregated as those who perceive much. While further research is neces-

sary, it may be that the experience of anti-Semitism, not its perceived existence, makes a greater difference in the segregation of Jews. Our survey also found, for example, that far more respondents perceived at least "a bit" of anti-Semitism in Montreal (85%) than had actually been victimized by it personally (50%).

The weak effect of perceived anti-Semitism suggests that some portion of Jewish social segregation is voluntary. For example, the survey revealed a general relation between religiosity and segregation: Jews who are more religious tend to be more socially segregated ( $r = .25$ ). While this may reflect non-Jewish exclusion of those Jews who are more religious, voluntary factors must not be discounted. The strictures of Orthodoxy such as the requirements of walking to synagogue on the Sabbath, the dietary requirements, inhibiting interfaith socializing, proscriptions against intermarriage, etc., all limit the potential of interaction with non-Jews.

#### 4. ECONOMIC ACHIEVEMENT

Journalistic accounts of survivors in America highlight cases of dramatic entrepreneurial success (Rabinowitz). Yet scientific studies with control groups (Eitinger) and without (Matussek) have described impairment in economic achievement for concentration camp survivors. Survivors have experienced interrupted education and training, the erosion of skills, and the destruction of careers or businesses in full bloom, all of which could adversely affect subsequent life chances. This is quite apart from the obvious physical and mental strain of the experience, which might also affect economic achievement.

Table 5 presents comparative data for three indicators of economic achievement. Looking first at the measures of family income and occupational status (measured by the Blishen index)<sup>6</sup> we note large gaps between the native group and the other two groups. The native advantage is a result of this group's youth (implying earned incomes rather than retirement pensions) as well as their higher educational level. The data do not show the survivors to be substantially worse off than the immigrant controls for these two variables, and the two groups are comparable in educational attainment (Table 1). The regression analysis, however, reveals that survivors do seem to be significantly, and adversely, affected by their experience compared to other immigrants (standardized betas of  $-.27$  and  $-.19$  for income and occupation respectively). Still, both this and the tabular data suggest that large numbers of survivors were indeed able to make satisfactory economic adjustments.

Support for this view is seen from the patterns for intragenerational mobility observed for the three categories, presented in Table 5:C. The measure in question is based on the difference in Blishen scores of respondents' first full-time job in Canada and their current one, or their last

**Table 5.** ECONOMIC ACHIEVEMENT COMPARISON OF SURVIVORS, IMMIGRANTS, AND NATIVES

NATIVES		Categories			
		Survivor	Immigrant	Native	Total
A	<u>Family Income</u>				
	Less than \$20,000	70	70	33	53
	More than \$20,000	30	30	67	47
	N	(98)	(91)	(160)	(349)
	<u>Survivor-Immigrant</u>			<u>Three Groups</u>	
	$\chi^2 = 0.00$ (d.f. = 1)			$\chi^2 = 48.3$ (d.f. = 2)	
	p = .99			p = .001	
B	<u>Occupational Status</u>				
	Low	72	65	49	60
	High	28	35	51	40
	N	(102)	(98)	(178)	(378)
	<u>Survivor-Immigrant</u>			<u>Three Groups</u>	
	$\chi^2 = 1.14$ (d.f. = 1)			$\chi^2 = 15.8$ (d.f. = 2)	
	p = .26			p = .001	
C	<u>Intragenerational Mobility</u>				
	Downwardly mobile	9	13	15	13
	Immobile	47	39	35	39
	Slightly mobile	20	28	31	27
	Highly mobile	25	20	20	21
	N	(113)	(108)	(156)	(377)
	<u>Survivor-Immigrant</u>			<u>Three Groups</u>	
	$\chi^2 = 3.3$ (d.f. = 3)			$\chi^2 = 7.7$ (d.f. = 6)	
	p = .32			p = .26	

\*These ranks are formed by grouping the differences in the Blischen scores of respondents' present, last, or usual job if unemployed and their first job in Canada.

Percentages do not sum to 100, owing to rounding.

or usual one if they were currently unemployed. There is no significant difference among any of the groups, with survivors slightly (if insignificantly) more likely to be highly upwardly mobile and less likely to be downwardly mobile. This is supported by the low beta coefficient (.00) in Table 7.

Many of those in the survivor group may have begun their occupational careers in Canada as refugees, at positions far below their abilities, and thus the subsequent gains would reflect the temporary effects of their desperate initial condition. The phenomenon of initial loss of occupational



status is common to many immigrants, and can be assumed to be even more acute for refugees. Yet the survivors revealed little cumulative or prolonged loss of occupational position, as indicated by the mobility data. Stated simply, they seem to have made the best of a bad situation.

5. PROBABILITY OF EMIGRATION

Historians have noted the failure of German Jews to recognize and then deal adequately with the incipient dangers of the Nazi threat (Poppel). Their well-known attachment to Germany, blinding them to the mounting danger, is often cited as a major contributing factor. If minority groups learn from the experiences of their collective past, then the dangers of inertia and procrastination are one such Jewish lesson from the Holocaust. As we have indicated, no objective parallels can be drawn between the Germany of the 1930s and Quebec today. Nor does anti-Semitism loom large as a factor in the thinking of respondents about possible emigration from Quebec. Yet in view of publicized concern over the exodus of anglophones, including Jews, it is of interest to examine whether survivors are more likely to consider emigration, than are other Jews, regardless of the specific causes at work.

Very few if any of the respondents had made definite or tentative plans for emigration. Respondents were asked to estimate the probability of their being in Montreal in five years time, under a "status quo" assumption, and under the assumption of clear support for Quebec's independence in the upcoming referendum (since rejected). The responses were combined to form an index of propensity to emigrate (see Appendix:IV).

As seen in Table 6, the native group has the highest proportion of those most likely to emigrate. In large part this is due to their younger age.

**Table 6.** PROBABILITY OF MIGRATION WITHIN FIVE YEARS COMPARISON OF SURVIVORS, IMMIGRANTS, AND NATIVES

Probability of Migration	Categories			
	Survivor	Immigrant	Native	Total
Low	44*	44	26	35
Medium	38	37	48	42
High	19	19	26	22
N	(85)	(90)	(162)	(337)
<u>Survivor-Immigrant</u>		<u>Three Groups</u>		
$\chi^2 = .01$ (d.f. = 2)		$\chi^2 = 12.1$ (d.f. = 4)		
$p = .91$		$p = .02$		

\*Percentages do not sum to 100, owing to rounding.

Yet we find no difference between the survivor and immigrant groups. Indeed, the regression coefficient of Table 7 indicates a modest and *negative* (if statistically insignificant) association of being a survivor and probability of emigration ( $b = -.10$ ). This finding is not surprising, given our inability to document either a greater perception of anti-Semitism or lower levels of satisfaction on the part of survivors.

**Table 7.** STANDARDIZED BETAS\* INDEPENDENT EFFECTS OF BEING A HOLOCAUST SURVIVOR (REGRESSION CONTROL FOR AGE, SEX, EDUCATION, RELIGIOSITY)

Dependent Variable	Standardized Beta
Perceived anti-Semitism	.04
Level of satisfaction	.10
Social segregation	.16***
Probability of migration	-.10
Family income**	-.27***
Occupational status	-.19***
Mobility	.00

\*For Foreign Born respondents, over 40, of Ashkenazi Origin.

\*\*The equation for family income also controlled for presence of a working spouse.

\*\*\*Significant F statistic ( $p \leq .05$ ).

## Conclusions

The data presented reveal a consistent pattern of no significant or modest differences between the survivors and the immigrant control group. Of course, one cannot prove the null hypothesis. Yet while we cannot be certain that no differences exist (i.e. that survivor behavior is not unique) we have not been able to support hypothesized patterns that might be anticipated from previous work. Having survived the Holocaust contributes slightly to the degree of segregation within the Jewish community, and survivors have relatively lower occupational status and earnings (though they are not less occupationally mobile). No significant differences were found in perceptions of current anti-Semitism, level of economic and political satisfaction, and probability of emigration.

One possible explanation for this pattern of findings of modest or no effects is that this study examines survivors and controls in a setting in which both are in a minority group situation. The extensive work on Jewish survivors in Israel, or Norwegian survivors in Norway, does not reflect this

condition. A possible interpretation is, therefore, that the inclusive minority (Jewish) category dominates our survivor–non-survivor distinctions. Perhaps in a setting where Jews are in the vast majority, to have been a Holocaust survivor (in a society, Israel, which exalts self-defense) may become a salient distinction. Where Jews are in a minority, and perhaps experiencing various degrees of insecurity or victimization, being a survivor may represent simply one more dimension of minority status, and survivor–non-survivor differences for the variables measured here may be lessened.

Another possibility is that the heterogeneity of our sample's actual wartime experience precludes the emergence of a distinctive pattern of outcomes (see note 3). While some studies focus on the specific experience of concentration camp survivors, others treat, more inclusively, survivors of Nazi persecution, where survivors with different types of experiences are grouped together (Sigal et al.). Yet at a minimum, to have lived as a Jew in wartime Europe is to have endured a generalized climate of the most extreme anti-Semitism and to have lived in constant fear for one's life. It is due to this common denominator of extreme environmental hostility that some specific consequences were anticipated. Few were found.

A final possibility is that the culling process during their incarceration and subsequently resulted in only the initially fittest or more competent people surviving 33 years later. During concentration camp selection the Commandant sent those he judged to be physically or mentally unfit to work to the gas chambers. In labor camps, factories, and mines, the less fit were literally worked to a death hastened by poor nutrition, beatings, and disease.

It is also likely that malnutrition contributed further to the culling process after the surviving victims were released from the camps. Prisoners in the Nazi camps received 1,200 calories a day in the first years, and only 800 calories per day in the last year before their release. Although post-release mortality data are not available for the group that we are examining in this paper, Eitinger and Strøm found a significant relationship between the degree of malnutrition and postrelease mortality rates in their non-Jewish sample of survivors of the Nazi camps. Death by suicide or homicide may have further culled this group in the immediate postwar period. Eitinger and Strøm reported higher mortality due to these causes in their survivor group.

Thus, we cannot reject the possibility that survivors in our sample constituted a group that was potentially capable of mental or physical achievements superior to that of our immigrant control group. The long-term deleterious effects of their concentration camp experience may have resulted in their not being notably different from the control group 33 years later in many of the areas we examined.

For the purpose of this study, we have relied on measures of atti-

tudes and behavior that we assumed might be related to some of the psychological and social problems described in the literature on Holocaust survivors. These measures are at best indirect. Therefore, it could be argued that they do not measure adequately those symptoms identified by others. It could also be argued that because the symptoms referred to in the psychiatric literature occur relatively infrequently in a general population, a more appropriate comparison might be made between clinical groups of survivors and non-survivors.

In our questionnaire, we did include a recognized measure of psychological distress, Langner's 22-item scale. Langner scores are computed from an inventory of physical and mental symptoms associated with stress and have been used in community studies. Initial examination of these scores reveals that survivors report significantly greater frequency of these symptoms (Eaton et al.) What is of interest here is that this greater level of reported stress and illness do not seem to have large effects on the attitudinal/behavioral variables in this study. Perhaps greater differences would have been found had we studied quality-of-life measures such as the warmth of the marital relationship, job satisfaction, etc. In any event, our findings raise the possibility that these stress levels (and other clinical symptoms) have minimal consequences for collective social action. It is possible, for example, that a compartmentalization is at work, preventing stress or neuroses from distorting macro-social perceptions and behavior and impeding adjustment to the real world.

These questions point to the need for further research on the long-term effects of extreme discrimination on future attitudes and behaviors of victimized minority-group members, in both minority and majority settings. They suggest a degree of caution in assuming that such extensive victimization need have long-lasting or permanent effects on all subsequent attitudes or behaviors. The weight of clinical studies may have over-emphasized these effects because of the lack of controls, which may have led to inadequate consideration of compensatory mechanisms used by the victims.

It should be clear that our findings in no way diminish the immense magnitude of the horrors suffered by these (and all) Jewish survivors, nor do they deny the reality of severe, ongoing mental and physiological consequences. Rather they focus attention on the magnificent ability of human beings to rebuild shattered lives, careers, and families, even as they wrestle with the bitterest of memories.

Such an emphasis may provide a useful corrective to popular, clinically derived impressions of survivors, and now evidently their children (Epstein), prevalent in parts of the therapeutic communities, as suffering from incapacitating or harmful neuroses, or worse. Research might shift to the study of the adaptive or rehabilitative processes at work.

## Notes

1. The data collection for this study was funded by the Jewish Community Research Institute of Montreal. The analysis was funded by grants from the Nathan Steinberg Foundation of Montreal, and from the Quebec Ministry of Education. Leora Swartzman assisted in the bibliographical research and the data analysis. This article is a revision of a paper presented at the annual meeting of the American Sociological Association, 1979

2. The master list was compiled by integrating various lists of Jewish community organizations, and eliminating duplicates. A sample of 1,000 households was selected using a table of random numbers, of which 937 had a correct address, and were Jewish in composition; 657 completed interviews were obtained, for a response rate of 70 percent. Respondents completed a lengthy questionnaire of over 300 questions. All the interviewers were Jewish, to encourage rapport between the interviewer and respondent, given the sensitive nature of some of the questions, these amateur interviewers were trained and supervised by a professional survey company

3. Several comments on the subsample of survivors are in order. First, this study deals with Jewish survivors, and does not attempt to resolve possible similarities or differences with the case of non-Jewish survivors of Nazi persecution, or any other personal victims of extreme oppression; interested readers may well speculate on these questions

Anonymous reviewers have remarked on the heterogeneous nature of the survivor sample, in terms of the actual experiences included in the group. We are well aware of the differences among these types of experiences, and are in fact analyzing differences in long-term consequences within this survivor group

Yet sociological analysis consists, in essence, of comparisons using groups which are both homogeneous (relative to other groups) and heterogeneous: racial, sexual, or occupational groups come to mind. We feel that the variance in life experience in our group of Jewish survivors is no greater than that found in most other groups, and does not impede valuable comparisons with our controls

Moreover, clinical, historical work has suggested that survivors who hid or were hidden, or fought in armies, or with guerrillas, suffered from the daily insecurity and fears of discovery, betrayal, capture, or death in combat, with often equally harmful long-term consequences compared to survivors of labor camps or concentration camps.

One reviewer wondered whether those 41 respondents who did not identify their specific experiences in wartime Europe may have somehow biased the sample, and thus the ensuing results. Clinical findings lead us to believe that these respondents reticent about identifying their Holocaust experiences are likely to have been more severely victimized, possibly concentration camp inmates. This simply magnifies the difference between the survivor group and controls, increasing our expectation of significant between-group differences.

Finally, the setting of this study in Montreal raises the hypothetical possibility of different initial selection and migration patterns of survivors, with Montreal's survivors being in some way unique. Any case study or one-shot survey in social sciences must deal with the issue of generalizability. However, familiarity with the migration process of survivors from postwar Europe suggests that the two major factors at work were the availability of family or friends in a specific location, and simple luck—the destination of the first available ship, a random disembarkation, etc. A systematic, selective migration of survivors to Montreal seems unlikely.

4. Given that the data are rather original, it was felt that a visual, tabular presentation, along with chi squares computed for both all three groups and the immigration-survivor comparison, would be more valuable than a summary table of analysis of variance. An analysis of variance (MCA) duplicated the findings of the regression analysis reported in Table 7.

The regression analysis is confined to respondents who are *foreign born*, as well as over 40 years of age and of European (Ashkenazi) origin. A "dummy" variable differentiated survivors from other respondents.

The inability to find significant differences (to prove the null hypothesis) does not rule out possibilities that such differences may in fact exist. Yet any failure to discover expected relationships is of scientific interest, and such findings are suggestive if not conclusive.

5. Tests for interaction effects were conducted by creating new variables for inclusion as independent variables in the regression analysis. The beta coefficients of all these variables

(the products of a dummy variable for being a survivor with age, sex, etc.) were not statistically significant

6 The Blishen Index is a measure of socioeconomic status derived from average levels of education and income associated with given occupations (in the Canadian census) as well as prestige rankings from survey data. For more details see Blishen.

## **Appendix**

### **Description of Variables**

#### **I. PERCEIVED ANTI-SEMITISM**

Responses to the following two questions,

1. Do you feel there is prejudice against Jews in Quebec?

Yes, a great deal; Yes, some; Yes, a bit; No, none.

2. "Anti-Semitism" is a problem in this city.

Strongly agree; Agree somewhat; Don't know; Disagree somewhat; Strongly disagree.

were combined into an additive ordinal index of perceived anti-Semitism (correlation of .47, alpha reliability coefficient of .64), which was used in subsequent analyses. (The index of perceived anti-Semitism ranges from a low of 2 to a high of 9, with a mean of 5.78 and a standard deviation of 1.90.) Scores between 2-6 were recoded as low, those 7-9 as high.

#### **II. SATISFACTION: (ECONOMIC AND POLITICAL)**

An ordinal index of Satisfaction was formed by combining responses to the following questions.

A. Generally speaking would you say that in Quebec society at present things are going

Very well; Rather well; Not well, Poorly.

B. How would you rate the state of the Quebec economy?

It is. . . . Very good; Rather good; Poor; Very poor.

C. Generally speaking, how do you judge the present provincial government of Quebec? Are you. . . .

Very satisfied; Rather satisfied; Not too satisfied, Not at all satisfied.

D. How do you feel about Bill 101, the French language law adopted last summer by the Quebec government? Are you. . . .

Strongly in favor; In favor; Against; Strongly against.

The average inter-item correlation of these three variables is .327, the reliability coefficient is .71. The values were recoded into a six-ranked ordinal index with a mean of 3.8 and a standard deviation of 1.49. High scores indicate high levels of satisfaction. Ranks 1-3 were recoded as low, those 4-6 as high.

#### **III. INDEX OF SEGREGATION**

Responses to the three questions below were combined to form an ordinal, additive index.

1. Being Jewish affects my choice of a place to live

Strongly disagree; Disagree somewhat; Don't know; Agree somewhat; Disagree somewhat.

2. Among my friends. . . .

3. Among people in my neighborhood. . . .

None are Jewish; Few are Jewish; Some are Jewish; Most are Jewish; All are Jewish.

The inter-item correlation of the three variables was .18, the alpha reliability coefficient is .40. The index ranged in value from a low score of 3 to a high of 15, with high scores indicating high segregation. The mean was 11.09, and the standard deviation was 2.43. Values of 3 to 9 were recoded as low, 10–12 as medium, and 13–15 as high.

While the correlation and reliability coefficients are relatively low, these three variables did factor out together on a common factor with loadings of .43, .36, and .71, respectively, in a varimax rotated factor analysis performed on the data.

#### IV. PROBABILITY OF EMIGRATION FROM QUEBEC

Responses to the question:

. . . . the chances of your moving out of Quebec in the next five years. Are you. . . . Definitely leaving; Probably leaving; Probably not leaving; Definitely not leaving. were sought for the case of an implied "status-quo" assumption and for a scenario in which the referendum strongly supports Quebec independence. Responses were combined to form an ordinal index of the propensity to emigrate. The correlation of the two variables was .61, the alpha reliability coefficient is .76, and the index has a range of a low of 2 and a high of 8, with a mean of 4.60 and a standard deviation of 1.59. The index was recoded with values 2,3 = low, 4,5 = medium, and 6–8 = high.

#### V. RELIGIOSITY

An ordinal, additive index of religiosity was computed from responses to:

1. Apart from wedding or bar-mitzvas, how often do you attend synagogue religious services?

Never; Primarily on the High Holidays; Primarily on the major Holidays; On major Holidays and some Sabbaths; All Sabbaths and Holidays; Daily.

2. It is all right for Jews to marry non-Jews.

Strongly agree; Disagree somewhat; Don't know; Agree somewhat; Strongly disagree.

3. How well do you read Hebrew?

Very well; Fairly well; With some difficulty; With great difficulty; Not at all.

4. The number of the following six rituals observed by the respondent.

Take part in a Passover Seder; Keep Kosher at home; Light Sabbath candles; Fast on Yom Kipper; Refrain from eating bread on Passover; Light Chanukah candles.

The index of religiosity ranges from a low of 4 to a high of 22, with a mean of 14.2 and a standard deviation of 4.4. The inter-item correlation is .42, the alpha reliability coefficient is .74. The index is recoded with values of 4–11 = low, 12–16 = medium, and 17–22 = high.

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# Marital Dissolution, Early Motherhood and Early Marriage\*

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## ABSTRACT

*The age at which a young woman marries appears to be related strongly to the probability that the marriage remains intact: older couples tend to make more stable pairings than those who wed while quite young. But youthful marriages are often accompanied by youthful childbearing. The effects of the age at which the woman first wed and the age at which she bore her first child on the likelihood that the marriage dissolved during this period were assessed, net of each other and of other characteristics and circumstances of the woman. We found that, among young wives, teenage parenthood did not appear to increase the risk of divorce or separation, whereas teenage marriage significantly raised the probability of disruption. When the analysis was performed separately by race, this pattern held among white wives; however, for black wives a first birth before the age of 20 was found to increase instability more than a first marriage before that age. The finding that age at first marriage but not age at first birth is significantly related to the probability of marital dissolution appears robust in the total sample: among subsamples of wives all married at about the same age, the age at which they had their first birth did not influence stability of marriages.*

The likelihood that a first marriage will end in divorce is strongly linked to the age of the partners, with especially young and especially old couples facing higher risks of breakup than those who marry for the first time when they are in their twenties (Glick and Norton). The chances that a first marriage will remain intact also are related directly to the age at which the

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woman bears her first child; whether the woman was married when the first child was conceived does not seem to alter this relationship (McCarthy and Menken).

Unfortunately, all research to date on the connection between age at first marriage, age at first birth, and marital disruption has examined only two of these factors (McCarthy and Menken). Because early marriage and an early first birth so often occur together, it is not clear which tends to decrease the stability of marriages, or if stability is diminished only when the two occur together. In the research reported here, we use information on marital disruptions from 1968 to 1972 among a large, national sample of young wives, to attempt to untangle these effects.

Understanding the causes of marital disruption is important for a number of reasons. Marital dissolution, while often resulting in an improvement in the economic situation of men, frequently catapults women and the children dependent on them into poverty (Hoffman). A third of all families headed by women were at or below the poverty level in 1975, compared to about one in twenty of the families with male heads (Brown). And half of all female-headed families are such as a consequence of divorce or separation (U.S. Bureau of the Census, a). While the economic cost of marital instability is high, both for the individuals involved and for society as a whole, the personal, emotional price paid by parents and children for family disorganization is also great (Weiss). Yet, policymakers, social workers, and social scientists alike lack information needed to predict the risk of marital dissolution. In this paper, we attempt to determine the relative risk of dissolution associated with marriage and parenthood during the teen years. But let us note, first, the prevalence of early marriage and childbearing and their relation to divorce and separation.

Although the median age at marriage in the U.S. for women has risen past 21 (U.S. Bureau of the Census, b), many women marry while in their teens. Among recent cohorts of women, about a third have wed by age 18—37 percent for blacks and 32 percent for whites. A high proportion of these young marriages were accompanied by a premaritally conceived birth. Among female adolescents who married at 14 to 18 years of age, approximately 30 percent of the whites and 60 percent of the blacks bore a child within eight months of marriage (U.S. Bureau of the Census, a). Since some evidence indicates that adolescent pregnancy is typically unintended (Zelnik and Kantner, a, b), a high proportion of all teenage marriages might not have been contracted when they were had there been no conception (see also Furstenberg, a).

Previous research has established a strong link between early marriage and subsequent divorce (Bumpass and Sweet; Glick and Norton; Mott and Moore; Ross and Sawhill; Weed). Several researchers have also explored the association between premarital pregnancy and marital disruption (Bumpass and Sweet; Coombs and Zumeta; Furstenberg, b, c).

However, the relationships between youthful marriage, early childbearing, and marital instability have not been evaluated. Teenage marriage and pregnancy so often occur together that to examine one without considering the other is likely to give an incomplete picture.

The social and economic difficulties posed by the combination of early marriage plus parenthood are unlike those suffered by couples who simply marry young but postpone childbearing and unlike those experienced by an older couple faced with an untimely pregnancy. But despite the pressures experienced by teenage parents there are several factors which may make them less likely than others to dissolve their marriage. The presence of preschool children has been found by Cherlin to decrease the probability of marital disruption. Thornton reports that parents of moderate-sized families are less likely to divorce or separate than either the childless or those with large families. The presence of a child or children may increase the emotional and economic cost of divorce for both partners (Becker et al.). For the woman who typically will retain custody of the child it may reduce the availability of other roles; and it makes the man financially responsible for people he will not live with after the break-up of the marriage. In addition, separations or divorces may be taken more seriously by the couple and by others when there are children involved than when there are not. Also, those who have children soon after marriage simply may be more family oriented than those who delay parenthood, and may, for the same reason, be less willing to divorce.

### **Research Questions**

To understand the relationship between early marriage and early parenthood, it is important to know how these factors, individually and in combination affect marital stability. We suggest the following possibilities. (1) Perhaps it is simply early marriage which leads to divorce. It is possible that marriages between young, unprepared teenagers end more frequently in divorce than those of older couples regardless of the presence or absence of a child. On the other hand, (2) it is possible that teenage marriage, alone, without the extra financial, emotional, and physical drains of parenthood, is highly viable—that it is the particular burden of parenthood which so often characterizes these marriages that makes them fragile. Exploration of which of these factors can be said to make teenage marriages unstable is the central focus of the current paper.

## Description of the Study

### ANALYTIC STRATEGY

In the research reported here, we estimate the effect of age at marriage and age at first birth on the likelihood of marital dissolution (either separation or divorce) during the period 1968 to 1972.<sup>1</sup> We chose this approach rather than an examination of current marital status in a particular year or at a particular age for several reasons. First, as Hannan et al. point out, use of current marital status to analyze marital instability entails the assumption that divorce is a state which individuals occupy rather than an event which may or may not occur to them. Thus, those who divorce and remarry immediately are much less likely than those who remain unmarried to experience marital dissolution if current marital status is used as the measure. In a cross-sectional analysis the likelihood of divorce is confounded with the speed of remarriage. An examination of the occurrence of marital disruption over time does not have this problem. An additional advantage of the longitudinal over the cross-sectional approach for our purposes arises from the data used in this analysis. The panel study which provides the information used here contains little data on divorces which took place before the survey began in 1968. In addition, many of the important control variables used in this study (for example, characteristics of the husband), are available only for the years 1968 to 1972. We therefore selected women who were currently married in 1968 and examined the factors which influenced their possible divorce or separation by 1972.

### DATA

The data analyzed in this paper are drawn from the National Longitudinal Study of the Labor Market Experiences of Young Women (hereafter referred to as the NLS data). The NLS was funded by the U.S. Department of Labor and has been designed and fielded by the Ohio State University and the U.S. Bureau of the Census. The first wave of the NLS in 1968 sampled over 5,000 young women between the ages of 14 and 24. Attempts to reinterview these women were made annually from 1969 through 1978. Sample retention was very good: by 1972, the last year considered here, 90 percent of the original sample remained intact.

Since sample attrition may have reduced the representativeness of the sample, some caution in generalizing to the population of all young women in this cohort is in order. Yet the sample was selected to be

representative of the U.S. 14-to-24-year-old female population in 1968, and these data are believed to be among the best available for present purposes. Several advantages stand out. First, the data are longitudinal and cover a four-year period which allows examination of the occurrence of marital dissolution unconfounded with its duration (as discussed above). These data cover women whose marital and childbearing experiences are relatively recent, and the large sample size provides enough cases of marital dissolution between 1968 and 1972 to allow detailed analysis. Information in the NLS on economic factors, education and labor force experience and attitudes is excellent. Information on marital and childbearing events, while not as detailed as would be ideal, is adequate. This combination of factors makes these data the best available for our purposes.

#### DEPENDENT AND INDEPENDENT VARIABLES

##### *Marital Dissolution*

The dependent variable in this analysis is whether a marriage intact in 1968 dissolved, either through divorce or separation, by 1972. The measure of marital disruption is coded 1 if the respondent dissolved her 1968 marriage at any time before 1972. If this marriage remained intact, the variable is coded 0. Only women who were currently married in 1968 are included in the sample used in this analysis. Since the NLS young women were 14 to 24 years old in 1968, the oldest age at first marriage included here is 24.<sup>2</sup>

The necessary restriction of this analysis to young women first married by age 24 means that we are comparing the stability of teenage marriages with those entered into during the early to mid twenties. Since half of all women in the U.S. wed by about age 21, and 84 percent of the NLS young women who were 24 in 1968 had ever married, we are excluding from our analysis the minority of all first marriages contracted by young women over 24. The effects on the results should be minimal.<sup>3</sup>

##### *Age at First Birth*

The NLS data do not contain a childbearing history for the respondent. However, a record of all persons living in the respondent's household in each survey year and their relationship to the respondent is available. From this information the young woman's age at first birth (AFB) was estimated.<sup>4</sup> The measure of age at first birth used here does not include children who were given up for adoption shortly after birth, who were stillborn, who died in early childhood, or who were sent to live outside the respondent's household. Own children of the respondent cannot be distinguished from adopted children. We are, in effect, measuring the impact of the age at which a young woman takes on the duties and responsibilities of motherhood—the age at which she becomes a parent in a social sense.

The variable used here should be a fairly unbiased measure of social, if not biological, motherhood.

The measure of age at first birth is classified into a series of dummy variables scored 1 if the young woman had a first birth during those ages, 0 if her first birth occurred either earlier or later. All first births occurring by 1972 are included in this measure (see Table 1).

### *Age at Marriage*

Age at marriage, available directly from the NLS data, was grouped into a series of dummy variables similar to those for age at first birth.

### CONTROL VARIABLES

Researchers have suggested a variety of other factors that might affect or that have been documented to affect the probability of divorce for which we try to control in this analysis. For example, blacks have been found to have a higher likelihood of marital disruption than whites (Norton and Glick; Ross and Sawhill). Low education, occupation, and income have all been found associated with a greater probability of disruption; although, when these variables were considered together, income was found to be of far more importance than education or occupation (Cutright). Ross and Sawhill report no straightforward effect of income, however. Lower husband's earnings relative to expected earnings and the experience of unemployment by the husband were found to be related to marital disruption. Higher earnings by wives also were found to increase the likelihood of dissolution, perhaps because wives with earnings can afford to divorce and support themselves—a factor that may affect both the wife's and the husband's propensity to divorce (see also Cherlin, a, b). A high salary also may increase marital conflict because of the threat it poses to a husband's self-concept (Komarovsky).

Marital dissolution also has been reported to occur less frequently when a couple owns a home (Levinger), and, generally, when they possess material assets (Cherlin, a; Ross and Sawhill). The probability of disruption has been found to be higher in the central city (Ross and Sawhill) and on the West Coast than elsewhere (U.S. Department of Health, Education and Welfare); presumably this reflects differing levels of acceptability of divorce. Marriages between individuals of different races (Norton and Glick), of disparate ages and of differing religions (Bumpass and Sweet), and having a family background of marital disruption (Pope and Mueller) tend to be less stable than others as well. Stability also tends to increase with marital duration (Cherlin, a; Ross and Sawhill).<sup>5</sup>

The control variables included in this analysis are also listed and defined in Table 1, along with their means and standard deviations. Unless otherwise indicated, all variables are measured as of 1968.

**Table 1. MEANS AND STANDARD DEVIATIONS OF VARIABLES BY AGE AT FIRST MARRIAGE**

Age at First Birth (AFB)	Age of Women at First Marriage												
	≤ 17			18			19-20			21+			Total
	Mean	S.D.		Mean	S.D.		Mean	S.D.		Mean	S.D.		
< 16													
16-17	.157	.365		.001	.033		.005	.071		.011	.106		.205
18	.505	.501		.096	.295		.010	.097		.020	.139		.363
19-20	.138	.345		.355	.479		.052	.222		.019	.136		.341
No birth at 21	.115	.319		.369	.483		.451	.498		.074	.262		.445
21-23	.085	.279		.179	.384		.482	.500		a			a
No birth at 24	. . .	. . .		. . .	. . .		. . .	. . .		.393	.489		.398
Parental socioeconomic status	9.17	2.01		9.95	2.27		10.45	2.18		10.64	2.37		.398
Education 1968	10.179	1.638		11.53	1.39		12.22	1.68		12.99	2.25		.398
Intact family	.727	.446		.844	.363		.886	.319		.867	.340		.374
Metro residence	.531	.500		.548	.499		.619	.486		.717	.451		.490
Assets													
< \$1000	.411	.493		.354	.479		.302	.460		.210	.408		.468
= \$1000	.284	.451		.305	.461		.319	.467		.336	.473		.463
> \$1000	.305	.461		.341	.475		.379	.486		.455	.499		.482
No children	.379	a		.407	a		.501	a		.655	a		a
# of children = 1	.495	.501		.455	.499		.390	.488		.283	.451		.492
# of children > 1	.126	.332		.138	.345		.109	.312		.062	.242		.313



= 0	.021	a	.018	a	.017	a	.082	a	.091	a
≤ 3000	.149	.357	.119	.324	.125	.331	.142	.350	.133	.340
3001-6000	.368	.483	.344	.476	.294	.456	.277	.448	.321	.467
6001-9000	.307	.462	.334	.473	.356	.479	.274	.447	.322	.467
9001-12000	.111	.314	.135	.342	.170	.376	.175	.380	.148	.355
> 12000	.044	.205	.050	.219	.038	.190	.050	.219	.045	.207
Respondent Contributes:										
0% to family income	.411	.493	.390	.489	.331	.471	.152	.360	.329	.470
1-25% to family income	.389	.488	.356	.480	.298	.458	.307	.462	.336	.473
25-50% to family income	.158	.366	.181	.386	.264	.442	.361	.481	.238	.426
50-70% to family income	.019	.136	.055	.229	.055	.228	.135	.342	.062	.241
> 70% to family income	.023	.150	.018	.133	.052	.222	.046	.209	.036	.185
Race (1 = white)	.870	.337	.909	.288	.918	.275	.906	.293	.901	.298
West Coast residence in 1968	.137	.344	.165	.372	.168	.374	.153	.361	.157	.363
Respondent working (1 = yes)	.557	.497	.607	.489	.680	.467	.805	.397	.658	.457
Number of years married since 1968	3.94	2.36	3.12	1.92	2.38	1.61	1.40	1.08	2.76	2.02
Age at First Marriage										
10-15	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	.045	.206
16-17	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	.215	.411
18	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	.228	.419
19-20	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	.330	.471
21-23	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	.176	.381
Not married at 24	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	.007	.081

a = omitted category.

#### METHODS

The sample on which this analysis was done includes 1,447 respondents who were married at the time of the first interview in 1968. By 1972, 191, or 13 percent of these women had divorced or separated. To estimate the effect of age at first marriage and age at first birth, net of each other and of other theoretically important factors, equations containing all these variables were estimated. Multicollinearity between age at first marriage and age at first birth casts doubts on the findings when both are included in a single equation, but, to obtain their separate effects, it is essential to control for one while estimating the impact of the other on marital dissolution. For this reason, and because several of our hypotheses deal with interactions between these two variables, models of marital instability were then estimated separately for various ages of first marriage. This allowed a test of the hypothesis that, given an early first marriage, youthful childbearing increases the likelihood of dissolution. In addition, this strategy overcomes the confounding effect of multicollinearity between these two important variables.

The dependent variable in this analysis is a dichotomy—whether a woman married in 1968 remains so, or is separated or divorced by 1972. The estimate of the dependent variable produced by the model may be interpreted as the probability that an individual woman disrupted her marriage. In this situation, probit analysis is appropriate. The probit model assumes that the underlying probability function is normal. Probit is especially appropriate for predicting a dichotomous dependent variable because it constrains the predicted probabilities to a range of 0 to 1 and allows the independent variable to have different effects at various points on the curve. Ordinary least-squares regression (OLS) requires a uniform slope throughout the range of the independent variable. This feature of OLS could have the effect of penalizing a continuous relative to a dichotomous variable (Vanneman and Pampel). Probit analysis does not have this drawback and is therefore a better choice than OLS for estimating the likelihood of marital disruption. A maximum-likelihood solution is developed through an iterative technique. The results yielded by probit analysis may be interpreted like those from multiple regression.

#### Results

To begin to examine the impact of age at first marriage and age at first birth on marital dissolution, net of each other, four equations were estimated. First, a model of marital disruption between 1968 and 1972 was estimated (Model 1) which contained all the independent variables mentioned earlier except age at first marriage and age at first birth. Then age at first marriage

was added to the equation (Model 2) and the increment in variance explained was examined. Next, age at first birth was added to the equation without age at first marriage (Model 3) and, finally, both of these measures were included along with the other variables in a single equation (Model 4).

As Table 2 shows, addition of either age at first marriage or age at first birth alone increases the explanatory power of the model by a very small amount; in neither case is the improvement statistically significant. None of the categories of age at first birth has a statistically significant impact on the likelihood of divorce or separation even when age at first marriage is omitted from the equation. When age at first marriage is included in the equation (Model 4), the probability of marital instability is significantly *lower* for young women who become mothers between 16 and 18 compared to those who waited until at least 21. A relatively young age at first marriage does appear to increase the probability of marital instability, however, even when age at first birth is controlled. Those women who first wed at 18 or less experienced a likelihood of divorce or separation 8 to 12 percentage points higher than that for those who delayed marriage until they were at least 21 (Model 4). Given that 13 percent of the women who were married in 1968 had divorced or separated by 1972, the increase in probability of disruption associated with a marriage during the teen years is quite large.

The results presented in Table 2 suggest that teenage marriages are less stable than those contracted later and that this relationship holds regardless of youthful parenthood. A first birth at a young age does not appear to increase marital instability even if age at marriage is not controlled. Results for other variables generally correspond to other studies; our brief summary will highlight the differences.

Like other researchers we find that the probability of dissolution is considerably higher for couples whose marriage is of relatively short duration (Cherlin a; Ross and Sawhill), for blacks (Ross and Sawhill), and for women from non-intact families (Bumpass and Sweet; Mueller and Pope; Pope and Mueller). Disruption was also found to be more frequent among Pacific Coast couples, though no impact of residence in an SMSA was evident in our results.

Having assets of at least \$1,000 appears to have moderate though nonsignificant association with marital stability. The husband's income is only slightly and nonsignificantly associated with marital stability; however, the probability of divorce or separation increases substantially with the proportion of family income earned by the wife.<sup>6</sup> Wives from families of relatively high socioeconomic status also have less stable marriages, perhaps because they have relatively high expectations or greater resources that permit them to leave a bad marriage.

Unlike Cherlin (a) and Bumpass and Sweet, we find that education

**Table 2.** THE EFFECT OF AGE AT FIRST MARRIAGE, AGE AT FIRST BIRTH AND CONTROL VARIABLES ON MARITAL DISSOLUTION BETWEEN 1968 AND 1972 (UNSTANDARDIZED PROBIT SLOPE COEFFICIENT EVALUATED AT  $P = 10.0\%$ )

	Model 1 W/O AFB, AFM	Model 2 With AFM Only	Model 3 With AFB Only	Model 4 With AFB, AFM
<u>AFB</u>				
< 16				
16-17				
18				
19-20			.03689	-.04030
≥ 21			.01019	-.05505**
			.00041	-.04549*
			.01051	-.01258
			-0-	-0-
<u>AFM</u>				
≤ 15				
16-17		.09400**		
18		.07350**		.12350**
19-20		.05690**		.10499**
≥ 21	-0-	.03116		.07684**
		-0-		.03640
Parent SES	.01132**		.01152**	-0-
<u>Education</u>		.01085**		.01141**
< 12				
= 12	-0-	-0-	-0-	-0-
13-15	-.06644**	-.04824**	-.06218**	-.05378**
≥ 16	-.10612**	-.07634**	-.10168**	-.08401**
	-.22725**	-.18247**	-.22146**	-.18846**
Intact	-.03716*	-.03315*	-.03498**	.03223*
Metro	.02184	.02483	.02255	.02235
<u>Assets</u>				
≤ 0	-0-	-0-	-0-	-0-
\$1-\$100	.01812	.01850	.01717	.01860
> \$1000	-.02773	-.02769	-.02811	-.02792

- 1	.01723	.01504	.02067	.01925
> 1	-.00631	-.01236	.00145	-.01921
0	-0-	-0-	-0-	-0-
<u>Income</u>				
\$1-\$3000	-.02764	-.02646	-.02900	-.02907
\$3001-\$6000	.00596	.00685	.00450	.00578
\$6001-\$9000	.00524	.01429	.00520	.01397
\$9001-\$12000	-.05094	-.03950	-.05106	-.04020
>\$12000	-.07072	-.06186	-.07143	-.06247
<u>Rincome</u>				
0				
1-25%	.03592**	.03493**	.03729**	.03626**
26-50%	.04444**	.05363**	.04614**	.05427**
51-70%	.06574*	.07455*	.06879*	.07444**
>70%	.09853*	.10760**	.09989*	.10874**
<u>Race</u>				
	-.10365**	-.10503**	-.10324**	-.11012**
<u>West</u>				
	.06395**	.06217**	.06412**	.06260**
<u>Duration 68</u>				
< 2				
2-3	.01226	.04144**	.00678	.05812**
4-5	.02132	.04495**	.01663	.05540**
> 5	-.01473	-.00063	-.01734	.00716
	-0-	-0-	-0-	-0-
<u>Variance explained</u>				
	.19601	.20565	.19722	.20941

N = 1,277.

0 = omitted dummy in a set of dummy variables.

\*.05 < p < .10.

\*\*p < .05.

has a strong, positive association with marital stability. Given the relative youth and recency of the current sample, this difference may arise from changes in the determinants of marital dissolution for brides in the 1960s; alternatively, education may affect marital stability more early in the marriage than later on. Finally, unlike Cherlin (a), we do not find that the presence of very young children acts as a deterrent to the break-up of a marriage, a finding we will pursue later.

One of the major predictors of marital disruption in our analysis is race. In the analyses presented to this point race has been included as an additive variable; however, to explore the possibility that the process which determines marital stability may be very different for black than for white women, the model of marital disruption was estimated separately for blacks and whites. Since we are interested primarily in racial differences in the impact of age at marriage and age at first birth on divorce or separation, only the coefficients from these variables are presented in Table 3. The equations from which they were estimated include all variables in the basic model presented in Table 2.

Early childbearing does not seem to increase the probability of marital break-up among whites, quite the opposite. Net of other influences, an early birth seems to reduce the chances of disruption. Among blacks, though, there is a strong association between teenage childbearing and marriage break-up which remains statistically significant when age at marriage is included in the regression. The reason for these different patterns may lie in the greater tendency of whites to enter "shotgun" marriages (U.S. Department of Health, Education and Welfare), unions that might not have occurred except for the press of pregnancy. While blacks less often hasten marriage solely to legitimate a pregnancy, they often enter marriage with an out-of-wedlock child, a circumstance associated with marital disruption (U.S. Bureau of the Census, b).

To summarize briefly, we have found a substantial negative impact of teenage marriage on marital stability, particularly among whites, whether age at first birth is controlled or not. Net of age at first marriage, teenage childbearing does not appear to increase the likelihood of divorce or separation among whites, though it appears to have some effect among blacks. However, the multicollinearity between these two variables (mentioned earlier) makes these results subject to question. For this reason, and because we hypothesized that the effect of some factors would depend on age at first marriage, we divided the sample on this variable and estimated the equations separately within each group. Since within groups age at first marriage is constant, we can examine the impact of age at first birth without encountering multicollinearity. The results shown in Table 4 confirm our earlier finding.

Given age at first marriage, early childbearing is associated with a higher probability of divorce or separation in only one instance. Among

**Table 3.** THE EFFECT OF AGE AT FIRST MARRIAGE AND AGE AT FIRST BIRTH, NET OF CONTROL VARIABLES ON MARITAL DISSOLUTION BETWEEN 1963 AND 1972 (UNSTANDARDIZED PROBIT SLOPE COEFFICIENTS, BY RACE, EVALUATED AT  $P = 10.0\%$ )<sup>1</sup>

	AFM Only	AFB Only	AFM + AFB
<u>Whites</u>			
<u>AFB</u>			
≤ 15		-.06122	-.15057**
16-17		-.05651**	-.15277**
18		-.02251	-.08963**
19-20		-.01639	-.05305**
≥ 21		. . . .	. . . .
<u>AFM</u>			
≤ 15	.06277		.15785**
16-17	.03633**		.17241**
18	.08453**		.12736**
19-20	.05983*		.08056**
≥ 21	. . . .		. . . .
<u>Blacks</u>			
<u>AFB</u>			
< 15		.19846**	.15556**
16-17		.12014**	.11505**
18		.03914	.04324
19-20		.05592	.07410
≥ 21		. . . .	. . . .
<u>AFM</u>			
≤ 15	.15841**		.06446
16-17	.05217		-.01777
18	-.00695		-.05323
19-20	-.03897		-.06469
≥ 21	. . . .		. . . .

<sup>1</sup>These coefficients are net of the effects of the control variables listed in Table 2.

\*.05 <  $p$  < .10.

\*\* $p$  < .05.

those who wed at 18 or younger, the pattern of coefficients suggests that the earlier the age at first birth the lower the likelihood of disruption compared with those who first became parents at 21 or older.<sup>7</sup> These results do not lend support to an hypothesis that early childbearing is the reason for the relationship between youthful marriage and instability; those who wed while teenagers and delay parenthood fare no better than their classmates who also had their first child while in their teens.

**Table 4.** THE EFFECT OF AGE AT FIRST BIRTH AND CONTROL VARIABLES WITHIN CATEGORIES OF AGE AT FIRST MARRIAGE ON MARITAL DISSOLUTION BETWEEN 1968 AND 1972 (UNSTANDARDIZED PROBIT SLOPE COEFFICIENTS EVALUATED AT  $P = 10.0\%$ )

	AMLT18	AMEQ18	AM1920	AM21+	ALL
<b>AFB</b>					
< 16	-.07476**	-.66522	.25934*	-.36624**	.03203
16-17	-.08334**	-.04110	.00464	-.99082	.02187
18	-.06550	-.08316	.00456	.03179	.00922
19-20	-.00715	-.07508	.04597	-.03322	.01364
≥ 21	. . . . .	. . . . .	. . . . .	. . . . .	. . . . .
Parent	.07185	-.00187	.01910	.01820	.01249
Education	-.01555*	-.00473	-.02258	-.04955	-.02198*
Intact	.01253	-.01992	-.03577	-.07313	-.03680
Metro	.02353	.04267	.01233	.04814	.02133
Nuptial 68	.03521**	.01262	-.08381**	.10389	-.01025
Race	-.12737**	-.04551	-.11051	-.27157	-.10164
West	.02391	.08908	.08087	.10929	.06270
Duration 68	-.02762**	-.02210*	.01633	.03317	-.00374
<b>Assets</b>					
1 = Missing	-.05357	-.00761	-.02766	-.08014	-.02747
2 = 0	. . . . .	. . . . .	. . . . .	. . . . .	. . . . .
3 = 1-1000	.01226	-.02497	.00432	.05800	.00878
4 = > 1000	-.05073	.00299	-.07857	-.08362	-.03981
<b>Kids</b>					
1 = 1	.00770	.04241	.06829	-.23547	.02326
2 > 1	-.03314	.02456	.03033	-.40081	.00135
3 = 0	. . . . .	. . . . .	. . . . .	. . . . .	. . . . .
<b>Mincome</b>					
1 = 0	-.05689	.10918	.07387	-.52012	.02980
2 = 1-3000	. . . . .	. . . . .	. . . . .	. . . . .	. . . . .
3 = 3001-6000	.06539	.02940	.01871	-.06711	.03339
4 = 6001-9000	.05260	.01033	.09978*	-.08685	.03866
5 = 9001-12000	.07913	-.12417	.01031	-.02254	-.01500
6 = > 12000	.14044	-.50998	-.43712	-.61982	-.03192
<b>Rincome</b>					
1 = 0%	. . . . .	. . . . .	. . . . .	. . . . .	. . . . .
2 = 1-25%	.04475	.04448	.04823	-.04178	.03686
3 = 26-50%	.04906	.09504	.03747	.00413	.04954
4 = 51-70%	.03052	.05890	.18649*	-.85192	.06151
5 = > 70%	.10680	.25915	.01222	.09126	.09745
Variance explained	.22872	.40675	.37323	.80846	.18239

\*.05 <  $p$  < .10

\*\* $p$  < .05.

Table 4 does suggest that the impact of a number of independent variables on marital stability differs among groups who first married at different ages. Presence of children under three years of age has no influence on marital stability among brides 18 or younger and significantly increases the likelihood of dissolution for those 19-20 at first marriage. However, the presence of a young child has a significant inhibiting effect among those who delayed first marriage until at least 21. In this subsample which is most similar to that of Cherlin (a), our results are quite comparable. Perhaps the young child of an older couple is more typically a wanted child and represents less of a strain to a marriage.



The stabilizing effect of the wife's educational attainment rises with her age at first marriage from 1.6 percentage points for each additional year of schooling among the youngest brides to 5 percentage points among the oldest. The racial differential in the probability of dissolution also rises dramatically as age at first marriage increases, from 13 percentage points at the youngest ages to 27 percentage points at the oldest ages. As noted above, black teenage brides may be a more select group than their white counterparts since social pressures to legitimate a nonmarital pregnancy appear to be lower in the black than in the white community.

The stabilizing effect on marriages of their duration appears to exist only for those who wed before age 19. Their chances of disruption decrease by 2 to 3 percentage points for each year that the marriage has been intact. Living on the West Coast boosts the likelihood of divorce or separation substantially for all but the youngest brides. The only other factor which appears to affect marital stability differently depending on age at first marriage is whether the woman was raised in an intact family. Intergenerational transmission of marital instability appears to occur only among those who marry at around the average age and not among those who wed while teenagers.

These results, taken as a whole, suggest that many factors which buffer the marriages of those who wed at or near the typical age do not protect those who marry while very young. Having young children, possessing substantial assets, coming from an intact family, having completed a relatively large amount of formal schooling, being white, or having a husband with a high income increase marital stability among those who marry in their late teens or early twenties but do not protect younger brides to the same extent.

In general, it might be noted that our model of marital disruption does not work as well for couples who married when the wife was a teenager as for older brides. Overall, doing separate analyses among groups similar in their age of marriage substantially increases the proportion of variance explained—from 18 percent among the total sample to between 23 and 81 percent within the several subgroups. However, the model, built (by necessity) only with measures of social, demographic, and economic status, does a considerably better job of explaining marital disruption among couples marrying in their twenties than it does among teen couples. Perhaps those who marry young disproportionately possess attitudes or personal characteristics that dispose them to divorce. Or maybe they are not able to develop the coping skills that their unmarried peers have the leisure to work out. On the other hand, since a great deal of maturation follows rather than precedes the wedding, it may be that young couples simply face a greater likelihood than older couples of growing apart. Understanding the processes that underlie disruption presents an interesting research challenge, but one we cannot attack with the data at hand.

### **Summary and Discussion**

The often-observed relationship between age at marriage and marital instability has been attributed by some to the early childbearing which often accompanies teenage marriages (e.g., Winch et al.). In this paper, the following possibilities are suggested and evaluated. (1) Teenage marriages are, in and of themselves, less stable than the marriages of older women, regardless of whether they are accompanied by teenage parenthood. (2) Teenage childbearing, rather than teenage marriage, is responsible for the lower stability of these unions. When not accompanied by youthful parenthood the marriages of teenage brides are no more likely to break up than others.

We have found that first marriage at ages younger than 19 significantly increases the likelihood of divorce or separation for young women, particularly white women, even controlling for the effect of age at first birth and for various social, economic, and demographic factors. Age at first birth was not found to significantly increase the likelihood of marital dissolution among whites, whether age at first marriage was included in the equation or not. Among blacks, an early birth was found associated with greater instability.

When the effect of all independent variables, including age at first birth, on marital dissolution was estimated separately among groups of women similar in their age at first marriage, the same overall pattern was found: teenage brides who delayed parenthood did not have a significantly lower likelihood of divorce or separation than those who became teenage parents. In addition, the impact of various independent variables on the stability of marriage appears to depend on the age of the young woman when she first married. The probability of remaining married between 1968 and 1972 was found to increase with possession of substantial assets, the wife's educational attainment, presence of young children, coming from an intact family, being white, and greater husband's income—but only for women who waited at least until they were about 20 to marry. None of these factors improved the prospects for marital stability among teenage brides as much as for those who first wed when older.

These results agree with those of Furstenberg (b) who found in a sample of premaritally pregnant adolescents and their classmates that rapid family building did not appear to increase marital instability. Furstenberg concluded that economic factors were most central in explaining the link between adolescent premarital pregnancy and marriage break-up. We find that, even when important economic variables are controlled, those who married in their teens were substantially more likely than others to experience divorce or separation.

Marriages of very young brides may be more likely to dissolve than others for a number of reasons. These weddings often may occur without

adequate emotional preparation of the partners. The teen years in our society are a time of great change in plans, goals, and tastes. The partner who seems to fit perfectly in one year is often all wrong the next. The very young might also be more likely than those who are older to have romantic and unreasonable expectations of marriage which produce disappointment and disillusionment when these are not met. Marriages among teenagers more often than among others might occur for reasons which do not promote commitment to the union—for example, desire to escape from an unpleasant home life, as proof of adulthood, or for independence. The personality factors which lead certain individuals to wed while teenagers also might increase the probability that they will not remain married. Whatever the underlying reasons for the association, they are probably numerous and varied. However, the current tendency to delay marriage is a hopeful trend, one which may eventually lower the frequency of marital disruption.

## Notes

1 Separation is being included with divorce in these analyses for several reasons. First, we wish to study marital instability, and separation is certainly a measure of instability. Second, separation rather than divorce is a course more frequently followed by black women than white women (Norton and Glick), often being described as equivalent to divorce; therefore, we would underestimate the incidence of marital disruption among blacks if separations were ignored.

2 Age at marriage and marital duration as of 1968 are somewhat confounded, since those who first wed at 24 must have been married less than a year by 1968. Similarly, those first married at 23 can only have marital duration of one or two years. This relationship is dictated by the structure of the data set. Problems arising from this connection between age at marriage and marital duration in 1968 should be slight because of the categorization of each of these variables. All first marriages contracted at age 21 or older are grouped together. Thus, those included in the highest category of age at first marriage may have marital durations in 1968 of up to 3 years. The zero-order correlations between age at first marriage and marital duration range from 0.002 to 0.33 with only 4 out of 24 over 0.2.

3 Those women who had divorced or separated by 1968 and had not remarried are excluded from our sample. Unfortunately, among currently married women, there is no way to know whether the husband present in 1968 is the first. The older the woman in 1968, the greater the likelihood that her current marriage is not her first. Concomitantly, the younger her age at first marriage, the higher the probability that she is in a second or later marriage. However, the median age at first marriage for this cohort is about 21, the median time from marriage to divorce for those divorcing is six and one-half years (U.S. Bureau of the Census) and the median time to remarriage is about two and one-half years for women under 35 (Suchindran and Koo). Therefore, the majority of the currently married women 14 to 24 in 1968 should be in their first marriage. The number of women in second marriages is likely to be so small that our results will be unaffected.

4. The household record in 1968 was searched for any sons or daughters of the respondent. The age of the oldest of the respondent's children was subtracted from the respondent's age in 1968 to yield age at first birth. First births which occurred in subsequent survey years were identified by searching the household records of childless respondents; when a first birth was identified, the respondent's age at the last interview was assigned as AFB. Since exact birth dates are not known for either the respondent or her children, age at first birth contains some error.

5. Other variables which seem theoretically plausible influences on the probability of divorce are more difficult to measure and/or are not measured in the current survey. For example,

sexual satisfaction, esteem for the partner, religion, religiosity, attitudes toward divorce, perceptions of alternatives to the current marriage, attitude similarity between spouses, and competence in marital roles seem likely to affect marital satisfaction, but it has only been possible to measure a few of these concepts. A measure of the proportion of total family income earned by the wife captures in part the male's performance in the traditional role of breadwinner, it also reflects, of course, the independence effect provided the wife with a relatively high income. Although religious preference may influence marital dissolution, this information is not available.

6. A series of dummies was developed to measure the husband's approval of the employment of his wife (qualified by his wife's employment status). This variable captures a discrepancy between role ideology and performance that has been hypothesized to lead to instability (Ross and Sawhill). However, the categories were recombined since only employment status was found to be associated with divorce; husband's attitude lent no additional explanatory power. Women who earned any proportion of the family income obviously were employed.

7. Among those who married at 18 or younger, so few were still childless at 24 that it was not possible to include a childless category. Thus, the highest age-at-first-birth category, 21 or over, includes some parents and some childless couples.

#### Appendix. DEFINITION OF VARIABLES

AFB - 16	Age at first birth less than 16
AFB 16-17	Age at first birth 16 to 17
AFB 18	Age at first birth 18
AFB 19-20	Age at first birth 19 to 20
AFB - 21	Age at first birth 21 or older
AM 10-15	Age at first marriage 10 to 15
AM 16-17	Age at marriage 16 to 17
AM 18	Age at marriage 18
AM 19-20	Age at marriage 19-20
AM 21-24	Age at marriage 21-24
<u>Assets</u>	Total net family assets. Market value of house, real estate, stocks, bonds, etc. less debts and liabilities on same.
\$0	
\$0-1,000	
>\$1,000	
Education in 1968	Number of years of schooling respondent had completed by 1968
<u>Husband's Income</u>	Total income of husband from all sources. dummy variables (1 = yes, 0 = no).
No income	
<\$3,000	
\$3,000-6,000	
\$6,000-9,000	
\$9,000-12,000	
>\$12,000	
Intact family	Structure of family of origin (1 = intact, 0 = not intact).
<u>Number of Children Under 3 Years of Age</u>	Number of own children under the age of 3 living with respondent. dummy variables (1 = yes, 0 = no).
No children	
1 child	
> 2 children	
West	Region of residence. (1 = Pacific Coast, 0 = other).
Parental socioeconomic status (SES)	An index composed of three variables: occupation of head of household when respondent was fourteen, mother's education, father's education--standardized deviation of 3. $\alpha = .774$ .

## Appendix. Continued

Proportion of Family Income Earned by Respondent	The proportion of family income earned by the wife: dummy variables (1 = yes, 0 = no).
0	
1-25%	
26-50%	
51-70%	
>70%	
Race	Race of respondent (1 = white; 0 = black). (The small number of non-white/non-blacks is excluded from the analysis.)
Marital duration	Number of years the respondent has been married by 1968.

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# Temporal Shifts in the Determinants of Young and Elderly Migration in Nonmetropolitan Areas\*

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## ABSTRACT

*Analysis of the determinants of net migration rates, comparing persons under age 65 with those over age 65, for nonmetropolitan areas over three time periods (1950-60, 1960-70, 1970-75) supports the following conclusions: (1) economic factors are more important determinants of migration for the young than for the elderly; (2) the reverse is the case for noneconomic factors; (3) over time, the influence of economic determinants diminishes for the young and the elderly; (4) noneconomic determinants are gaining in importance for both populations; and (5) temporal change in the relative importance of both economic and noneconomic factors is most evident in the younger population. These conclusions support the ecological proposition that migration is an adaptive response to organization change. Findings also imply that we must look beyond the community context to broader societal change in order to explain changing migration patterns in nonmetropolitan areas.*

From the human ecological perspective, migration is viewed as an adaptive response to organizational change (Frisbie and Poston, a, b; Sly and Tayman). Recent evidence supports an additional proposition: transformation of societal organization alters the relative importance of specific determinants of migration (Beale and Fuguitt; McCarthy and Morrison). Areas which were once considered depressed and backward are now experiencing dramatic population growth while their counterparts are undergoing decline. Of particular note has been the reversal from substantial

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outmigration from nonmetropolitan areas throughout most of this century to net immigration since the beginning of this decade. The sharp break in longstanding trends provides an opportunity to demonstrate the proposition that the determinants of net migration in nonmetropolitan areas are shifting in importance as social transformation occurs. Thus, research reported here will examine the degree to which sets of independent variables can account for net migration in nonmetropolitan areas, and will focus on temporal change in the relative importance of these variables.

Our strategy differs somewhat from the earlier ecological analysis of migration. One approach has been to select variables indicative of environment, technology, and organization, and to explain migration patterns with these variables, relying on correlations to test causal ordering among the factors affecting migration (Sly; Sly and Tayman). Based on Duncan's assertion that causal ordering must be determined by theory, not by the relative magnitude of zero-order and partial correlations, we agree with Frisbie and Poston (b) that the temptation to specify causal ordering among these variables should be resisted. Moreover, since only selected aspects of environment, organization, technology, and population have been measured, general statements regarding ordering among these variables are premature. A more fundamental departure of this work, however, lies in its conception of organization. Organization exists in all social units ranging in size from small groups to nations or even world systems. Yet previous analysis often focuses on one time period, and limits the definition of organization to aspects of small areal units. We contend that organization at the societal level has an impact on the magnitude of association between migration and organizational characteristics of smaller units of analysis. Comparison of patterns of association across smaller units for various time periods provides a method for detecting ways in which changes in organization at the societal level affect migration between these smaller units, even though societal level characteristics are not directly measured.

The model of regional growth devised by Perloff and Wingo provides a useful starting point for this analysis. They argue that there are different stages of societal development in which different natural resources are important for regional growth. They describe the nation's transformation from an early agricultural period, to a minerals-dominant economy, and finally to an economy dominated by services and amenity resources. From this approach, Hinze has derived a model of net migration in SMSAs, but his model is restricted to one time period limiting his ability to demonstrate change in the importance of determinants of migration. Obviously the narrow time span used in the present analysis is insufficient to show the broad changes described by Perloff and Wingo. Nevertheless, we can examine a truncated section for the developmental process in nonmetropolitan areas. In short, we will demonstrate that the importance



of determinants of migration has shifted substantially over the time periods considered, and will argue that these shifts reflect changes in organization at the societal level.

An additional proposition guiding this analysis is that various segments of society will be differentially affected by determinants of migration, depending on their position in the social structure. According to Hawley, "Migration is a means of redistributing that part of a population most directly affected by conditions of disequilibrium in closer conformity with the distribution of opportunities" (342). By far the most widely observed generalization regarding migration differentials is that the young are more mobile than the old (Shaw). True to our underlying proposition, this age differential reflects the prevalence of age grading in our social structure (Goldscheider, b). The elderly have been relegated to positions with marginal economic opportunities. It may come as a surprise, then, that a factor related to older age is one of the most frequently cited explanations for growth in nonmetropolitan areas: namely, retirement migration. Here we argue that, although the elderly may not be as affected by some variables, they may be more affected by others. Moreover, temporal change in the importance of these determinants will not be the same for younger and older persons. These ideas will be examined by relating the same sets of independent variables to elderly and young migration rates separately and results for age groups will be compared for each time interval.

### **Determinants of Nonmetropolitan Net Migration**

Such a plethora of social trends has been offered to explain recent nonmetropolitan population shifts that it is not possible to classify them accurately and succinctly and empirically verify each one, but the broad distinction between noneconomic and economic determinants provides a useful conceptual scheme if we allow for some overlap and ambiguity. Each of these sets of factors will be discussed in turn.

On the economic side, transformations of the economic base of the nation underlie most explanations of redistribution trends (Rees; Rostow). Perhaps the single most relevant set of developments for nonmetropolitan areas has been the industrialization of food production. To a large extent the process has run its course at least insofar as labor force demand is concerned. The dramatic exodus of labor, or of potential labor force entrants, from the farm appears to be nearly over. The potential for massive rural-to-urban redistribution as a consequence of introduction of capital intensive production techniques seems to have dissipated (Beale). Thus at the community level, the negative association between specialization in agriculture and migration should diminish over time.

Of course growth in other types of employment has absorbed excess farm labor and attracted new migrants to nonmetropolitan areas. Due to imbalance in demand and existing supplies of energy and other raw materials, areas which specialize in mining and which were previously experiencing outmigration are now growing (McCarthy and Morrison). Although the consequences of industrial expansion in nonmetropolitan areas is not completely understood or agreed on, it has undoubtedly led to some growth (Heaton and Fuguitt). It is now recognized, however, that growth in services is more important than manufacturing in accounting for new job opportunities (Haren and Holling). Of particular interest has been growth in recreation and tourist related activities (Beale; McCarthy and Morrison). Here it is impossible to separate economic and noneconomic dimensions since both are concentrated in the same geographical areas. Amenity-rich areas grow because they are nice places to live and contain job opportunities. At the community level, the association between mining and migration is expected to move in the positive direction. A similar but larger effect is expected for recreation, but trends for manufacturing are not as clearly predictable.

Somewhat more subtly, the importance of economic factors seem to be shifting as a consequence of the rising standard of living (Berry). Analogous to theories about a hierarchy of needs, it is argued that as incomes rise noneconomic factors take on increased importance. It is implied that once a certain level of living is obtained people place greater emphasis on nonpecuniary aspects of their lives. Therefore, we would expect a temporal decline in the drawing power of communities with high incomes. Indeed, evidence indicates that the effect of average income on migration has diminished in nonmetropolitan areas (Beale). Related to the declining importance of income, we would expect noneconomic characteristics such as a pleasant climate or scenic beauty, to be of growing salience in predicting net migration.

Because of the realization that noneconomic factors may account for nonmetropolitan growth, results of residential preference surveys have attracted scholarly attention. The expressed preference to live in more remote settings parallels recent redistribution trends, suggesting that social change has prompted consideration of residential preferences in the decision to move. Although there are several reasons to expect preferences to be important, there is no strong evidence linking preferences to actual migration decisions (Dillman). Nevertheless, if preferences are becoming more important in the decision to move, we should observe a change in size of place differentials favoring growth in smaller places and rural areas.

To summarize, change in social and economic conditions in the U.S. should produce temporal shifts in the effects which community characteristics have on nonmetropolitan migration. Thus (1) economic variables will become less important in explaining migration patterns, (2) variables

reflecting desirable environmental attributes will become more important in explaining migration, and (3) the traditional growth advantage of larger urban places within the nonmetropolitan sector will disappear and more remote areas will gain in popularity.

#### AGE DIFFERENTIALS

Various explanations have been given for observed age differentials in migration behavior, many of which indicate that older people are less affected by some determinants of migration and more affected by others, because of their position in the social structure. Migration is commonly viewed as a process which will promote upward social mobility and will provide a more efficient distribution of human capital (Greenwood, b). Thus economic determinants of migration have been emphasized. But it is generally recognized that the elderly stand to gain less from economic opportunities at least insofar as higher wages and upward social mobility are concerned (Cebula; Goldscheider, a; Lenzer). Many of the elderly are retired so that income is independent of location of residence. The major economic constraint is a result of the cost of living in different areas. For those who are still working, further investments in human capital will have low returns since withdrawal from the labor force is often anticipated in the near future. Because of a source of income that does not require presence in a fixed location, and lower motivation for upward socioeconomic mobility, older people may be more affected by noneconomic determinants of migration. Evidence reported by Long and Hansen indicates that older people are less likely to give job related reasons, and more likely to give noneconomic reasons for interstate migration compared with the younger population. Jobs or schooling are stated as motives by 67.7 percent of those aged 20–34 compared to 17.9 percent of those over age 55. Of particular interest to Long and Hansen, 12.1 percent of interstate migrants over age 55 gave climate as the main reason for moving, while only 2.6 percent of those aged 20–34 gave the same response.

Furthermore, the elderly population may be less affected by temporal trends. They are insulated from many changes which occur in labor markets since they are either not linked or are marginally linked to economic institutions because of their low participation rates. Changes in the economic base, in technology, organization of production, etc., more directly affect the working population. It stands to reason, from an ecological point of view, that the most productive segments of the population will be most responsive to differences in sustenance opportunities. Moreover, the rising standard of living which allows noneconomic considerations to gain salience is less dramatic among the elderly. For example, the ratio of mean family income of household heads over age 65 to mean family income for all families dropped from .74 in 1952 to .66 in 1972 (U.S. Bureau of the

Census, b). Because they have benefitted most from economic growth and are thus in an increasingly better position to consume leisure and recreation oriented goods and services, any increase in the importance of noneconomic determinants may be more obvious in younger populations.

In sum, where comparing older and younger populations, economic factors should be more important determinants of migration for the young than for the elderly, while the reverse will be the case for noneconomic factors. In addition, any temporal change in the relative importance of both economic and noneconomic factors should be most evident in the younger population.

### Data and Methods

Analysis will be restricted to the set of counties which were still classified as nonmetropolitan by 1974. (This classification is based almost entirely on results of the 1970 census.) A focus on nonmetropolitan counties derives from issues raised in the preceding discussion regarding the factors leading to a reversal in these counties. The decision to exclude those counties which were nonmetropolitan in 1950, but became metropolitan thereafter, was made so that the same units would be used in each time period.<sup>1</sup>

Three measures of the traditional economic base are derived from census labor force statistics, the percentages employed in agriculture, mining, and manufacturing. It should be noted that when all three percentages are included as independent variables in a regression equation, the percentage in all other types of employment (largely services) is an implicit variable analogous to the excluded category in a set of dummy variables. Thus each regression coefficient should be interpreted as the effect on the dependent variable of having additional employment in the specific independent variable rather than in all excluded types of employment. For example, a coefficient of  $-1.0$  for agriculture indicates that for every additional percentage point of employment in agriculture, vis-à-vis the excluded types of employment, the rate of net migration will be 1.0 units lower.

Median family income will also be included as an indicator of the general level of living. To some extent income may have an intermediate effect, mediating the relationship between the economic base and migration. For example the negative influence of high employment in agriculture on net migration may be, in part, because incomes are extremely low in such areas. Thus we will want to determine whether income has any influence over and above that of the economic base and amenity endowments. We would also like to include some measure of cost of living, but can find no reasonable indicator at the county level. Housing costs or

median rent have been suggested, but these factors may often be influenced by migration such that their inclusion would violate assumptions of the regression analysis.

Counties are also classified by their adjacency to metropolitan areas and by size of largest place (rural, 2,500–10,000 and 10,000+). Based on people's desire for some amenities of an urban society, it is expected that adjacent counties will exhibit lower outmigration or higher immigration than the more remote counties. But to the extent that people act on their preferences for a bucolic setting we would expect rural areas to experience a greater change in growth patterns than urbanized counties.

In measuring amenity-rich environments, a conceptual distinction can be made between natural and social characteristics. Scenic beauty, sunshine, water, forest, wild life, etc., form the basis for creation of a desirable environmental setting, but unless development occurs these qualities may go unnoticed. On the other hand, it may be possible to construct recreation or leisure oriented communities in a relatively barren natural environment. Thus, it is important to consider both types of characteristics.

The paucity of data on natural characteristics makes it difficult to construct any measures of physical desirability. Here only two factors will be considered. Mild temperatures are argued to be an important attraction especially for the elderly. Preliminary analysis indicated that a single variable ( $MILDTEMP = \text{average January temperature} / \text{average June temperature}$ ) which is directly proportional to the average January temperature and inversely proportional to the difference between June and January temperature is an efficient index of mild temperatures in predicting migration. The presence of water ( $WATER$ ) combines three separate values. Codes for presence of scenic rivers, lakes, and seashore were added to form a presence of water variable. The logarithm of area of inland water was taken as another indicator. These two variables were then standardized and summed to create the final water variable.<sup>2</sup>

Three other measures were combined to create an index of amenity development: percent of labor force employed in entertainment, recreation, and personal services, the number of hotels and motels per capita, and the proportion of housing units that are seasonal. Because these three variables were highly skewed, a logarithmic transformation was made on each. Then the transformed variables were standardized and summed to create the amenity index. Based on the possibility that a combination of characteristics is more desirable than the separate additive effects of each, we will also explore the interactive effects which these variables have on migration.

We do not contend that this set of variables is in any way comprehensive. Numerous other facets of the economic base, the amenity endowments, and the level of urbanization might be considered. The crucial point

is that we do have variables which measure these aspects of nonmetropolitan areas. Thus, it is possible to compare how types of variables relate to migration at different points at different points in time.

Two separate age groups of migrants are compared. Age 65 is generally accepted as the modal age of retirement and is considered to be the onset of old age. Thus age 65 will be the dividing point between younger and older migration. We recognize that this dividing point will make errors in both directions: some people under 65 have physically and socially reached old age, while other older persons are active members of the labor force and have consumption patterns identical to younger people. But any other point would make similar, and perhaps greater, errors of classification. Ideally, more than two age groups should be compared. In particular, those aged 50–64 are of interest because of the preretirement and early retirement moves which occur at this stage of the life cycle. Unfortunately, we are constrained by post 1970–75 population estimates, a focal period for this study, that are made only for the total population and for the population age 65 and over. We expect that coefficients for the 50–64 age group would lie somewhere between those for younger and older groups, and some evidence supports this expectation (Hinze), albeit in a different research context. County level migration figures for these two age groups are obtained for 1950–60 and 1960–70 from reports prepared by Bowles and Tarver and Bowles et al. For the 1970–75 period forward survival techniques were applied to 1970 census data and 1975 population estimates by county (see Tordella for more detailed description of the data).

Annualized rates of migration were computed to control for unequal time intervals. Here an important issue concerns the appropriate denominator for these migration rates. For outmigration, the concept of population exposed to risk applies and the mid-period population for each specific age group is an appropriate denominator. For immigration, however, this is not the case. Indeed, if a relatively large number of older people move into a county with a small base of elderly people, a rate with only elderly in the denominator will be overinflated. It seems just as logical to think of the total resident population as being at the risk of receiving elderly migrants in which case the total mid-period population is the denominator. Since one focal point of this research is on a period when immigration exceeds outmigration, we have opted to use the total population in the denominator. An additional advantage of this choice of denominators is that the sum of the young and elderly migration rates equals the total migration rate.

Counties vary a great deal in population size and measurement error is likely to be greater in smaller than in larger counties. For both substantive and methodological reasons a decision was made to weight each county by its population size. On substantive grounds, weighting

places greater emphasis on the areas where the largest volume is occurring and provides more direct information on which characteristics attract or repel the greatest number of people. In contrast, unweighted analysis would place the emphasis on which types of areas are more likely to grow or decline regardless of the absolute magnitude of the change. Weighting is desirable on methodological grounds, because it reduces measurement error to the extent that errors are greater in smaller counties. The weighting procedure will also deemphasize counties with inflated net migration rates resulting from a small population base.

Multiple regression analysis will be used to examine the effects of each set of independent variables. Comparisons will be made of the degree to which these effects change from period to period for young and elderly migration. Since the entire population of nonmetropolitan counties is included, formal tests for statistical significance are not needed. Standard errors are reported, however, as indicators of goodness of fit.

## Results

Rates of net migration in nonmetropolitan areas for the young, elderly, and total population are shown in Table 1. A turnaround from out- to immigration is evident from these rates. In the 1950s there was net movement of both the young and old from nonmetropolitan areas. In the 1960s young outmigration diminished and elderly migration shifted to positive, and during the first half of the 1970s both rates were positive. Focusing on the latest period one might argue that elderly migration is an important part of the turnaround. The elderly account for 12.9 percent of the total net migration while constituting 11.3 percent of the nonmetropolitan population in 1970. Migration does disproportionately redistribute old people into nonmetropolitan counties.

From a comparative perspective, however, elderly migration seems less significant as a component of the turnaround. First, we note that migration has consistently resulted in an older nonmetropolitan population. In the 1950s the elderly could only account for 1.9 percent of the total net outmigration, and in the 1960s the old were moving in while the young were leaving. Second, it is a change in the behavior of the young, not the elderly, that led to the turnaround. If the elderly had continued to move out at their 1950–60 rate, total net migration in the 1970–75 period would have been only slightly altered (.458 instead of .556). On the other hand, if those under age 65 had continued to move out at their 1950–60 rate, there would have been total net outmigration in the 1970–75 period (–1.264 instead of .556). These findings give credence to the proposition that changing response to determinants of migration will be greater among the younger population.

*Table 1. YOUNG, ELDERLY, AND TOTAL NET MIGRATION RATE IN NONMETROPOLITAN AREAS 1950-60, 1960-70 AND 1970-75 (ANNUALIZED RATE PER 100 POPULATION)*

	Period		
	1950-60	1960-70	1970-75
Young migration rate	-1.34	-.64	.48
Elderly migration rate	-.03	.02	.07
Total migration rate	-1.37	-.62	.55

As a caveat, this does not deny that retirement migration is an important part of the turnaround. Indeed, some evidence (Heaton et al.) indicates that retirement migration to nonmetropolitan areas is most dramatic among those who have not yet reached age 65.

#### DETERMINANTS OF YOUNG MIGRATION

Turning to more detailed results from the regression analysis, several interesting changes can be observed in the relative importance of determinants of migration.<sup>3</sup> Effects of sets of variables on the rate of net migration for persons under age 65 are shown in Table 2. In the 1950s there is marked net outmigration from areas specializing in agriculture, mining, and, to a lesser degree, in manufacturing. Together these variables account for nearly one-half of the variance in migration rates. In the second column, positive coefficients for adjacent counties and counties with larger places indicate that migration leads to a more centralized population distribution. Of the amenity variables, only recreational development has any noteworthy influence on migration in the 1950s. As was stated above, however, it is impossible to determine whether this is because of employment opportunities or quality of life factors. When all of the above-mentioned factors are included simultaneously (column IV), most of the relationships remain unchanged. But coefficients for size of largest place become slightly negative, indicating that centralization can be explained by the economic base and amenity endowment of areas. Also, the effect of the recreational development index declines sharply in size when other variables are included.

Income is introduced (column V) as an intervening variable. Higher median income apparently attracted new migrants or deterred outmigration in the 1950s and most other relationships are only slightly altered by inclusion of income in the regression equation. The major exception is a reduction in the coefficient for agriculture, implying that part of the explanation for outmigration from agricultural areas lies in the fact that such areas are economically depressed.



The final column for the 1950s (column VI) reports a test for interaction effects of amenity variables. Positive coefficients indicate that the combination of amenity factors are more influential than the separate additive effects of each. Nevertheless, these interactions do not contribute a great deal to the explanatory power of the model.

As a general rule the pattern of results for the 1960s parallel those for the 1950s, but coefficients are smaller in magnitude. For example, comparing results of model V for each decade, we observe smaller (in absolute value) effects for agriculture, mining, manufacturing, adjacency, water, mild temperatures, and income. Net of other variables decentralization to rural counties and growth in recreationally developed areas are stronger in the 1960s than in the previous period. Amenity interactions make roughly equal contributions in each period.

In the first half of the 1970s, we noted another precipitous decline in the effects of the traditional economic base variables. The drop in explained variance attributable to agriculture, mining, and manufacturing from over 40 percent to 2 percent represents an amazing shift in the determinants of migration. Although adjacency still has a small positive effect, the size of largest place variables become negative, either independent of or net of other variables in the analysis. Of the amenity variables, water has virtually no impact, mild temperatures appear to be more attractive in the 1970s, and recreational development has a larger effect. The net effect of income drops to virtually zero and amenity interactions retain roughly the same impact.

Comparing model V for each period, temporal shifts in coefficients are quite dramatic. Economic forces, as represented by the traditional economic base and income, were by far the most influential determinants of migration in the 1950s, but in the 1970s they had very little impact. In contrast, recreational development and, to a lesser extent, mild temperatures have emerged as important determinants of growth. At the same time, continuity in the adjacency effect and stronger negative coefficients for the more urbanized counties are consistent with the argument that preferences for rural areas within commuting distance of a metropolitan center are playing a greater role in migration decisions. In short, these data support the general proposition that, among the younger population (under age 65), there has been a shift in the relative importance of determinants of migration away from economic factors and toward noneconomic factors.

#### DETERMINANTS OF ELDERLY MIGRATION

Identical equations have been applied to migration rates of the population over age 65 and results are presented in Table 3. In the 1950s, the elderly also moved away from areas specializing in agriculture, mining, or manu-

**Table 2.** EFFECTS OF COUNTY CHARACTERISTICS ON THE RATE OF NET MIGRATION OF THE POPULATION UNDER AGE 65 IN NONMETROPOLITAN AREAS (STANDARDIZED REGRESSION COEFFICIENTS WITH UNSTANDARDIZED COEFFICIENTS AND STANDARD ERRORS IN PARENTHESES)\*

Independent Variables	1950-50								
	I	II	III	IV	V	VI	I	II	III
<u>Economic Base</u>									
1 Percent in agriculture	-.760 (-6.599) (.192)			-.663 (-5.803) (.284)	-.461 (-4.000) (.300)	-.454 (-3.945) (.297)			-.488 (-4.772) (.219)
2 Percent in mining	-.584 (-9.222) (.293)			-.538 (-8.497) (.341)	-.486 (-7.677) (.332)	-.475 (-7.500) (.328)			-.430 (-8.198) (.382)
3 Percent in manufacturing	-.210 (-2.493) (.251)			-.186 (-2.214) (.280)	-.130 (-1.545) (.273)	-.103 (-1.225) (.273)			-.070 (- .674) (.215)
<u>Urbanization</u>									
4 Adjacency		.157 (.522) (.064)		.140 (.465) (.052)	.109 (.363) (.051)	.104 (.346) (.049)			.175 (.445) (.050)
5 Largest place 10,000+		.426 (1.336) (.081)		-.025 (- .079) (.084)	-.058 (- .183) (.081)	-.032 (- .099) (.081)			.218 (.548) (.073)
6 Largest place 2,500-10,000		.142 (.436) (.078)		-.038 (- .117) (.066)	-.045 (- .137) (.064)	-.026 (- .080) (.062)			.050 (.129) (.073)
<u>Amenities</u>									
7 Proximity of water			.045 (.047) (.022)	.037 (.039) (.017)	.056 (.059) (.017)	-.097 (- .103) (.035)			.061 (.052) (.019)
8 Mild temperature			-.016 (- .048) (.058)	.000 (.001) (.046)	.113 (.341) (.051)	.026 (.080) (.059)			.042 (.099) (.047)
9 Recreational development			.391 (.282) (.014)	.097 (.070) (.014)	.060 (.043) (.014)	-.048 (- .035) (.025)			.263 (.163) (.013)
<u>Income**</u>									
10 Median family income					.322 (.065) (.005)	.254 (.052) (.005)			
<u>Amenity Interactions</u>									
11 Temperature x water						.172 (.153) (.030)			
12 Temperature x development						.121 (.153) (.073)			
13 Water x development						.068 (.026) (.006)			
Constant	1.428	-.2175	-.1.280	1.054	-.1.351	-.950	.564	-.1.129	-.724
R <sup>2</sup>	.437	.163	.171	.465	.507	.524	.275	.075	.087

\*Based on weighted regression analysis where each county is weighted by population size

\*\*In 100s of dollars

facturing. Although the adjacency effect was small, the elderly tended to centralize in more urbanized counties (column II). Both mild temperatures and recreational development were an attraction to the elderly, and these two factors account for nearly twice as much variation in migration as do the traditional economic base and urbanization. When all three sets of variables are entered simultaneously there is a tendency for coefficients to diminish, but the basic patterns are not altered.

Income actually had a small negative effect on elderly migration net of other factors (column V). This supports the claim that the elderly are

## Determinants of Migration

-70			1970-75				
IV	V	VI	I	II	III	IV	V
-.058 (-.075) (.039)	-.074 (-.096) (.041)	-.134 (-.174) (.038)	-.107 (-.244) (.049)			-.016 (-.036) (.051)	-.017 (-.038) (.051)
-.150 (-.378) (.057)	-.152 (-.384) (.057)	-.140 (-.353) (.051)	-.144 (-.554) (.079)			-.114 (-.440) (.073)	-.115 (-.441) (.073)
-.112 (-.143) (.034)	-.112 (-.144) (.034)	-.071 (-.091) (.031)	-.149 (-.211) (.030)			-.025 (-.036) (.030)	-.025 (-.035) (.030)
.058 (.020) (.006)	.065 (.022) (.006)	.049 (.016) (.006)		.059 (.024) (.009)		.052 (.021) (.007)	.054 (.022) (.007)
-.146 (-.049) (.010)	-.132 (-.044) (.010)	-.091 (-.030) (.010)		-.027 (-.011) (.013)		-.023 (-.009) (.012)	-.019 (-.008) (.013)
-.067 (-.023) (.009)	-.063 (-.022) (.009)	-.043 (-.014) (.009)		-.013 (-.006) (.013)		-.002 (-.001) (.012)	-.001 (.000) (.012)
.076 (.009) (.003)	.075 (.008) (.003)	-.022 (-.003) (.004)			.085 (.012) (.003)	.078 (.011) (.003)	.078 (.011) (.003)
.310 (.097) (.006)	.296 (.093) (.006)	.102 (.032) (.007)			.279 (.103) (.007)	.278 (.103) (.007)	.275 (.102) (.007)
.303 (.025) (.001)	.312 (.026) (.001)	-.218 (-.018) (.003)			.320 (.032) (.001)	.317 (.032) (.001)	.318 (.032) (.001)
	-.042 (-.001) (.0006)	-.201 (-.003) (.0004)					-.007 (.000) (.000)
		.053 (.035) (.028)					
		.186 (.103) (.022)					
		.073 (.024) (.006)					
.296	-.422	-.055	1.020	.680	.090	.401	.080
.324	.332	.348	.023	.014	.235	.255	.256

insulated from economic opportunities and do not generally move to obtain higher incomes.

Inclusion of amenity interactions (column VI) improves explanatory variance substantially. A combination of amenity factors, particularly temperatures and recreational development, are especially attractive to the elderly. Here it is interesting to note that the variance explained by regression's main effects ( $R^2 = .218$ ) and interactive effects net of other variables ( $R^2 = .374 - .259 = .115$ ) account for nearly 90 percent of the total variance explained in the 1950s.

**Table 3.** EFFECTS OF COUNTY CHARACTERISTICS ON THE RATE OF NET MIGRATION OF THE POPULATION OVER AGE 65 IN NONMETROPOLITAN AREAS (STANDARDIZED REGRESSION COEFFICIENTS WITH UNSTANDARDIZED COEFFICIENTS AND STANDARD ERRORS IN PARENTHESES)\*

Independent Variables	1950-60						1960s		
	I	II	III	IV	V	VI	I	II	III
<u>Economic Base</u>									
1 Percent in agriculture	- .445 (- .285) (.017)			- .288 (- .185) (.025)	- .334 (- .214) (.027)	- .335 (- .215) (.025)	- .208 (- .269) (.032)		
2 Percent in mining	- .315 (- .367) (.027)			- .219 (- .254) (.029)	- .230 (- .268) (.030)	- .207 (- .240) (.027)	- .240 (- .609) (.059)		
3 Percent in manufacturing	- .300 (- .263) (.023)			- .223 (- .195) (.024)	- .235 (- .206) (.025)	- .164 (- .144) (.023)	- .282 (- .361) (.032)		
<u>Urbanization</u>									
4 Adjacency		.009 (.002) (.004)		.048 (.012) (.004)	.055 (.013) (.004)	.043 (.011) (.004)		.022 (.007) (.007)	
5 Largest place 10,000+		.206 (.048) (.006)		.035 (.008) (.007)	.042 (.010) (.007)	.096 (.022) (.007)		-.089 (- .030) (.010)	
6 Largest place 2,500-10,000		.139 (.031) (.006)		.028 (.006) (.006)	.030 (.007) (.006)	.066 (.015) (.006)		-.026 (- .009) (.010)	
<u>Amenities</u>									
7 Presence of water			.026 (.002) (.001)	.031 (.002) (.001)	.027 (.002) (.001)	-.191 (- .015) (.003)			.067 (.008) (.003)
8 Mild temperature			.349 (.078) (.004)	.354 (.079) (.004)	.330 (.074) (.004)	.115 (.026) (.004)			.315 (.099) (.006)
9 Recreational development			.345 (.018) (.001)	.215 (.011) (.001)	.224 (.012) (.001)	-.226 (- .012) (.001)			.341 (.028) (.002)
<u>Income</u>									
10 Median family income					-.071 (- .001) (.0004)	-.272 (- .004) (.0004)			
<u>Amenity Interactions</u>									
11 Temperature x water						.238 (.016) (.003)			
12 Temperature x development						.527 (.026) (.001)			
13 Water x development						.154 (.004) (.001)			
Constant	.119	-.058	-.097	-.010	.030	.121	.165	.038	-.067
R <sup>2</sup>	.107	.024	.218	.257	.259	.374	.064	.005	.226

\*Based on weighted regression analysis where each county is weighted by population size.

\*\*In 100s of dollars

In the 1960s, there is a noticeable decline (toward zero) in the negative effects of traditional economic base variables, most notably agriculture. Also, the size of place coefficients shift to negative, indicating decentralization of the elderly to entirely rural counties. But effects for amenity variables, income, and amenity interactions are relatively stable across the two decades.

Comparison of coefficients for the 1970s with those for the 1960s reveal no sharp contrasts. Effects of traditional economic base variables undergo another small decline as do the negative effect for size of largest place. The only other notable decline is for the temperature-development



income were so influential. By the 1970s, traditional economic variables had roughly equal explanatory power in the two populations. But income still has a slightly positive effect for the young in the 1970s, compared to a miniscule negative effect for the elderly. Also, assuming that recreational development provides more of an economic incentive for the young and more of a quality of life incentive for the elderly, we note that this variable has a larger effect as indicated by the standardized coefficients in the younger populations in 1970. As an interesting possibility, it may be that prior elderly migration creates employment opportunities in service and recreation oriented industries, and that younger migrants then respond to these opportunities.

In contrast, noneconomic factors have a larger influence on elderly migration as indicated by the difference in standardized coefficients. This is consistently the case with the mild temperature variable and the water variable tends to have a slightly larger effect among the elderly. Only in the 1970s does recreational development have a larger effect on the young than on the old, but this, we have argued, is because of its dual influence as an economic and noneconomic factor. Perhaps the only exception is a tendency for the young to select more rural counties than the elderly net of other variables. In part this may be because the elderly are often more dependent; e.g., they need more frequent medical care, they are less mobile, and they are less able to afford travel.

Finally, it is apparent that temporal change in the magnitude of effects is larger for the young than for the elderly population. A large majority of variables undergo greater temporal shifts in relative importance among the young than among the elderly. A few have about the same change in each population. The one exception is the temperature-development interaction which declines somewhat in importance for elderly migration. Together these results provide strong evidence in support of the proposition that the younger population is more responsive to transformation in the social structure than are the elderly.

Two additional aspects of these models deserve comment. First, the overall explanatory power of these models decline over time, particularly for the young. In part, the decline in the latter period may reflect greater measurement error contained in the migration estimates. There are, however, substantive reasons to expect such a decline. Wardwell applies the concept of equilibrium migration to the nonmetropolitan turnaround, suggesting a reduction in migration differentials. Moreover, Bogue argues that in developed economies, as equilibrium obtains, push-pull forces decline in importance. Under such conditions the explanatory power of models such as ours would diminish.

Second, the variables included in these models do not necessarily account for the magnitude of the migration turnaround. Computations in Table 4 illustrate this point. If we accept the definition of the constant term

**Table 4.** CHANGE IN MEAN NET MIGRATION RATES DUE TO INCLUDED AND EXCLUDED VARIABLES

Period	Mean Net Migration Rate	Effect of Excluded Variables (the Constant Term)*	Effect of Included Variables ( $\sum b_i \bar{x}_i$ )*
<u>Under Age 65</u>			
1950-60	-1.34	-.95	-.39
1960-70	-.64	-.06	-.58
Change	.70	.89	-.19
1960-70	-.64	-.06	-.58
1970-75	.48	.25	.23
Change	1.12	.31	.81
<u>Over Age 65</u>			
1950-60	-.03	.12	-.15
1960-70	.02	.18	-.16
Change	.05	.06	-.01
1960-70	.02	.18	-.16
1970-75	.07	.11	-.04
Change	.05	-.07	.12

\*The constants and bs are derived from Model VI of Tables 2 and 3 for each decade and age group.

in a regression equation as the average effect of excluded variables (Rao and Miller) then we can decompose the mean migration rate into that part due to included variables (i.e., unstandardized coefficients times means for respective independent variables) and that part due to excluded variables (i.e., the constant term). Differencing terms for successive decades yields a decomposition of the total change in migration rates into change due to included and excluded variables. Excluded variables would reflect unmeasured conditions in nonmetropolitan counties such as employment growth and population pressure, and, perhaps more importantly, conditions in metropolitan areas such as crime rates, pollution, and cultural and economic opportunities. The decomposition (see Table 4) implies that unmeasured factors were of greater importance for change between the 1950s and the 1960s, whereas included variables were more salient for change between the 1960s and the 1970s.<sup>4</sup>

## Conclusions

Results reported here point to important structural changes occurring in society. Variables which were important predictors of migration in the 1950s had diminished impact in the 1970s, indicating that theories which

emphasize wage differences or sustenance structure of communities appear to be too narrowly conceived as explanations for shifts in migration patterns in nonmetropolitan areas. Explanation of those patterns must also take into account societal changes in economic conditions such as overall gains in the standard of living and growth of the retired population. Finally, shifts in the sustenance structure of the national economy toward recreational and leisure oriented consumption must be considered in order to understand recent trends.

Comparisons of age groups support prior research showing that the elderly population is less affected by economic variables and more affected by noneconomic variables compared with younger people. Furthermore, it appears that this attraction to noneconomic factors insulates the elderly from transformation of the economic structure of society. Changes in the importance of variables is more dramatic for the young than for the old. Thus, within the context of this research, statements regarding age differentials in rates of migration can be expanded to include the fact that there are age differentials in changing response to determinants of migration. However, an analysis of more detailed age categories than the two groups we were able to include here should be undertaken to confirm these conclusions.

One implication of these results is that the causal link between employment change and migration needs to be reconsidered in a temporal context. Policies designed to promote development have generally been based on the assumption that jobs attract people. The primary goal, then, has been to create jobs by inducing new industry and service activity to locate in target areas. But economists have also considered the possibility that people attract jobs (Greenwood, a; Muth). Evidence reported here suggests that this latter link between people and jobs may be gaining importance. An increasing segment of the population does not have location specific ties to a source of income. Moreover, it appears that as the standard of living is rising, an additional group of people are willing to trade-off purely monetary benefits for amenity related qualities at their place of residence. Locational decisions of these two groups of people could have a substantial impact on the growth of employment opportunities.

As Long and Hansen have noted, the rising importance of personal preferences for amenity-rich environments implies that environmental protection will become crucial for communities which want to maintain their populations. It is conceivable that overcrowding, overcommercialization, and general mismanagement may give birth to the ghost towns of the future. Beyond environmental consequences, areas of rapid growth may also undergo significant social and economic changes, the implications of which are yet to be discovered.



## Notes

1. A few of these counties were quite large, scored high on measures of recreational development, and were growing very rapidly. Examples include counties containing Tucson, Arizona; Fort Lauderdale and West Palm Beach, Florida; Palm Springs, California, and Colorado Springs, Colorado. This exclusion is detrimental to longitudinal comparisons, since similar rapid growing counties of the 1970s are not excluded nor can they be identified as their metropolitan status may not change until after 1980. Nevertheless, our judgment is that the rapidly growing counties which became metropolitan in the 1950s are somewhat atypical such that their inclusion would create even greater problems of longitudinal comparability.
2. Area of inland water was taken from a series of reports published by the U. S. Bureau of the Census (a). The other two water variables were provided by Oak Ridge National Laboratories. The first has four categories coded as follows: 1 = stream with 7-day/10-year low flow of at least 200 cfs., 2 = stream with 300 cfs. and storage capacity, 3 = great lakes, and 4 = ocean, and 0 otherwise. Our goal in combining these variables was to explain as much variance as possible in migration rates with as few independent variables as possible. Alternative combinations yield results very similar to those reported in the text.
3. Careful inspection of correlations among independent variables in the six matrices utilized for this analysis indicate that multicollinearity does not present a serious problem of interpretation. Consequences of the high correlation between percentage in agriculture and income ( $r = -.64$  in 1950) are indicated in the text. Interaction variables are highly correlated with their component variables, but interpretation in the text also takes this into account.
4. Explanation of variance between units is conceptually and methodologically distinct from explanation of the mean level of occurrence for these units. Thus, explained variance may decline at the same time that average effects of included variables on the mean rate of migration are increasing.

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# Suburban Racial Segregation as a Nonecological Process\*

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## ABSTRACT

*In the case of one large northern suburban region, the best predictors of change in racial composition of individual suburbs are the initial black proportion and the presence of white ethnics. The effect of initial racial composition reflects the process of increasing ghettoization of blacks which has commonly been found in the North. Ethnicity is interpreted as a basis for social solidarity in the context of a collective action model of community development. Its effect is found to be limited to working-class suburbs, suggesting that the emergence of ethnicity has some specific class and community bases. Finally, none of a series of community characteristics associated with the ecological life-cycle model has a significant effect on racial change.*

Several studies have documented the suburbanization of blacks which took place during the 1960s, providing evidence that this trend was accompanied by increasing segregation between suburban communities. The most suggestive study concluded that in the 1960s black suburbanization in New York and Chicago was concentrated in a few communities: "older suburbs which are experiencing population succession, new developments designed for Negro occupancy, and some impoverished suburban enclaves" (Farley, 512). A similar pattern has been reported for Detroit suburbs (Schnore et al.). These descriptions are consistent with Guest's finding that between 1950 and 1970 northern suburbs with more than average percent black had the greatest increases in black proportion (198-9).

This research, however, has not dealt explicitly with the causes of the apparent resegregation of blacks within suburbia, or with how changes in racial composition fit into the overall process of community develop-

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ment. The major theoretical reference point is the model of ecological succession outlined by Park and Taeuber and Taeuber. This model interprets the entry of blacks into communities and the ensuing replacement of white by black households as part of a natural community life-cycle which involves simultaneous increases in population density, intrusion on non-residential activities, decline in socioeconomic status, and increase in rental housing, all of which reflect the decline in attractiveness of aging communities (see also Hoover and Vernon). Rose's detailed descriptions of 12 "all-black" suburbs fit this model: most of these suburbs were working-class areas similar in housing, employment, and social status to the central city ghetto. Similarly, Guest has shown that northern suburbs which became predominantly black since 1950 (a process he labels Invasion-Succession and Declining suburbs) were older, poorer, and had higher proportions of renters than other suburbs (although these differences were less pronounced in the South). Guest's systematic comparison between types of suburbs offers the best evidence to link racial change to other characteristics associated with transition and succession in ecological theory.

In this paper we provide a limited test of the ecological life-cycle model as a predictor of the differential movement of blacks into some suburbs and not others. However, our main purpose is to explore an alternative model which emphasizes collective action as a determinant of community change. Recent field studies of communities experiencing racial change (Berry et al.; Goodwin; Molotch) document the strong response of white residents to blacks, their perception of loss of community status, and fear of continued change. While communities vary in their ability to arrest the process of racial transition or to manage integration, these studies imply that racial change is affected by the capacity of white (and black) residents for collective action.

Some recognition of the importance of community collective action is found in writings of the early Chicago School. Burgess noted that the second stage of the process of invasion and succession was the reaction to penetration by the inhabitants. McKenzie also pointed out that the resistance to invasion depends on the degree of solidarity of present occupants. But the idea of collective response was lost from the work of human ecologists as the predominant mode of analysis went from field studies to analysis of census data. Thus, the life-cycle model as currently interpreted describes community racial change as an unplanned, unmanaged, evolutionary process. Our knowledge of the politics of racial change is limited to a few case studies, which have themselves neglected the many communities which remained virtually all-white. What characteristics of social composition or organization distinguish suburbs which succeed in excluding blacks from those which do not?

## The Bases of Local Collective Action: Class and Ethnicity

One of the studies cited above (Berry et al.) describes white response to the arrival and expansion of the black populations in six different Chicago communities. Forms of response ranged from the militant activities of the Murray Park Civic Association with its history of racial violence to the Oak Park Housing Center whose goal is to maintain a racially balanced community through "reverse steering." The common element in all these cases from the researchers' perspective is the fear of status deprivation by whites who perceive black residents as a threat to the status of their community, and therefore symbolically to themselves. Any model of community-based collective action depends on recognition of the extent to which people perceive that their own lives are intertwined with the future of their community. However, we believe it is possible to move beyond the notion of "fear of status deprivation" to specify what is at stake for white residents in various class, ethnic, and community situations and what their options are in responding to their situation.

### THE EFFECT OF CLASS POSITION

Racial segregation takes place in a suburban context marked by a high level of segregation of social classes among suburbs, as well as great disparities in political and fiscal strength. In the suburban region which we have studied, the Nassau-Suffolk (Long Island) SMSA, for example, the coefficient of variation in median incomes of suburbs increased by nearly 60 percent during the 1960s. This ongoing process of community stratification results in the bifurcation of suburbs into two kinds of communities which differ radically in their prospects for racial change and in their responses to black entry. We will describe these two kinds of communities, particularly the character and sources of collective action in each.

The first type of community is the upper-class suburb. In one sense it may be expected that residents have a limited attachment to their community because, while it may provide them a pleasant environment and sense of social status, residents' social position is secured primarily by their place in the occupational structure. In addition, they have the financial resources to move if the community ceases to meet their needs and expectations. At the same time, these suburbs have ample resources to protect their interests. Blacks are presumably excluded in part by the cost of housing, maintained by strict governmental control over the density of new development. Further, formal organizations such as civic associations are available to express the community's perception of its collective interest.

The situation of the working-class suburb is quite different. The price mechanism cannot be counted on to exclude blacks, so that zoning

controls available through municipal incorporation are relatively useless tools. Formal organizations with the specific function of representing residents' interests are similarly weak. For many residents, home ownership is their only capital investment, and one which is periodically threatened by property tax increases and fluctuations in the business cycle which may mean loss of overtime or of employment for a working wife.

Protection of this investment may also have other meanings. For example, after a recent cross-burning on the lawn of a black family in a nearly all-white working-class Long Island suburb, one resident was quoted in the local newspaper: "The church is full every Sunday. It's clean. It's safe. You've got to understand that this is the first time in my life that I have lived in a community that feels like a family to me. I have seen what happens in the Bronx. When blacks move in, within a year the neighborhood is completely destroyed."<sup>1</sup> Here, the threat of racial change is a more salient dimension of people's sense of their community situation.

#### ETHNICITY AND RACIAL CHANGE

There has been considerable speculation about the ethnic factor in racial change. Clay proposes that "communities with no particular ethnic identity and those with a significant turnover are the prime targets since there is no dominant group to inhibit immigration or to keep demand high and vacancies low" (419). Schnore et al. (88) describe a Detroit suburb with a large Polish population in which the proportion non-white fell appreciably during the 1960s, and where "to put it most bluntly, blacks do not find it a 'friendly' city" (88). Finally, three of the six case studies reported by Berry et al. are "heavily ethnic central city communities" (226).

Our interpretation of ethnicity in this context parallels the understanding of ethnic antagonism which has developed in studies of the split labor market (e.g., Bonacich). Just as whites and blacks compete for jobs in the secondary labor market, they also compete for space in the suburban housing market. We propose that a complementary source of antagonism between white ethnics and blacks is their concentration in working-class suburbs.

In addition, urban researchers have long argued that ethnic behavior is primarily based on the conditions of working-class community life (Fried; Gans; Suttles). Ethnic identity and solidarity may emerge as a basis for collective action particularly under conditions of residential stability and segregation, common class position, and dependence on community institutions (Yancey et al.). Thus, if the working-class suburb is the likely arena for white-black conflict over residential space, it is also the likely context for the emergence of ethnicity as a nexus for community organization, one of the few bases on which the community can mobilize against racial change.

## Research Hypotheses

There is a potentially rich field for research on the relationships between class, ethnicity, and community collective action. Our purpose here is to suggest the relevance of these concepts to the community development process. According to the life-cycle model, which has received the most attention among urban sociologists, the selective movement of blacks into a few suburbs can be understood as part of a natural market process of transition and succession. Thus, the community characteristics of proximity to the central city, low socioeconomic status, high level of employment-related land uses, large size and density, aging housing stock, and high proportion of rental housing should be good predictors of racial change. From the perspective of a collective action model, by contrast, the prime determinant of racial change should be community solidarity, whether based on formal political organization or informal social networks. Limiting out attention to characteristics for which information is readily available for a large sample of suburbs, we hypothesize (1) that political incorporation and high concentration of ethnics among white residents will be negatively associated with increasing black population, (2) that the effect of incorporation will be stronger in affluent communities where the price mechanism can be used to restrict black entry and there are higher levels of local political participation, and (3) that the effect of ethnicity will be greater among working-class suburbs where ethnicity is more likely to emerge as a basis of collective action.

This formulation of the research problem focuses on the suburban community as the unit of analysis and therefore neglects other significant nonecological causes of suburban racial segregation. These include the structuring of a dual housing market by region-level actors (racial steering by realtors, mortgage lending policies of financial institutions) as well as the reinforcement of segregation by public action (location of subsidized housing projects, "dumping" of welfare recipients, neglect of fair housing legislation). These processes are beyond the scope of our research only because we lack systematic information on their occurrence.

## Sample and Measures

Our data are for the 1960–70 period of development of the Nassau–Suffolk SMSA, which extends to the east of New York City on Long Island. Within this suburban region, longitudinal data are available for 89 communities which had over 2,500 population in 1960. The general pattern of racial change in Nassau–Suffolk was similar to that described in previous studies of New York, Chicago, and Detroit. The proportion of blacks in

these suburbs increased from 3.1 to 4.6 percent, and the level of segregation between suburbs (as measured by eta-squared) increased from .198 to .296 in the same period. The new black population was disproportionately located in four suburbs. Three of these, in Nassau County adjacent to New York City, lost white population while gaining blacks; consequently, the percent black increased from 22 to 36 in Hempstead, from 17 to 68 percent in Roosevelt, and from 36 to 61 percent in New Cassell. In Wyandanch, further from the city, both white and black population grew, but the proportion black nevertheless increased from 41 to 60 percent.

Data for this study are taken primarily from the U.S. Census of Population and Housing, with exceptions noted below. However, estimates of total population, racial composition, ethnicity, and income were adjusted for boundary changes by the Nassau-Suffolk Bi-County Regional Planning Commission. Thus, unlike most census data, this information is not confounded by annexations and changes in place definitions used by the Bureau of the Census.

We have operationalized the community characteristics discussed above in the following ways:

1. Distance from city—the straight-line distance of each suburb from the central city boundary.
2. Class composition. To measure class composition an SES Index was calculated as a simple average of standardized scores for median income and education and percent white collar.
3. Economic function. Industrial employment figures within the census are only available for suburban communities which are incorporated. Because many of the communities within our sample are unincorporated places, we have defined industrial and employment communities through 1968 land use data provided by the Nassau-Suffolk Bi-County Planning Board. An employment/residential land use ratio was created as the quotient of commercial, institutional and industrial acreage divided by residential acreage for each community.
4. Size and density—measured by population size and total area in 1960.
5. Age of housing—the percent of total housing stock in 1960 built prior to 1940.
6. Percent renters—the quotient of renter occupied units divided by total occupied housing units.
7. Ethnic composition—measured by percent of population of foreign parentage.
8. Incorporation—whether the suburb was an incorporated municipality by 1960.



## Results

Correlations among all pairs of variables are presented in Table 1, and regression estimates of the effects of independent variables on change in proportion black for all 89 communities are provided in Table 2. We have employed a step-wise procedure, deleting variables which had a regression coefficient smaller than its standard error. In this model, 1970 black proportion is predicted as a linear function of 1960 black proportion and the 1960 values of other independent variables.

Table 2 shows that by far the best predictor of the percent black a community will have in 1970 is the percent black in that community in 1960. Some persistence in black concentration is expected (perfect persistence being indicated by the equation  $X(1970) = 1.0X(1960)$ ). But a slope substantially greater than 1.0 indicates more than just the fact that blacks are still living in the same communities as in 1960. Rather, the absolute differences in black percentage between communities increased during the period, due to greater increases in suburbs with higher initial percent black. Indeed, the standard deviation in percent black nearly doubled during the decade.

Although inspection of the correlation matrix shows that black suburbs were likely in 1960 to be older, poorer, and renting communities, none of these variables had independent effects on change in percent black, contrary to the ecological life-cycle theory. This suggests that while such characteristics help determine which suburbs develop initial black concentrations, the subsequent process of change has other causes. Population density did have a significant effect. Denser suburbs had lower than average proportion black in 1960, but by 1970 were neither lower nor higher than less dense suburbs, which is somewhat different from the expectations of the life-cycle model. Finally, in this suburban region, variables which are commonly believed to be relevant to racial redistribution (distance from city, size, economic function) have no effect.

The effect of ethnic composition, by contrast, was significant. Communities with a large ethnic population in 1960 had smaller increases in black proportion. Political incorporation, the other organization variable, has no effect.

Because we believe that ethnic composition and political incorporation have different meanings for working-class and affluent suburbs, we repeated the analysis separately within each subsample of communities, including only initial racial composition, density, ethnicity, and incorporation as predictors. The results are given in Table 3.

The regression for working-class suburbs parallels our findings for the full sample. There is a strong effect of initial black proportion which reflects the movement of blacks into already black suburbs. The average black proportion in these working-class communities increased from 3.4

Table 1. CORRELATIONS AMONG COMMUNITY CHARACTERISTICS 1960-1970 WITH MEANS AND STANDARD DEVIATIONS

	% Black 1970	% Black 1960	Distance	Incorpo- ration	SES 1960	Size 1960	Density 1960	% Old Housing 1960	% Rent 1960	% Foreign Stock 1960	Land Use
% Black 1970	1.00										
% Black 1960	.80	1.00									
Distance	.02	.03	1.00								
Incorporation	.01	.04	-.28	1.00							
SES 1960	-.21	-.16	-.37	.37	1.00						
Size 1960	.02	-.05	-.30	-.20	-.15	1.00					
Density 1960	.00	-.13	-.57	.20	-.08	.45	1.00				
% Old housing 1960	.28	-.39	.03	.45	-.10	-.21	.12	1.00			
% Renters 1960	.30	.40	-.13	.35	-.16	-.10	.26	.53	1.00		
% Foreign stock 1960	.06	.23	-.37	.13	-.03	-.05	.25	.11	.43	1.00	
Land use	.07	.14	.05	-.09	-.13	-.06	-.18	.06	.38	.06	1.00
Mean	.039	.029	109.9	1.46	.017	14728	8.85	.35	.15	.39	.24
Standard deviation	.089	.049	62.7	.50	.933	11965	5.34	.23	.14	.08	.19

**Table 2.** DETERMINANTS OF CHANGE IN SUBURBAN RACIAL COMPOSITION 1960-1970

Variables	b	Standard Error of b	$\beta$	F <sub>1,85</sub>
Initial percent black	1.576	.117	.87	185.48
Percent foreign stock	-.212	.076	-.18	7.69
Population density	.003	.001	.16	6.09
$R^2 = .68$				
$F_{3,85} = 61.2$				

percent in 1960 to 5.3 percent in 1970, accompanied by a near doubling of the standard deviation from 6.2 to 11.5 percent. This indicates the increasing segregation of blacks among working-class suburbs. In addition, ethnicity is a significant predictor of racial change, leading to an increasing segregation between blacks and white ethnics in particular. Indeed, whereas for these suburbs in 1960 proportion black and ethnic were positively associated ( $r = .20$ ), they were negatively associated by 1970 ( $r = -.39$ ).

The results for upper-class suburbs are quite different. The regression coefficient for initial proportion black is less than 1.0, reflecting the fact that these suburbs registered a decline in average black percentage, from 2.1 to 1.9 percent. Ethnicity has no effect on racial change and, indeed, in both 1960 and 1970 proportion black and ethnic were positively associated ( $r = .52$  and  $.43$ , respectively). Apparently among wealthier suburbs both blacks and ethnics were segregated from the nonethnic white population.

Incorporation is not a significant predictor of racial change even among affluent suburbs. This finding forces us to question the assumption that these suburbs exclude blacks primarily through zoning controls imposed by local government. It is relevant here to note that in the absence of municipal incorporation, zoning authority for Long Island suburbs is in the hands of town government, intermediate between the level of village (or city) and county. Perhaps, therefore, incorporation is not a prerequisite to a substantial local influence over zoning and, indirectly, on housing costs. We are left with the unresolved question as to whether housing costs are, in fact, the primary means of preventing integration of affluent suburbs.

We have taken one further step in the analysis of available data on this issue. Given the family income distribution of both blacks and whites in the suburban region, and the income distribution within each suburb, it is possible to calculate an expected percent black who would reside in each

Table 3. DETERMINANTS OF CHANGE IN RACIAL COMPOSITION BY COMMUNITY SES 1960-1970

Variable	Working Class Communities (N = 51)				Affluent Communities (N = 38)			
	b	Standard Error of b	B	F <sub>1,47</sub>	b	Standard Error of b	B	F <sub>1,34</sub>
Initial percent black	1.593	.154	86	106.87	708	.19	.630	13.20
Percent foreign stock	-.288	.128	-.19	5.05	.047	.05	.160	.83
Population density	.003	.002	.14	2.65	.000	.000	.034	.05
		R <sup>2</sup> = .69				R <sup>2</sup> = .52		
		F <sub>3,47</sub> = 35.6				F <sub>3,34</sub> = 12.2		

suburb if income were the sole cause of residential choice. We have compared this expected black proportion with the actual proportion of blacks in each suburb in 1970.<sup>2</sup> The first result of this procedure is that upper-class suburbs (defined as higher than average on the SES index for 1970) have lower expected proportion black than working-class suburbs. This follows from the fact that black families had lower average incomes than whites (\$9,054 and \$13,358, respectively). Second, among both sets of suburbs, the actual distribution of blacks differed from the expected distribution (i.e., blacks were more segregated than would be predicted simply on the basis of income differentials). Thus, processes other than the price mechanism were clearly at work in both affluent and poor suburbs to segregate the black population. But third, the average difference between expected and actual percent black was smaller among upper-class suburbs (a difference of 3.1 percentage points, versus 4.7 points for working-class suburbs). A t-test of this difference in means between the two subsamples is significant at well below the .001 level.

## Conclusions

Our research leads to several conclusions, limited by the breadth of the sample (one northern suburban region) and reliance on census data for comparison of causes of racial change in a large number of suburban communities. Probably the most surprising finding in light of the existing literature is the negative one: ecological variables associated with the standard life-cycle model are not significant predictors of change in racial composition of these suburbs. Perhaps further back in the history of this region, in the decades of the 1940s or 1950s, these characteristics did play some part. By 1960 blacks were much more likely to be concentrated in suburbs with older housing and higher proportions of renter households. Other nonecological factors were also surely important during that period (e.g., the initial development of all-white Levittown at a time when FHA mortgages were restricted to whites). But once established, the best predictor of racial change after 1960 was the initial black population. In extreme cases, black population grew from 10–20 to 40–70 percent black in only 10 years. The pattern of black residential location in 1970 was clearly structured by 1960.

Ethnicity was a significant factor, but only in working-class suburbs where blacks' choice of residence was less restricted by price. We interpret this result as consistent with a collective action model of community development. Ethnicity emerges primarily in the context of the working class community, and in the absence of more formal secondary associations provides a principal basis of social solidarity and collective action to resist community change. Nevertheless, we believe that we have only touched

on a major research problem, one which will require a combination of survey and fieldwork methods to pursue further. Some of the questions which now need to be addressed are these.

1. How do people perceive the phenomenon of racial change in relation to their own interests and their interests in the local community?
2. What is the nature of ethnic consciousness, what are its causes, and how is it related to a consciousness of class and community?
3. What are the social networks, formal and informal, through which collective action is organized and carried out?

### Notes

1. Quoted in *Newsday*, October 1, 1979.
2. This procedure is subject to two sources of error. First, up to 10 percent of the families in individual suburbs and in the two-county region did not provide information to the census on family income, and it is possible that nonresponse rates were different for blacks and whites. Second, the income distributions are provided for families while population is reported as number of individual residents; the proportion of black families expected under our projections is only an approximation of the proportion of black residents reported by the census.

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# Dangerous Places: Crime and Residential Environment\*

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## ABSTRACT

*This research investigates how the characteristics of residential areas in the city affect where crimes occur. Using data for city blocks in Cleveland and San Diego, the analysis tests three major hypotheses relating crime to (1) household composition, (2) features of the residential environment, and (3) the interaction of the social composition and the features of the residential environment. The results support the major hypotheses and show that a substantial portion of the variance in crime depends on the opportunities provided by the social and physical differentiation of the city.*

The relationships between crime and the conditions of city life are an important concern in urban sociology. Most early research on urban crime inquires where juvenile and adult offenders lived before apprehension in order to uncover the effects of city life on the development of criminal motivation and behavior. Much recent research is on the victim and deals with either the types of persons and households who become victims (Ennis; Skogan, b), or the types of places where crimes occur (Boggs; Schmid, a,b). This latter type of research is a relatively small part of the urban crime literature, although it has a longer history than the study of victim characteristics. For the potential victim, where crimes occur is the crucial issue (Wolfgang). Offenders need not and often do not commit crimes where they live. Others do not always become victims in the areas

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where they live.<sup>1</sup> The transforming of potential offenders and potential victims into offenders and victims may depend on the environments in which both are present (Cohen and Felson).

This research examines how the characteristics of the residents and of the housing environments of city blocks affect the amount of crime on all the residential city blocks of Cleveland, Ohio and San Diego, California in 1970. The purpose is to identify the conditions associated with incidence of crime. Behavior necessarily takes place in an environment and its characteristics can affect the interactions that occur. Studies of individuals in different types of environments support this (Gove, et al.; Loo; Sommer; Verbrugge and Taylor). Studies from Shaw and McKay to the present indicate that the amount of crime and delinquency in different areas of cities varies with their social compositions and housing environments (Boggs; Choldin and Roncek; Lander; Pyle et al.; Schmid, a,b; Schmitt).<sup>2</sup>

### **Theoretical Background**

The basic concern underlying this research is the effect of the urban environment on social control. This issue is fundamental for Wirth and Simmel and informs the sociological literature on cities. Although not all students of the city agree with their views, the issues they raise remain important. For Wirth and Simmel, the size, density, and heterogeneity of the city promote deviant behavior. These variables are still the subject of research examining the variation in deviance, especially crime, across and within cities (Galle et al.; Gillis; Harries).

For Wirth and Simmel, the city produces an anonymous milieu which permits deviant behavior because informal social control is relatively weak. Newman's (a,b) and Jacobs' works reflect this view of an anonymous urban environment producing the conditions for deviant behavior, especially crime. Their concepts of "defensible space" and "eyes on the street" point to the way certain types of places in cities provide settings for crime by impeding observation and intervention.

The differences in opportunities for behavior are crucial for Fischer and for Cohen and Felson. For Fischer, the diversity of people and environments in the city provides opportunities for behaviors that are not possible in smaller, less heterogeneous places. Although he rejects Wirth's and Simmel's emphases on the anonymity and isolation of individuals, his perspective is still compatible with theirs. Fischer argues that the availability of sufficient people with similar views, including those who accept illegal activity, permits the development of subcultures supporting many types of deviance. While subcultures may foster deviant or criminal motivations, their conversion into crime can depend on the anonymity possible in different areas of the city, so permitting the isolation of victims or

targets. The concept of guardianship (Cohen and Felson) refers to those routine activities of people which affect crime. Many of these may not be formally noticed, such as inquiry into a stranger's purpose for being at a particular place or extended observation of such individuals.<sup>3</sup> Such activities can affect a potential criminal's decision to commit a crime. Newman (a,b) and Jacobs argue that they do and that such activities depend on the residential environment.

Crime is often situational (Block) and often is determined by the available opportunities at a particular place and time. Opportunities result, in part, from the probabilities of detection, intervention and apprehension in different places. These in turn depend on the types of activities and other characteristics of different areas (Cohen and Felson; Jacobs; Newman, a,b). Peoples' vulnerability stems both from their particular characteristics and behaviors (Cohen and Felson), and from the context in which they are located. Cities are not random collections of individuals, but are structured organizations of people and environments (Hunter; Rees). This organization ultimately affects the amount of crime in different areas by altering levels of social control. Previous research neglects the effects of three major aspects of urban organization: (1) household composition; (2) features of the housing environment other than overcrowding and density; and (3) the interactions between the characteristics of the residents and housing environments.

#### **HYPOTHESES**

The ability of people in an area to distinguish those present for legitimate purposes from potential offenders affects vulnerability. This ability depends on the cohesiveness of people in an area and their concern over the local environment. Both of these vary with the types of households in an area. The basic types of households are families which "consist of a household head and one or more other persons living in the same household who are related to the head by blood, marriage or adoption" and primary individuals who are household heads "with no relatives living in the household" (U.S. Bureau of Census, b,53). In areas with high concentrations of families, there is much use of and concern over the local environment (Greer). Conversely, primary individuals spend much time away from home and have higher victimization rates than those in family households (Cohen and Felson). Burglary occurs frequently in areas where residents are seldom at home (Reppetto) and areas with low concentrations of families have a high incidence of crime (Boggs; Polk; Schmid, a,b). Zorbaugh notes the problems in areas with many individuals living alone. The concentrations of different types of households in an area can affect the vulnerability of all households there. Both primary individuals and

families in areas with high concentrations of the former as well as persons merely using such areas may be more vulnerable than those in areas primarily containing families. Thus, the first hypothesis is that *the higher the concentration of primary individuals in an area, the more frequently will crime occur.*

Areas of the city differ not only by the types of households they contain, but also by the types of environments they provide. By altering activity patterns in an area, different environments can change the amount of crime occurring. Indeed, the effects of the urban environment are an important interest in the urban crime and delinquency literature and reflect Wirth's and Simmel's attention to the effects of population concentration. The effects are controversial and the literature can be confusing because part of it examines whether different environments affect the incidence of crime (Booth et al.; Galle et al.), and another part examines how the environment affects where crimes occur (Boggs; Schmid, a,b). Even in the occurrence literature, environmental effects are often unclear because only density and overcrowding are examined. (Density is the ratio of the population of an area to the amount of space it occupies. Overcrowding measures the concentration of people in housing units in persons per room.) Booth et al. argue that population concentration is complex and has several aspects which can affect crime. To help remedy this problem, this research uses three additional features of residential areas, the number of residents, the population concentration in the surroundings, and the type of housing. Each can affect the vulnerability of potential victims. Verbrugge and Taylor find that recognition of and interaction with neighbors depend on the size of the resident population. Since it alters the number of people using an area, variations in population concentration can affect crime.<sup>4</sup> Newman (a) argues that part of the crime problem of housing projects results from the number of units in project buildings.<sup>5</sup> Conversely, Jacobs argues that heavily used areas are safe because victims are hard to isolate. Thus, higher values of these variables can decrease crime. Therefore, the second hypothesis is that other features of the environment besides density and overcrowding, specifically, *the size of the resident population, the surrounding population concentration, and the types of housing, will affect the incidence of crime.* The directions of the effects of these variables cannot be specified in advance because both positive and negative effects are possible.

Urban crime and delinquency research often seeks to separate the effects of the social composition of neighborhoods from those of the environment (Freedman; Galle et al.; Gillis). Such strategies are inappropriate if the environment has statistical interactions with the characteristics of the residents. For example, some studies find that socioeconomic status and environmental variables affect crime or delinquency and suggest that dense, poor areas have high rates (Schmitt, Galle et al.). Newman (a) finds that the size and design of housing projects have interactive effects on the

incidence of crime and suggests that the concentration of poor, minority residents in housing exacerbates the crime problem (Newman, b). Thus, the third hypothesis is that *crime will be highest where the characteristics of the residents and of the environment increase the opportunities for crime*. For example, crime will be highest in areas with high concentrations of primary individuals in large apartment buildings in dense areas.

Four problems complicate research on urban crime patterns. First, even among large U.S. cities, there are many differences between older cities and younger cities and their crime patterns may differ (Harries). Second, the relationships of crime to the characteristics of different parts of the city can depend on the unit of analysis. Third, crimes differ; therefore, the effects of the social composition or the environment may differ by the type of crime. Fourth, other variables that are not central to this analysis may also affect the amount of crime. The section describing the data explains how each of these problems is taken into account.

The task is to identify how the social composition and environments of city blocks affect crime incidence. The three major hypotheses concern the effects of: (1) household composition; (2) several features<sup>a</sup> of the environment; and (3) the interactions of the social and environmental variables. The analysis cannot identify the complex social dynamics which produce crime, but it can contribute to understanding urban crime in two ways. First, it will clarify how much of the variation in crime within the city is due to the structure of opportunities provided by the variation in the social composition and environmental features of residential areas. Second, the patterns identified can sensitize future researchers to the types of variables and areas which should be studied intensively to understand the processes by which residential environments affect crime.

## Data

### THE CITIES

To cope with the variation in crime patterns by type of city, the analysis examines crime patterns for Cleveland and San Diego for 1970. Previous intraurban studies of the effects of environmental variables on crime patterns use only a single city. Cleveland has a conventional U.S. urban density while San Diego is a new type of low-density city, built in the era of the automobile. Cleveland and San Diego are similar in total population but differ markedly in density. Cleveland's population in 1970 was 751,000 and its density was 9,893 persons per square mile. The figures for San Diego are 697,000 and 2,199. They also have different overall crime levels: San Diego ranked third lowest in robbery and fourth lowest in property crime among the thirty largest cities while Cleveland was eighth highest

in both robbery and property crime (Carol). Finally, minorities represented a greater part of Cleveland's population than of San Diego's: Cleveland's largest minority was black; San Diego's was Mexican-American. Using these two cities enables determining whether the independent variables have similar effects despite the differences in crime levels and environments.

The cities have a wide range of block-level densities. The average density of city blocks in Cleveland was 52 persons per acre with a standard deviation of 35. The average for San Diego was 37 persons per acre with a standard deviation of 31. Thirty percent of the blocks in San Diego have low, suburban density levels under 23 persons per acre. Studies of Chicago, which dominate the literature examining the effects of density, have never included such low density areas. Thus, this study contains areas with considerable variability in population density.

#### **UNIT OF ANALYSIS**

Taeuber and Taeuber note that "City blocks are . . . the smallest readily identifiable subareas for which reliable data can be tabulated" (226). A city block is "a well-defined rectangular piece of land bounded by streets or roads. However, it may be irregular in shape or bounded by railroad tracks, streams or other features" (U.S. Bureau of the Census, a,iv). City blocks are relatively homogeneous socioeconomically and in housing conditions. Since the concern is with the locations of crime, the size of the unit of analysis is a crucial issue. Most locational studies dealing with criminals or delinquents (Freedman; Galle et al.; Schmitt; Winsborough), crime (Boggs; Pyle et al.; Schmid), or the effects of spatial concentration (Galle et al.; Gilles) use either census tracts or larger units of analysis. (See Roncek (a) for a detailed examination of the methodological problems in studies of the relationships between spatial concentration and crime.) With units as large as census tracts, aggregation error could inflate apparent statistical relationships (Hammond; Hannan; Slatin; Yule and Kendall). Using block-level data does not eliminate the problem of aggregation, but it permits a much more precise identification of dangerous urban spaces.<sup>6</sup> All of the residential blocks in each city are in the analysis. Cleveland has 3,985 blocks and San Diego has 4,586.

#### **DEPENDENT VARIABLES**

The numbers of property crimes and violent crimes occurring on each block are the dependent variables. Property crimes are burglaries, grand thefts and auto thefts. Violent crimes are murders, rapes, assaults, and robberies. Using both property and violent crimes permits comparing the effects of the independent variables across crime types. The crime data are

offenses known to the police and, although they typically underestimate the amount of crime because of nonreporting, they are the best data available on a city-wide basis despite their deficiencies (Nettler). The address at which each crime occurred was obtained directly from the Cleveland and San Diego police departments. These addresses were assigned tract and block numbers (geocoded) using Census Bureau computer programs. The crimes were tallied by type for each block and merged with the census data for city blocks from the Third Count Summary Tapes. Using composite frequencies helps avoid the distributional problems that could occur using individual crimes. Using the number of crimes rather than rates avoids the problem of some blocks with many crimes having low rates because of having many residents. In such cases, rates conceal which blocks have high frequencies of crime. Using the block population as an independent variable indicates how heavily the frequency of crime depends on the opportunities provided by the number of residents.

#### INDEPENDENT VARIABLES

The three groups of independent variables represent the social composition, the residential environment, and other demographic aspects of the blocks that could affect the amount of crime. The effect of the household composition of residential areas is the subject of a major hypothesis, but it is necessary to control for the effects of socioeconomic and ethnic compositions. The following variables represent these different aspects of social composition: the percentage of primary individuals; the value of owned housing<sup>7</sup>; the percent black; and the percent Spanish.<sup>8</sup> The percentage of primary individuals is a direct measure of the lack of family households.

Five variables reflect the residential environment. Following previous studies, the analysis includes density, measured in persons per acre, and overcrowding, measured as the percent of persons living in housing units with 1.01 or more persons per room. The population of the block is the third environmental variable. Using it as a predictor permits examining if the amount of crime depends on the number of residents on a block. The fourth environmental variable, the percent of units in ten-or-more unit structures, represents the presence of apartment buildings.<sup>9</sup>

Population potential is introduced in this study as the fifth environmental variable in order to reflect the concentration of population surrounding a block (Choldin and Roncek). Population potential, measured in persons per mile (Choldin and Roncek; Duncan et al.; Heer), indicates the potential contacts possible on a block.<sup>10</sup> For Simmel and Wirth, urban density was important because it vastly increased the number of potential contacts among people. For a city block, density is not an adequate measure of potential contact. Consider two blocks of equal density, one surrounded by open space, the other by densely populated blocks. Population

density does not reflect the adjacent open space in the first case or the adjacent people in the second. Clearly, the potential for contact is higher on the second block.<sup>11</sup>

The last set of variables may also affect the vulnerability of blocks. Preliminary analyses show that the percent of families that are female-headed, the sex ratio, and the percent of males aged 18 to 24 have virtually no effect on crime in either city, but that the percent over age 60 and the vacancy rate do. Therefore, the analysis omits the first three variables, but includes the percentage over 60 and the vacancy rate.<sup>12</sup>

## Method

The analysis uses multiple regression equations predicting the logarithm of the dependent variable from the logarithms of the independent variables. Such equations are common in economics and are called Cobb–Douglas Production Functions (Theil) or double-log equations (Johnston). Equation 1 is a double-log equation.

$$\log y = \log a + b_1 \log X_1 + b_2 \log X_2 \dots + b_j \log X_j + \log e \quad (1)$$

The reasons for using regressions with logarithms relate to the distributions of the dependent and independent variables and the interpretation of these equations. For both crime measures in both cities, the frequency distributions resemble negative exponential curves. Apart from occasional fluctuations, the number of blocks with a particular level of crime decreases as the amount of crime occurring on a block increases. For example, in both cities, there are more blocks with no crimes on them than blocks with one crime or two crimes and more blocks with one crime than with two crimes or any other higher number of crimes. Ordinary regression on the frequencies of crimes may be inappropriate because these variables do not have normal distributions. Several of the independent variables have similar distributions.<sup>13</sup> Using the logarithms of these variables is one technique for transforming them to normal distributions.

The most important reason for using the logarithms concerns the form of the double-log equation in equation 2.

$$y = a(X_1^{b_1})(X_2^{b_2})(X_3^{b_3}) \dots (e) \quad (2)$$

Converting a regression equation with the dependent and independent variables in logarithms to its non-logarithmic form yields an equation in which the dependent variable is the product of the independent variables raised to certain powers. Such an equation is similar to a complex interaction term. Using double-log regression equations permits examining such interactions in a very simple fashion avoiding the complications of

using ordinary, multiplicative interaction terms in regression (see Southwood; Stolzenberg).<sup>14</sup>

## Results

Evaluating the importance of the interactive effects requires comparing the double-log regressions with ordinary multiple regressions. Table 1 contains the zero-order correlations, means, and standard deviations for the variables before logging. Based on these correlations, the ordinary multiple regression equations for Cleveland explain 44.2 percent of the variance in property crime and 37.2 percent of the variance in violent crime. The figures for San Diego are 15.3 percent for property crime and 11.1 percent for violent crime.<sup>15</sup>

Table 2 contains the zero-order correlations, means, and standard deviations for the variables after logarithmic transformation. Some of the correlations of the independent variables with the crime measures increase after logging because logging changes the distributions of the variables. Most of the correlations among the independent variables in Table 2 are low, although a few are of moderate size. Using the Hartovsky test (Rockwell) on the correlation matrix of independent variables for each city indicates that significant collinearity among these variables is not present for either city.

Table 3 presents the double-log regressions for both crime measures for both cities. The explained variances for the double-log regressions are larger than those for the corresponding ordinary multiple regressions.<sup>16</sup> The double-log regressions increase the explained variance for Cleveland by 9.7 percent for property crimes and by 8.9 percent for violent crimes. The figures for San Diego are 3.1 percent for property crimes and 2.2 percent for violent crimes. Without adding any new variables to the regression, logging improves the power of the independent variables in accounting for crime by approximately 20 percent.

The results in Table 3 support the major hypotheses. As expected from the first hypothesis, the percent of primary individuals, the indicator of household composition, has an important effect in every equation. The higher the percent of primary individuals is, the more crime occurs. Although the sizes of the effects vary, the statistically significant effects for the three additional environmental variables in all the double-log regressions provide support for the second hypothesis. Clearly, variables other than density and overcrowding are important. The increase of approximately 20 percent in the explanatory power of the independent variables supports the third hypothesis about the importance of the complex interactions among the social and environmental variables. It is these interactions which the double-log regressions take into account. For each crime



Table 1. ZERO-ORDER CORRELATIONS, MEANS, AND STANDARD DEVIATIONS BEFORE LOGGING

	1. Property Crimes	2. Violent Crimes	3. % Primary Individuals	4. % Overcrowded	5. Density	6. Block Population	7. % Units in Multi-unit Structures	8. Population Potential	9. Owner Value	10. % Black	11. % Spanish	12. % Over Age 60	13. Vacancy Rate
1.		.694	.279	.086	.050	.473	.414	.321	-.178	.328	-.067	-.050	.235
2.	.484		-.272	.162	.098	.363	.361	.362	-.218	.376	-.063	-.038	.298
3.	.195	.148		-.164	-.038	-.014	.568	122	-.216	-.028	.046	.468	.257
4.	.116	.161	-.229		.234	.111	-.049	.190	-.310	.229	.152	-.386	.241
5.	.030	.045	.141	.043		.279	.116	.335	-.288	.175	.161	-.200	.147
6.	.201	.082	-.135	.071	.066		.249	.144	-.013	.210	-.094	-.107	.021
7.	.197	.116	.578	-.102	.159	.079		.098	-.031	.074	-.009	.149	.225
8.	.117	.100	.201	-.060	.054	-.027	.103		-.510	.590	.007	-.125	.344
9.	-.141	-.165	-.030	-.388	-.053	-.065	-.013	-.447		-.167	-.291	.081	-.398
10.	.188	.231	-.104	.480	-.009	.020	-.097	.075	-.302		-.271	-.253	.192
11.	.134	.158	-.042	.524	-.019	.008	-.010	.058	-.449	.316		-.051	.146
12.	.012	.001	.500	-.330	-.104	-.159	.226	.175	.056	-.161	-.119		.004
13.	.096	.095	.242	.049	.051	-.005	.281	.026	.006	.035	.092	.098	
$\bar{X}_C$	5.74	1.90	8.54	15.30	51.97	156.82	5.35	38.11	15516	31.98	2.12	15.89	5.15
$SD_C$	9.67	3.87	8.54	12.28	34.79	132.80	16.58	4.95	4673	42.93	4.00	9.34	6.57
$\bar{X}_{SD}$	1.56	0.25	12.92	12.95	37.25	103.85	7.89	17.50	24165	7.30	11.10	16.65	3.97
$SD_{SD}$	2.90	0.81	15.25	15.37	30.51	124.58	20.87	3.63	9281	20.60	10.26	14.92	6.39

The correlations for Cleveland are above the diagonal, and those for San Diego are below the diagonal. The subscripts "C" and "SD" denote the means and standard deviations for Cleveland and San Diego.

Table 2. ZERO-ORDER CORRELATIONS, MEANS, AND STANDARD DEVIATIONS AFTER LOGGING

	1. Property Crimes	2. Violent Crimes	3. % Primary Individuals	4. % Overcrowded	5. Density	6. Black Population	7. % Units in Multi-unit Structures	8. Population Potential	9. Owner Value	10. % Black	11. % Spanish	12. % Over Age 60	13. Vacancy Rate
1.													
2.	.478												
3.	.201	.657											
4.	.154	.145	.254										
5.	.046	.168	.253	.195									
6.	.234	.055	.191	.210	.024								
7.	.227	.125	.075	.150	.090	.419							
8.	.167	.134	.226	.027	-.036	.336	.395						
9.	-.201	.127	.457	.027	.276	.322	.368	.433					
10.	.231	.127	.282	.027	.207	.408	.418	.439	-.238	.440			
11.	.155	.196	.037	.429	.033	.146	.086	.176	-.274	.490	-.084		
12.	.034	.020	.580	.465	-.020	.120	-.013	.162	-.164	.207	-.097	-.086	
13.	.171	.141	.379	.065	-.107	-.204	.238	.312	-.136	.140	.075	-.344	
$\bar{X}_C$	1.42	0.66	1.98	2.33	3.76	4.78	.51	3.66	9.60	1.73	0.78	2.67	1.30
$SD_C$	0.96	0.80	0.74	1.17	0.71	0.79	1.21	0.14	0.39	2.11	0.73	0.61	1.05
$\bar{X}_{SD}$	0.60	0.13	2.03	1.82	3.39	4.34	0.63	2.89	10.03	0.67	2.24	2.47	1.01
$SD_{SD}$	0.74	0.35	1.19	1.45	0.76	0.77	1.40	0.23	0.35	1.32	0.69	1.00	1.07

The correlations for Cleveland are above the diagonal, and those for San Diego are below the diagonal. The subscripts "C" and "SD" denote the means and standard deviations for Cleveland and San Diego.

Table 3. DOUBLE-LOG MULTIPLE REGRESSIONS

Independent Variables	Cleveland				San Diego			
	Property Crimes		Violent Crimes		Property Crimes		Violent Crimes	
	beta	b	beta	b	beta	b	beta	b
<u>Family Status</u>								
% Primary individuals	.125*	.162*	.127*	.136*	.236*	.146*	.150*	.044*
<u>Environmental Variables</u>								
% Overcrowded	.016	.013	.060*	.041*	.016	.008	.022	.005
Density	-.331*	-.450*	-.184*	-.207*	-.063*	-.061*	-.006	-.003
Block population	.439*	.537*	.271*	.274*	.231*	.224*	.089*	.041*
% Units in multi-unit structures	.179*	.143*	.170*	.112*	.094*	.050*	.054*	.014*
Population potential	.270*	1.914*	.184*	1.080*	.078*	.247*	.032*	.049*
<u>Control Variables</u>								
Owner value	-.101*	-.247*	-.081*	-.165*	-.010	-.021	-.037	-.037
% Black	.177*	.080*	.300*	.113*	.176*	.099*	.223*	.060*
% Spanish	.047*	.063*	.045*	.049*	.025	.027	.051*	.026*
% Over age 60	-.093*	-.146*	-.027	-.036	-.069*	-.051*	-.029	-.010
Vacancy rate	.097*	.089*	.096*	.072*	.025	.017	.036*	.012*
R =	.734*		.679*		.429*		.364*	
R <sup>2</sup> =	.539*		.461*		.184*		.133*	

\*Significant at the .05 level. Significance tests are reported as a rough means of distinguishing high and low coefficients.

measure in each city, the variance explained by the double-log regression is greater than that explained by the ordinary regressions.

Table 3 contains an anomalous finding. Most of the environmental effects indicate that higher levels of population or housing concentration mean more crime. Yet, the effect of density is negative and statistically significant in three equations. For Cleveland, this negative effect is very strong and means that lower density blocks have more crime. This finding is not a statistical artifact. The correlation matrices do not contain significant collinearity. The unique variances of density are statistically significant in these three equations, and they are substantial for Cleveland. This finding requires explanation because it is difficult to understand how a larger resident population can mean more crime, while a smaller density also means more crime. A block with a large population can have a low density only if the area of the block (the number of acres it occupies) is large.

The double-log regressions in Table 4 permit examining the effect of replacing density by the area of the blocks.<sup>17</sup> Area has positive and significant effects for the three equations in which density has significant negative effects. The larger the physical size of a block, the more crime it has.<sup>18</sup> The effects of area in Table 4 are positive because they are no longer concealed in the denominator of density and where they produced density's negative effect.<sup>19</sup> The findings for the environmental variables are now clearer. The larger the size of a block's population, representing the availability of victims,<sup>20</sup> the higher is the frequency of crime. The larger a block's physical size, representing the availability of places for criminal activity, the higher is the frequency of crime. The negative effect of density on crime, when controlling for block population, is due to the effect of the physical size of the block.

Table 5 presents the final set of regressions omitting variables from the regressions in Table 4.<sup>21</sup> To avoid the problems of interpreting the effects of density, these regressions contain area rather than density. The results in Table 5 indicate that the three major hypotheses identify important effects for both crime types for both cities. The percentage of primary individuals has an important effect in every regression. Even after eliminating variables, some of which have statistically significant effects, every regression contains more than one environmental variable. Finally, these double-log regressions still explain more variance than the ordinary multiple regressions. The signs of the coefficients of the major variables are consistent for the corresponding equations for each city, even though the strength of the effects does vary. For example, area is more important than block population for Cleveland, while the reverse is true for San Diego. In general, the hypothesized effects are more important for Cleveland than for San Diego, and they are more important for property crime than for violent crime. Yet, both property and violent crimes in each city depend on

Table 4. DOUBLE-LOG MULTIPLE REGRESSIONS USING AREA INSTEAD OF DENSITY

Independent Variables	Cleveland				San Diego			
	Property Crimes		Violent Crimes		Property Crimes		Violent Crimes	
	beta	b	beta	b	beta	b	beta	b
Family Status								
% Primary individuals	.126*	.163*	.128*	.138*	.230*	.142*	.149*	.044*
Environmental Variables								
% Overcrowded	.016	.013	.060*	.040*	.015	.008	.022	.005
Land area (acres)	.360*	.560*	.193*	.249*	.058*	.056*	.007	.003
Block population	.108*	.132*	.091*	.092*	.186*	.180*	.084*	.039*
% Units in multi-unit structures	.171*	.136*	.166*	.109*	.094*	.050*	.054*	.014*
Population potential	.268*	1.898*	.181*	1.061*	.077*	.245*	.033*	.049*
Control Variables								
Owner value	-.098*	-.240*	-.079*	-.161*	-.010	-.020	-.037	-.037
% Black	.179*	.081*	.302*	.114*	.176*	.099*	.223*	.060*
% Spanish	.039*	.052*	.040*	.044*	.029	.031	.051*	.026*
% Over age 60	-.085*	-.134*	-.023	-.029	-.062*	-.046*	-.028	-.010
Vacancy rate	.094*	.086*	.094*	.071*	.024	.017	.036*	.012*
R =	.735*		.678*		.427*		.364*	
R <sup>2</sup> =	.541*		.460*		.183*		.133*	

\*Significant at the .05 level.

the household composition, features of the residential environment, and the interactions of the social composition and the residential environment.

Of the environmental variables, block population and percent multi-unit have strong enough effects to remain in every trimmed equation. Population potential and area (or density) remain in all equations except one (violent crime, San Diego). The percent overcrowded is the least important environmental variable for explaining crime because its only important effect is for violent crime in Cleveland. The environmental variables have more important effects for Cleveland than for San Diego and more important effects for property crime than for violent crime.

### Discussion

For both types of crime in each city, the concentration of primary individuals, the block population, and the concentration of apartment housing have strong effects. Population potential and area have important effects for both types of crimes for Cleveland, and weaker effects for property crime in San Diego. Although the percent black is a control variable, it also has strong effects, but these effects do not dominate the regressions in the same way as in studies using data for census tracts or larger units of analysis.<sup>22</sup> From this research, the most dangerous city blocks are relatively large in population and area with high concentrations of primary individuals and apartment housing. These blocks also tend to be in heavily populated surroundings.

The results are consistent with linking the features of residential areas to crime through anonymity. Each of the variables which is central to the research hypotheses can affect the amount and type of contact in residential areas. For example, the activity patterns of primary individuals and the lack of children in these households are likely to decrease residents' knowledge of, interaction with, and concern for each other as the concentration of these households in areas increases. Low levels of knowledge, interaction, and affect are consistent with the anonymity interpretation. Similar processes may operate in areas with apartment housing. Newman argues that as the number of households sharing common living space increases, residents are less able to recognize their neighbors, to be concerned for them or to engage in guardianship activities. The size of a block's resident population and its area can affect contacts among residents and their ability to detect or distinguish potential offenders. Larger populations and areas may decrease contact and interaction among neighbors. On physically large blocks, events in one part may be of little concern to those using a different part.<sup>23</sup> The concentration of people in the surroundings of a block may increase the number of potential contacts there.

Table 5. TRIMMED DOUBLE-LOG MULTIPLE REGRESSIONS

Independent Variables	Cleveland				San Diego			
	Property Crimes		Violent Crimes		Property Crimes		Violent Crimes	
	beta	b	beta	b	beta	b	beta	b
<u>Family Status</u>								
% Primary Individuals	.126*	.164*	.115*	.124*	.237*	.147*	.159*	.047*
<u>Environmental Variables</u>								
% Overcrowded			.070*	.047*				
Land area (acres)	.355*	.552*	.188*	.242*	.059*	.057*		
Block population	.114*	.140*	.090*	.091*	.194*	.188*	.105*	.049*
% Units in multi-unit structures	.170*	.135*	.171*	.113*	.098*	.052*	.059*	.015*
Population potential	.272*	1.926*	.185*	1.087*	.085*	.268*		
<u>Control Variables</u>								
Owner value	-.107*	-.262*	-.087*	-.177*				
% Black	.159*	.072*	.284*	.107*	.199*	.112*	.242*	.065*
% Spanish							.085*	.044*
% Over age 60	-.095*	-.149*	.099*	.075*	-.066*	-.049*		
Vacancy rate	.100*	.091*						
R =	.734*		.677*		.425*		.359*	
R <sup>2</sup> =	.539*		.458*		.181*		.129*	

\*Significant at the .05 level. The variables included in each regression have significant coefficients with beta weights greater than or equal .05 in Table 4.

Baldassare shows that residents in dense census tracts ignore many casual encounters in their areas.

Each of the major variables can provide opportunities for crime by affecting detection probabilities, but these findings suggest that a more social process operates to produce crime than the simple equating of opportunities to crime. The increase of approximately 20 percent in the explanatory power of the independent variables when using double-log regressions indicates the importance of the interplay between the types of residents and their environments. Since victimization, except for residential burglary, does not always occur on an individual's block of residence, these results suggest that the social composition and environmental features combine to affect the probabilities of victimization for any individuals using the areas. For example, the effects of primary individuals and apartment housing in the double-log regressions indicates the importance of their interactive effects and parallels Zorbaugh's concern with the problems of informal social control and anonymity in rooming-house areas.

The effects of the size of the blocks and their populations support much of Newman's and Jacobs' reasoning about the dangerous parts of cities. Newman describes the severe problems in large housing projects, while the safe parts of Boston and New York that Jacobs discusses have small blocks and walk-up apartment buildings. The negative effect of density on crime is also consistent with her arguments, but this finding results from the complex nature of density as a variable. The major differences between her work and these findings is that here it is shown that a higher population potential increases crime. Increased usage may produce safety under certain conditions, when it is purposive or when there is social integration among the residents as in the areas she describes. Otherwise, usage produced by dense surroundings results in more crime. Nevertheless, the power of these results, especially for Cleveland, and the importance of the environmental variables provides support for their extensions of Wirth's and Simmel's concerns about the effects of the urban environment.

The hypotheses identify similar effects across crime types. The major difference between the results for the two crime types is that environmental effects are more important for property crime than for violent crime. The weaker effects for violent crime may result from the importance of interpersonal relationships for these crimes. Murders, rapes, and assaults often occur among people who are acquainted with each other. Violent crimes also involve an element of spontaneity that makes the physical setting a secondary consideration (Block).

The results also support the hypotheses for each city. The directions of the most important effects are the same, but the strengths of the effects differ. The environmental variables are far more important for Cleveland than for San Diego. While the ideas of Wirth, Simmel, Newman, and



Jacobs refer to the effects of the urban environments in older, industrial cities, these findings suggest that similar processes operate in both cities. Yet, the discrepancies require explanation. For crime, there are two major differences between Cleveland and San Diego. First, the overall prevalence of crime is higher in Cleveland than in San Diego. Second, the different results for the two cities indicate that the distributions of crimes are different. Explaining the first difference may require considering the conditions which produce criminal motivations. San Diego has a relatively affluent population, fewer minority residents than Cleveland, a newer and more desirable housing stock, as well as accessible recreational areas. The combination of these features of San Diego can produce less pressure for developing criminal motivations, and thus less crime. Finally, regardless of the statistical technique used, it is always more difficult to predict a dependent variable with a relatively small variance, that is a rare distribution.

The differences in the results are apparent from the regressions for each city and there is little else that can be done statistically. Including a dummy variable identifying the cities in a double-log regression which simultaneously uses all the data for all the blocks in both cities indicates that the differences between the cities are significant without indicating the causes.<sup>24</sup> The most revealing attempt to uncover the cause of the differences emerged from examining the most dangerous blocks in both cities and the crime patterns of different types of blocks. In both cities, the most dangerous blocks could be described as slum blocks. In Cleveland, these blocks tend to be large in area and in population; but in San Diego they are often physically small with relatively few residents. The physically large blocks in San Diego have varying characteristics. Although statistical analysis cannot demonstrate this suggestion, part of the differences seem to be due to the less frequent coincidence of crime-producing conditions on San Diego blocks. Blocks with high values for some crime-producing variables often have low values for others and thus the combination of conditions generating high crime levels in Cleveland does not seem to occur as frequently in San Diego. The relative scarcity of large blocks in Cleveland and their association with other crime-producing conditions may partially account for the importance of area in Cleveland. The greater prevalence of large blocks in San Diego with their varying characteristics may diminish area's effects there and allow the effects of population to be more evident.

This research leads to three major conclusions. First, conditions conducive to anonymity are crucial for understanding where crimes occur. The effects of primary individuals and of the environmental variables which can affect the amount and type of contacts among residents support this proposition. Second, the environmental features of residential areas are important for explaining where crimes occur. The findings suggest that changes in the environments of residential areas can affect crime. The

effects of such changes will not be dramatic because increasing the safety of the environments of some areas will not only prevent crimes, but also will displace them to other areas, especially crimes not dependent on interpersonal relationships. Third, the patterns of crime are complex and the interactions of the environment with resident characteristics need to be taken into account. This conclusion parallels Newman's arguments that the kinds of households living in different environments matters.

These analyses can inform future work clarifying the processes by which parts of the city become dangerous places. The discussion links crime with anonymity based on research using city blocks. The strength of this approach is its extensivity; every residential block in each city is in the analysis. Yet, this link needs further specification by intensive analyses of high and low crime areas. Since most urban residents are integrated into social networks and are unlikely to feel anonymous or alienated (Baldassare), research on these sentiments may not be productive. Research focusing on crime and examining Newman's propositions about how people use different areas may be more appropriate. Observational studies, longitudinal studies, and studies including additional features of residential areas such as the presence of nonresidential uses would be valuable. Additional research on the production of offense data in different types of areas of the city would be useful. These results identify effects to which the designs of such studies should be sensitive.

## Notes

1. Although the percentages vary by type of crime and are affected by apprehension rates, Pyle et al. (54-55) show that relatively few offenders live on the blocks where they commit their offenses. While the percentages are higher for victims, many become victims elsewhere than on their block.
2. Both juvenile delinquency and adult offender studies have been extensively criticized (Hagan et al.) because they depend on the apprehension of an offender. Reiss, however, argues very persuasively that the biases in this research are not as severe as critics contend and that there are differences in the rates of offending and incidence of crime in different parts of the city. Studies of offense patterns depend on notification of the police and police recording. Skogan (a) shows that nonreporting is more important than police recording biases and that unreported crimes are less serious than reported crimes. In another article, Skogan finds that while nonreporting affects the volume of recorded crime, it does not appear to affect the "... distribution of known crime across many social and behavioral categories" (b, 49). He finds that both blacks and whites have very similar rates of reporting crimes. If the differences across such an important social distinction are minute, then it is unlikely that differences across less fundamental characteristics of individuals or of their residential areas has much effect on reporting rates. His findings suggest that if analyses are confined to serious crimes, as in this analysis, then inferences can be made about the distribution of crimes. See, also, Hindelang.
3. While people must choose to engage in such behaviors, the actions of one person probably have little effect on the amount of crime in an area. More likely, it is the total pattern of activities in an area which affects its crime experience, although ultimately these derive from the activities and decisions of separate individuals. The characteristics of residents and users of an area as well as those of the environment may affect individuals' decisions to undertake such behaviors (Newman, a,b). For example, Gove et al. claim that crowding in the home

increases antagonism and conflict with neighbors. While these attitudes and behaviors bear more on criminal motivation than on the crime experience of an area, they are not likely to foster the protective actions which neighbors can take for each other in the presence of a potential offender.

4. Besides affecting crime through the interactions in an area, or anonymity in Wirth's terms, each of these variables may also affect the availability of victims (Booth et al.). The number of people in an area, resident or nonresident, constitute a set of potential victims who represent opportunities for crime. Similarly, apartment housing provides many units in a limited area and these also represent opportunities for crime, especially if identification of individuals is difficult in such places.

5. Newman also argues that the design of housing can counteract many of the undesirable effects of the type or size of apartment housing.

6. Roncek (b) shows that the characteristics of census tracts do not adequately account for the variation in crime across blocks when compared to analyses using block-level characteristics to predict block-level crime.

7. The Census Bureau does not report income or occupation for blocks. The only direct measures of economic status for blocks are the average values of housing. The major reason for using the value of owned housing as the indicator of economic status is that banks and mortgage companies will not allow purchasers of housing to overallocate their incomes to housing. Therefore, the value of owned housing will correspond more closely to income than rent. To prevent the loss of cases, the value of owned housing in the tract in which a block is located replaces the block value when it is missing. This substitution is more accurate than replacement by the city mean because blocks within the same tract are similar to each other. Replacing by the tract mean allows the measure to vary for blocks with missing values.

8. This variable is actually the percent of Spanish-language persons in the census tract in which a block is located. The Census does not report the percent Spanish for blocks. Using this variable permits controlling for effects due to the presence of this minority which is prominent in San Diego.

9. The Census does not report how many buildings or structures of different types are in an area. It reports the number of units in structures of different types. For blocks, the number of units in ten-or-more unit structures is the largest reported category of units in structures. Thus, it is impossible to tell exactly how large the apartment buildings on a block are. The same problem exists for census tracts.

10. Quadratic forms of population potential expressed in persons per square mile were also examined, but they had virtually no effect.

11. With respect to each other, these measures can vary in ways that are not possible for census tracts or larger units of analysis because each city has many more blocks than tracts. Blocks with large populations need not be overcrowded or dense, nor must they have most of their housing in apartment buildings. A block with most of its residents overcrowded does not have to be dense if the population of the block is small or if the block has few housing units. Blocks with large apartment buildings may not have high densities or large populations if there are few people per housing unit. The population potential of a block does not depend on any population or housing characteristic of the block itself (see Choldin and Roncek).

12. Including the vacancy rate permits examining if a high proportion of vacant units affects criminal opportunities by increasing the space available for criminal activity.

13. The distributions of primary individuals, overcrowding, apartment buildings, and vacancy rate resemble negative exponential distributions. All the variables except population potential are positively skewed. Logging the independent variables handles the problem of skewness.

14. Because the logarithm of zero is not defined and several variables have values of zero, the constant of one was added to each value of each variable before logging. Adding this constant has no effect on the regressions because it does not affect the correlations among the variables. The regression coefficients indicate the importance of each of the variables. Further elaboration requires evaluating transformed partial derivatives at different values of the variables. Space limitations preclude discussing these effects. See Stolzenberg or write the author for details.

15. Because the coefficients of the ordinary regressions are not central to any of the hypotheses, they are not presented, but are available from the author.

16. The double-log regressions explain more variance than semi-log regressions in which only the dependent or independent variables are in logarithms.
17. Using the Haitovsky test on the correlation matrices containing area instead of density shows that significant collinearity is not present.
18. The average city block in Cleveland is approximately 4.4 acres. In San Diego, it is approximately 5.8 acres. Both cities contain many rectangular blocks and the major difference is that San Diego contains more relatively large blocks. The average size of blocks in both cities is quite similar to blocks in Chicago which are approximately 5 acres.
19. The logarithm of the reciprocal of a variable,  $\ln(1/X)$ , equals  $-\ln(X)$ .
20. Of course, block population only partially represents the availability of victims because, except for burglary, victims can reside in places other than the block where a crime occurs.
21. The regressions in Table 5 omit variables with beta weights less than .05 in Table 4. The large number of blocks in each city permit very small regression coefficients to be statistically significant. Using a more stringent criterion would lower the explained variance even further for San Diego and would make comparisons across the cities difficult.
22. Using Census tract data for Cleveland, Roncek (b) shows that ignoring all variables other than the percent black does not result in a dramatic loss of explanatory power, while ignoring other variables using block data dramatically reduces the power of the equations.
23. Verbrugge and Taylor find that the social ties among residents increase with the population of the block rather than decrease, but differences in methodology may account for the contrasting results. The effect of population on crime results from its interaction with other variables including primary individuals and apartment housing. Perhaps, on blocks with high values of all these variables, social ties are less frequent than elsewhere. If the large populations in the areas studied by Verbrugge and Taylor were due to households with children, then more social ties among residents might be expected.
24. These results are available from the author on request.

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# Age and Differential Predictability of Delinquent Behavior\*

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## ABSTRACT

*This paper views delinquent behavior as a defense against low self-esteem brought about by poor performance in school. Defensiveness is operationalized as a discrepancy between scores on measures of conscious self-esteem (high) and unconscious esteem (low). A representative national sample of 720 boys was divided into two age groups: 11–14 years, 15–18 years. Among the older group, successive interaction hypotheses predicting increasing levels of delinquent behavior were supported. The result was a predicted high delinquency defensive group whose mean delinquency score was at the 86th percentile of the population distribution. The hypotheses were not supported in the younger age group. Differences in accumulated social experience in school are suggested as accounting for the difference between age groups.*

The perspective to be evaluated here treats delinquent behavior as the result of influences in the social environment of adolescents interacting with common psychological characteristics of individuals. It is a motivational perspective, asserting that the explanatory link between performance in the student role and involvement in delinquent behavior operates through individual motivation. It is also an interactive perspective. As such, it asserts that sole reliance on role performance or on individual attributes will result in less accurate predictions of delinquent behavior than if both are taken into account.

In speaking to the questions of why and how experiences produce delinquent behavior, this approach is an alternative to structural, control, and labeling perspectives. The cross-sectional data to be presented below preclude a thorough evaluation of the merits of these different viewpoints.

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Nevertheless, they do provide findings consistent with the point of view briefly sketched above.

Note that self-reported delinquent behavior is the behavior of interest. Official delinquency—reflecting the relatively rare contact with formal agents and agencies—is not dealt with here.

This study sets delinquent behavior in the context of adolescence. Therefore, implications of adolescents' experiences in the common and salient social environment of school receive particular emphasis.

### Conceptualizing Delinquent Behavior

Delinquent behavior is here considered the result of a defensive strategy. Its object is to ameliorate the denigrated self-image which results from poor performance in school (e.g., Kaplan,<sup>a</sup>). Academic performance is seen as heavily symbolic and normatively salient for adolescents, whether as a function of the restricted role alternatives open to them, acceptance of grades as a measure of self-worth, implications of present performance for future status, the developmental necessity of earning status in progressively impersonal school settings, or as some combination of these and like factors (Coleman et al.; Douvan and Gold; Polk and Schafer; Rhodes and Reiss; Rosenberg,<sup>a,b</sup>; Vinter and Sarri). The importance of school to adolescents has been affirmed by heavily delinquent actors (Short), those who have fared poorly in their scholastic endeavors (Gold,<sup>a</sup>; Rosenberg,<sup>a</sup>), and those of different socioeconomic strata (Schafer and Polk).

#### DEFENSIVENESS

Delinquent behavior is seen as part of a defensive strategy involving the attempt to restore self-esteem and recast the social environment. The use here of the term "defensive" differs importantly from the sense in which it has been employed by others in the field. Theorists from Cohen to Lemert use the term to mean a rejection of or flight from putatively unattainable ends or derogatory attributions. In either instance, the values and prescriptions of the regnant culture are simply sloughed off in the service of self-esteem. This action is accounted for by defensiveness (and in turn accounts for further infractions). A more careful definition of the term has implications which suggest that matters are not so straightforward.

Fundamentally, defensiveness involves consequences at two levels. Defensiveness implies denying or otherwise suppressing that which is threatening or produces anxiety. A successfully defensive individual feels less threatened and anxious; self-confidence and self-esteem are restored. But, defensively gained high self-esteem is not free.

Things that threaten and produce anxiety—goals, values, activities,



associations—would have to be rejected in order to avoid the noxious state which first prompted the defense. Were this the only cost to individuals, it would be relatively low. But (as noted by Cohen with particular reference to school and more generally by Matza), the result of a lifetime of socialization and inculcation of cultural values is not simply and totally dropped when no longer convenient or comfortable—even if the lifetime spans only 14 years. The internalized values and norms remain, although unconscious and suppressed. The cost of defensive rejection thus accrues below awareness as depressed unconscious self-esteem. Continued defensive behavior—delinquency—is required to maintain high conscious self-esteem. This is because even a successful defense does not *solve* the problems which result in depressed self-esteem. The defense only suppresses the problems at a conscious level for as long as the defense is used (or until a change in developmental status obviates the problems). Under these circumstances, low unconscious self-esteem is an index of continued commitment and attribution of legitimacy to those conditions responsible for the initial insult to self-concept and self-esteem.<sup>1</sup>

Of course, if delinquency were not a successful defense, there would be no association between self-esteem patterns and delinquent behavior. But if this characterization of delinquent behavior as defensive is accurate, the more delinquent actors should be high in conscious self-esteem but low in unconscious self-esteem. For convenience, this pattern will be referred to as the defensive signature.

While this is a larger dose of psychological analysis than usually found in the sociological literature, if one takes seriously the interaction of social and individual issues, one must at some point discuss individuals and the influence of social effects on them and their subsequent behavior (e.g., Hirschi and Hindelang; Wells).

#### THE SOCIAL ENVIRONMENT

For adolescent males, especially in late adolescence, the student role is critical to the consolidation of identity (Erikson; Turner) and earning current and anticipating adult status (Davis; Lidz; Polk and Schafer; Rhodes and Reiss; White). As noted above, the importance of school is recognized even in the face of one's failure. Indeed, it is unique among attributes in maintaining the value ascribed to it independent of personal performance (e.g., McDill and Coleman; Rosenberg,<sup>a,b</sup>; Short and Strodbeck). It is no surprise that the widely valued attribute is associated with one's social value or status. The social ranking function of education is anathema to some educators but it is a common element in the experience of high school (e.g., Vinter and Sarri). Its implications for anticipated adult status and mobility are clearly understood by the adolescent (see Davis; Gold,<sup>a</sup>; Kenniston, Polk and Schafer).

The salience of the school experience and the student role is further underscored by the unusual societal position of the adolescent. Though fully equipped to discharge the responsibilities and assume the privileges associated with adulthood, the adolescent's sphere of recognized and significant social performance is largely limited to the student role (Coleman et al.; Douvan and Gold). The constricted role environment and concomitant restriction in the status structure has been poignantly noted by Polk and Schafer:

. . . We encounter the hard reality that what schools do—regardless of the educational rhetoric—is to process individuals differentially and thereby affect very directly their life chances for success. In our present technologically oriented society, the school has become the gatekeeper of success . . . Yet, in addition to this future implication—doing well serves also to fix the adolescent's present identity within the setting of the school as well as in the eyes of his teacher, his fellow students, and himself (22–3).

The proponent of labeling theory might well point out that it is precisely the labeling mechanism implicit in graded school performance which accounts for the blows to self-esteem considered propaedeutic to delinquent behavior. Note, however, that the term labeling denotes events much more public than receiving a sealed report card (Becker; Gibbs and Erickson; Lemert; see also Meade). Public disclosure of grades is up to the student. Even then, students' public reactions can be at variance with their private, though shared view that grades *are* important (McDill and Coleman).

The term self-labeling might be employed, though this contradicts a singular tenet of the labeling and reactivist perspective. Norms and their necessarily shared expectations and evaluative standards are severely de-emphasized in the explanatory lexicon of labeling (Becker; Gibbs and Erickson; Gove; Lemert; Schrag). Thus, normative standards are irrelevant to the etiology of delinquent behavior. Instead, it is asserted that the relative social characteristics of actor and audience define the contingencies which lead to labeling. Were this actually the case, performance in school would not be consensually perceived. It would thus not produce threats to self-esteem with sufficient consistency to be useful in the explanation of a general phenomenon. Yet, it is adolescents' shared normative standards and the implications for self-evaluation which make performance in the student role crucial in the etiology of delinquent behavior (e.g. Kaplan, b; Phillips and Kelly).

Not all boys characterized by the defensive signature of self-esteem are equally likely to be heavily involved in delinquent behavior. Experience in school is the crucial mediating factor in the relation of self-esteem and delinquent behavior: those boys characterized by the defensive signature

of self-esteem *and* by poor performance in and negative attitudes toward school should be more highly delinquent.

Increasing degrees of delinquent behavior bespeak increasing rejection of school, the cause of low self-esteem. Delinquent behavior provides an alternative arena for performance and achievement. At the same time, it represents rejection of the means and values espoused by the school. Viewed from this perspective, delinquent behavior is a well-suited, sophisticated defense against denigrating experiences in school—a point clearly made by Cohen 25 years ago. Consistent with this view are the findings that on leaving school delinquent behaviors decline sharply (relatively and absolutely) among dropouts—those for whom the provocative experiences of schooling have ceased—compared to their still-enrolled peers (Elliott; Elliott and Voss).

Relying solely on psychological predictor variables—self-esteem scores—was expected to produce weak predictions of delinquent behavior. Experience in the social environment was represented by introducing school attitude and performance as moderator variables. By this strategy, initial predictions of high delinquency based on self-esteem scores only could be refined. The final classification of respondents and predictions as to level of delinquent behavior was thus based on a serial set of necessary conditions designed to successively refine intermediate steps.

This model obviously implies a causal temporal sequence of experience which leads to increasing likelihood of high levels of delinquent behavior. This sequence might be summarized as: (1) failure in the impersonal, bureaucratic, and performance oriented environment of secondary school, resulting in; (2) depressed self-esteem, both conscious and unconscious, which stimulates; (3) the defensive rejection of school and substitution of delinquent performance, resulting in; (4) an increase to high levels of conscious self-esteem but continued low unconscious self-esteem—the defensive signature. An equally obvious implication is that this temporal model (steps 1 – 4) cannot be fully tested using the present cross-sectional dataset. That does not mean that the model cannot be tested here at all, however. The predictions whose tests are reported below represent steps 3 and 4 in the model. These predictions could be confirmed even if the causal sequence were different, but disconfirmation of the present hypotheses would cast serious doubt on the validity of the model. Thus, the hypothesized static relationships must minimally be demonstrated to withstand disconfirmation in order for the model to be valid.

## Methods and Procedures

The sample consisted of 720 boys selected in a nationally representative household sample of American adolescent males. Respondents ranged in age from 11 to 18 years when interviewed in the spring of 1972. (See Gold and Reimer for a description of sampling and interviewing procedures.) The variables used in the study were operationalized in the following manner.<sup>2</sup>

A measure of conscious self-esteem was derived from responses to two identical sets of 14 bipolar semantic differential scales selected for their rather transparent relevance to self-conception (e.g., sturdy–easily injured; slow–quick; brave–timid). The first set was introduced as a self-descriptive task. The second set was introduced as a description of one's ideal, or desired self. The absolute value of the item by item discrepancies was summed and used as the conscious esteem score with size of discrepancy the inverse of degree of esteem.

The measure of unconscious self-esteem was the Social Self-Esteem test developed by Ziller (Long et al., a,b; Ziller and Golding; Ziller et al., a,b). Its use is supported by extensive empirical work (above) in which, for example, it has distinguished sociometric stars from isolates, winning from losing candidates for political office, high- from low-status caste members (see also Mann,b).<sup>3</sup>

Attitude toward school was measured by an index of two Likert items asking, for self and for self compared to peers, how much the respondent liked school. Grade point average from the previous marking period provided a measure of academic performance. In most cases, grades were reported by the respondent's school; when not available, a self-report was used. (Among respondents for whom both reports were available, the two correlated highly,  $r = 0.85$ ,  $p < .001$ .)

These four variables—conscious esteem, unconscious esteem, school attitude, and grade point average—were the classification factors used. For analysis, each classification variable was split at its median into high and low levels. The classification variables were fully crossed, within which the defensive signature group was defined: those above the median on conscious self-esteem but below the median on unconscious self-esteem.

The measure of delinquent behavior was that developed by Gold (b). It is based on a detailed, self-reporting personal interview. Up to three instances of each of 14 types of criminal and juvenile offenses in the past three years are followed up by the interviewer. Specific information is elicited about the exact nature, location, time, date, companions, disposition of property involved, or other outcomes. In order to provide a more conservative measure of delinquent behavior, acts judged trivial were excluded. Nontrivial acts confessed were tallied to yield the delinquent be-

havior score.<sup>4</sup> It should be noted that 85 percent of the reported acts were said to have occurred within a year of the interview.

Socioeconomic status of the head of respondent's household was assessed as a continuous variable in keeping with conventional procedures (Blau and Duncan; Duncan; Gold and Reimer).

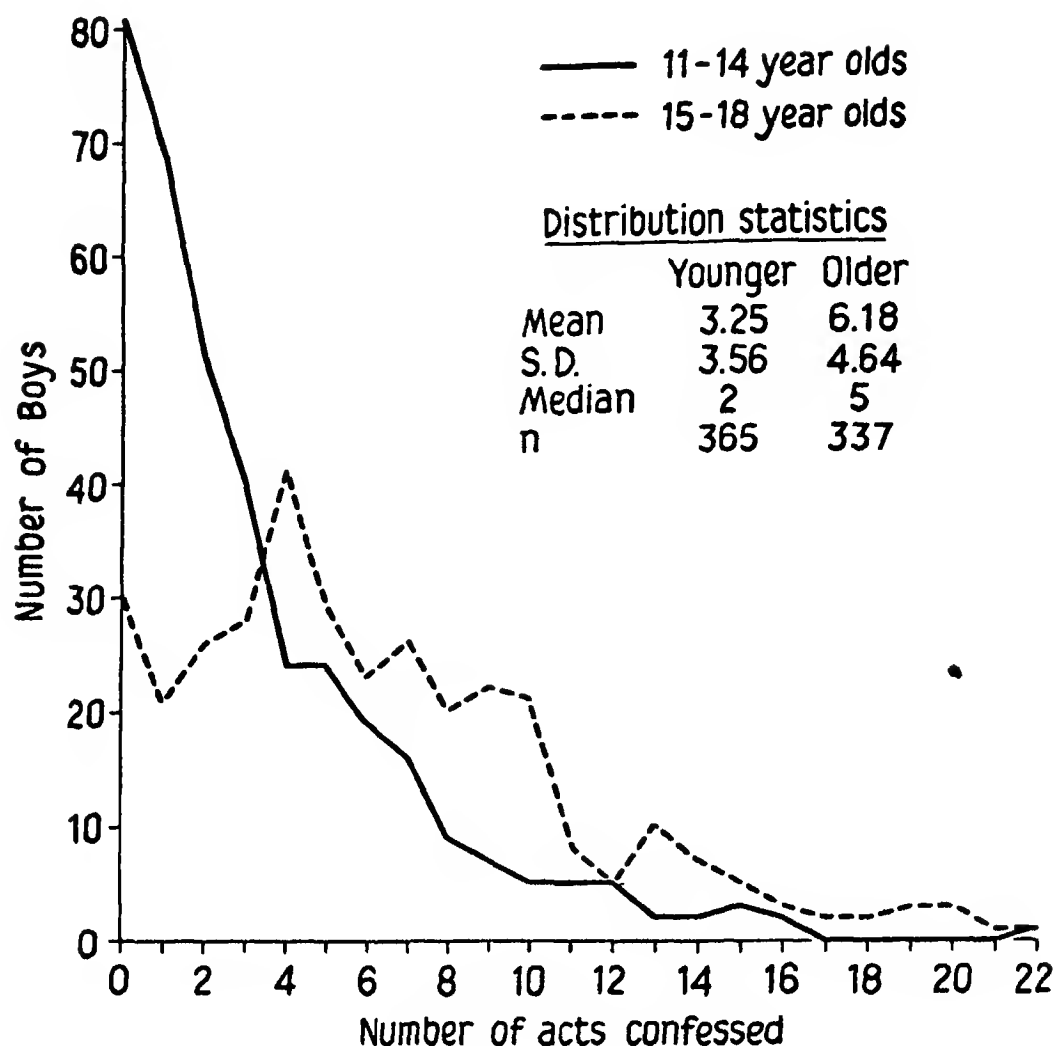
Analysis of the operating characteristics of several of the measures in a comparable sample revealed a monotonic relationship between the measurement scales' reliabilities and age of respondent (Mann, a). In consideration of these findings, it was decided to partition the current sample by age. A younger (11–14 years) and an older age group (15–18 years) were formed, and identical though independent tests of the hypotheses carried out within each group. Though the impetus to the partition was empirical, it was not without heuristic implications, to be considered later.

The study design called for a 2<sup>4</sup> analysis of variance of extent of self-reported delinquent behavior. As indicated, four dichotomized classification variables were used—conscious self-esteem, unconscious self-esteem, school attitude, and grade point average. There were 2 × 2 × 2 × 2 classifications of delinquent behavior. Hypotheses of differences in mean delinquency between groups were derived directly from the theoretical model, and thus were tested by orthogonal planned comparisons within the analysis of variance (Snedecor and Cochran).

## Findings

Figure 1 presents the frequency distribution and distribution statistics for self-reported delinquent behavior among the older and younger age groups. The distributions reflect age accelerated prevalence of involvement in delinquent behavior; only the younger boys' distribution approximates the J curve of conforming behavior. The total number of delinquent acts reported by older boys was about 25 percent greater than the total for younger boys, and the mean number of reported acts per boy was significantly greater in the older group ( $t_{(700)} = 9.44, p < .0001$ ).

Age has not played nearly so central a part in the delinquency literature as has socioeconomic status. The contribution of SES to the variance in delinquency was assessed through covariance analyses applied to the 2<sup>4</sup> classification of delinquent behavior. In this instance, the age groups responded alike. The effect of social status was not significant, accounting for less than 1 percent of the variance in delinquent behavior within the classification schemes for each age group. SES adjusted delinquency means and their differences were configured as the unadjusted means. In sum, the unadjusted ANOVA model was found appropriate for the data.<sup>5</sup>



**Figure 1.** FREQUENCY DISTRIBUTION OF DELINQUENT ACTS 11-14 AND 15-18 YEAR OLDS

#### THE OLDER BOYS

The analysis of variance and cell means for delinquent behavior are presented in Table 1. As noted, the hypotheses were tested by planned comparisons of means within the analysis of variance.

The first hypothesis called for a comparison of the mean delinquency of the defensive group—those above the median on conscious esteem but below the median on unconscious esteem—with the remaining respondents considered as a group.

As shown in Table 2, the first hypothesis was supported: the mean differences represented roughly one delinquent act. The prediction that boys with the defensive signature would report more delinquent acts than

**Table 1.** DELINQUENCY BY SELF-ESTEEM AND SCHOOL BOYS 15-18\*

N	Mean Delinquency	Conscious Esteem	Unconscious Esteem	School Attitude	Grades
28	5.25			H	H
11	8.55	HIGH	LOW	H	L
15	5.93			L	H
11	10.27			L	L
17	5.71			H	H
15	7.73	LOW	LOW	H	L
18	4.78			L	H
20	7.25			L	L
27	3.81			H	H
17	6.18	HIGH	HIGH	H	L
20	6.50			L	H
21	5.67			L	L
21	5.81			H	H
13	4.23	LOW	HIGH	H	L
10	4.40			L	H
26	7.39			L	L

$$F(15,274) = 2.26, p < .005, \text{SSB} = 655.7; \text{SSW} = 5288.7, N = 290$$

\*Missing data account for differences in sample size between Figure 1 and Table 1.

their peers was accurate ( $t_{(274)} = 2.98, p < .005$ ). The higher mean value fell at the 66th percentile of the distribution of delinquency.<sup>6</sup>

As noted, incorrect classifications were expected to have occurred from classifying respondents solely by self esteem scores. The second hypothesis was designed to remove some of these falsely classified respondents. The reasoning was that boys who were defensive but who also held positive attitudes toward school were unlikely to have ostensibly rejected society's normative prescriptions. Therefore, they should have been less delinquent than the boys with the defensive signature and negative attitudes toward school. The pertinent mean comparison in Table 2 supports the hypothesis. Among defensive boys the mean delinquency level of those

**Table 2.** MEAN DELINQUENCY BY HYPOTHESIS BOYS 15-18

Hypothesis	Mean Delinquency	N	S.D.	Significance
1. Defensive signature	7.05	65	5.20	< .005
All others	5.84	225	4.30	
2. Defensive signature with low school attitude	8.35	24	6.11	< .08
Defensive signature with high school attitude	6.18	39	4.36	
3. Defensive signature with low school attitude and low grades	10.27	11	7.04	< .05
Defensive signature with low school attitude and high grades	6.93	15	5.12	

predicted to have been more delinquent—those with negative attitudes toward school—was greater than that of the competing group—those with more positive attitudes ( $t_{(274)} = 1.46, p < .08$ ). Moreover, the mean of the predicted high delinquency group represented an increase of 1.3 acts over the delinquency mean of the group identified as more highly delinquent by the first hypothesis. The increment brought the predicted high mean delinquency level to the 75th percentile. Simultaneously, the lower mean remained virtually unchanged from that of the low delinquency group in the first hypothesis—essentially the sample grand mean.

An inference permitted by the data thus far presented is that sole reliance on self-esteem scores incorrectly predicted some respondents as highly delinquent. Attitude toward school used as a moderator variable interacted with the defensive signature to refine the prediction of delinquency. This refinement is reflected in the higher mean delinquency score noted above.

The final hypothesis, which completes the statement of the model, indicated that prediction of delinquency using the defensive signature moderated by school attitude could be improved on by adding grade point average as a second moderator variable. That is, the boys characterized by the defensive signature and negative attitudes toward school could be further divided on the basis of grades, such that those with low grades would have a mean delinquency level significantly higher than those with high grade point averages. This is the full model with the third hypothesis predicting a third order interaction.

From Table 2, the group predicted by the third hypothesis to have a higher mean delinquency score clearly did so ( $t_{(274)} = 1.91, p < .03$ ). The



higher mean from the third hypothesis represents an increase of about two delinquent acts over the high delinquency group isolated by the second hypothesis. The predicted high delinquency mean ( $\bar{X} = 10.27$ ) fell at about the 86th percentile of delinquency.

It is interesting to note that among the defensive boys, even in the face of devaluation of school—if this can be inferred from a negative attitude toward school—good scholastic performance seems to have buffered the defensive impetus leading to extremely delinquent behavior (see Palmore and Hammond).

Further analyses were performed to evaluate the reasoning behind the order of inclusion of the moderator variables. That is, would the same magnitude of effect be seen had grade point average been used to stratify the defensive signature group prior to stratification on school attitude?

The answer is no, the same effect was not produced by reversing the order of moderators (Table 3). The unreliable mean difference (even by the somewhat generous criterion of a one tailed test) supports the weight of theoretical importance ascribed to school performance by this and other reports. School performance, when poor, does seem to act as a specific goad to delinquency when found in combination with the other factors increasing susceptibility to defensive behavior. It is not surprising (Table 3) that as predicted, grades themselves have a clear and substantial effect on mean delinquency levels ( $t_{(274)} = .283, p < .0025$ ).

Comparing Table 2 with Table 3 for a moment, grade point isolates a higher mean delinquent group than school attitude when each was used as a single moderator within the defensive group. But, as has been shown, using both variables as moderators produced the highest mean delinquency score as well as a clear separation of the competing means. This, of course, supports the interaction model postulated.

After analysis of the model's target cells—those subsumed by the defensive signature classifications—an analysis of variance of delinquency

**Table 3.** MEAN DELINQUENCY FOR ALTERNATE ORDERING OF MODERATOR VARIABLES  
BOYS 15-18

Alternative Hypothesis	Mean Delinquency	N	S.D.	Significance
3. Defensive signature with low grades and low school attitude	10.27	11	7.04	> .19
Defensive signature with low grades and high school attitude	8.55	11	5.72	
2. Defensive signature with low grades	9.41	22	6.32	< .005
Defensive signature with high grades	5.84	43	4.10	

in the remaining 12 cells was performed to test the general alternative hypothesis that no variance remained to be explained. The analysis of variance and pairwise mean comparisons were insignificant, which supported the conclusion that no reliable variance remained.

A summary of the findings indicates clear support for the model. The initial cross-classification of conscious and unconscious self-esteem produced significant mean delinquency differences. The introduction of school attitude and grade point average improved the prediction, as did their combination. The observed significance of the analysis of variance was accounted for by the target defensive group—those with high conscious but low unconscious self-esteem. Finally, the hypotheses tested resulted in identification of a group of respondents whose average delinquency level was at the 86th percentile of the distribution of delinquent behavior in the representative sample.

#### THE YOUNGER BOYS

Findings for the younger age group were strikingly different. The analysis of variance was significant (Table 4), though none of the hypothesized mean comparisons resulted in reliable differences. The most delinquent group of younger boys was that characterized by the negative end of all four classification variables.<sup>7</sup>

The main effects of all four classification variables were found to be in the expected directions, and all but that of unconscious esteem were significant. Nonetheless, the proposed interactive model failed to fit the younger boys' data.

#### Discussion and Conclusions

The principal conclusion to be drawn from the data is that the nature of delinquency seems to vary developmentally, and that the static relationships reported above support the model of delinquent behavior among boys in the second half of adolescence.

One explanation for the different findings by age groups is that the results for the older age group occurred by chance, which leaves nothing to be explained. Beyond pointing to other supporting research (Berman; Gold and Mann) and noting that replication is in progress, it can be said that the odds in favor of random findings of the complex, contingent nature reported above are low. A more likely explanation is that the difference is attributable to some unmeasured factor correlated with age. Age differences in school experiences and social maturation are probably candidates for the unmeasured, age correlated explanatory factors. The change from elementary to junior high school has been shown to be stressful, especially

Table 4. DELINQUENCY BY SELF-ESTEEM AND SCHOOL BOYS 11-14\*

N	Mean Delinquency	Conscious Esteem	Unconscious Esteem	School Attitude	Grades
31	2.87			H	H
17	4.76			H	L
16	3.69	HIGH	LOW	L	H
9	4.33			L	L
17	1.82			H	H
14	1.79			H	L
17	4.47	LOW	LOW	L	H
18	5.50			L	L
42	2.26			H	H
16	3.38			H	L
18	4.11	HIGH	HIGH	L	H
15	5.33			L	L
13	2.23			H	H
22	2.59			H	L
14	2.21	LOW	HIGH	L	H
37	2.97			L	L

$$F_{(15,300)} = 2.32, p < .005; SSB = 398.76; SSW = 3437.5; N = 316$$

\*Missing data accounts for differences in sample size between Figure 1 and Table 4.

in terms of disturbance in self-image and decrease in self-esteem (Piers and Harris; Simmons et al.). Beginning in junior high school, the school becomes increasingly formal and bureaucratic. Social position becomes increasingly based on impersonal standards and achievements. These are marked changes from elementary schools. For those who fare poorly in this environment, social rejection, low self-esteem and frustrated lashing out appear to characterize delinquent behavior.

The transition to high school, captured by the age group split, is much less abrupt. Adjustments made to the junior high school transfer and with increasing maturation, the more sophisticated defensive responses can develop. A larger and more generally approving group of peers is available within which to develop networks of support and alternatives to

the formal status and activity structures of the school (Trickett and Todd). With these alternatives, the conscious self-concept can recover from the shock of adjustment to the changed social environment first represented by the move to junior high school (Simmons et al.).

The data are in accord with other research reports of a decline in self-esteem at entry to junior high school followed by a recovery through the high school years. They suggest, however, that the recovery of self-esteem is not simple or complete when school performance remains poor. Instead, the formal schooling experience continues to affect self-concept and delinquent behavior. Recent attention has been given to the potential to be realized by adding microsociological or individual level variables to explanatory treatments of social phenomena. A more carefully developed view of self-concept and the notion of developmental position appear to be candidates for such consideration when seeking explanations below the aggregate level.

## Notes

1. The operation of defensiveness and the unconscious is analogous to formal analytic theory (Freud), but motivation springing from instinctual conflicts reemerging with adolescence is not implied here. Cognition, socialization, and expectations in social interactions provide the unconscious motivation which powers the proposed model.

2. See Mann (a) for a more detailed discussion of the operationalization and operating characteristics of the variables.

3. The measure is a projective one, asking respondents to exhaustively and uniquely assign a list of 5 positive or negative brief descriptions of people plus "Yourself" to a row, and then a column of 6 positions. Absolute distance in position units between "Yourself" and "a person you know who is unhappy" was used as the measure of unconscious esteem (as per Long et al., b), with greater distance representing greater self-esteem.

Although unusual, the measure has operated systematically in the past and behaved in accord with the construct assigned in the present application.

4. Interview procedures designed to maximize interviewer-respondent rapport and to assure respondents of the confidence in which their reports were held are described in Gold and Reimer, and Mann (a). Trivial delinquent acts were those judged as not chargeable offenses. The categories followed up by the interviewers were:

1. Ran away from home
  2. Hit one of your parents
  3. Skipped a day of school without a real excuse.
  4. Purposely damaged or messed up something not belonging to you.
  5. Tried to get something by lying about who you were or how old you were.
  6. Tried to get something by lying to a person about what you would do for him.
  7. Took something not belonging to you, even if returned.
  8. Hurt or injured someone on purpose.
  9. Threatened to hurt or injure someone.
  10. Went onto someone's property when you knew you were not supposed to.
  11. Went into a house or building when you knew you were not supposed to.
  12. Took part in a fight where a bunch of your friends were against another bunch.
  13. Carried a gun or knife besides an ordinary pocketknife.
  14. Took a car without the permission of the owner even if the car was returned.
5. Pairwise correlations of the predictor variables with SES were trivial. In the most extreme case, SES accounted for 5 percent of the variance in grades. The zero-order relationship of SES to delinquent behavior was similarly negligible, accounting for less than 1 percent of the variance.

6. An obvious alternative explanation of this finding would be cast in terms of the main effects of either self-esteem variable. These alternatives were found to be unable to account for the data: the main effect of conscious esteem was not significant, while that of unconscious was only marginally so and did not produce nearly so clear a separation of means.
7. It should be noted that the most extremely different means in Table 4 failed to meet the Scheffe criterion for reliability of post hoc comparison of means.

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# Problems in the Work Setting, Drinking, and Reasons for Drinking\*

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## ABSTRACT

*Industrial sociology and studies in the social-psychology of organizations have long been concerned with the effects of work setting on mental health. Despite the familiarity of the expression "My job is driving me to drink," very little research has focused on the effects of work-setting induced stress on increased alcohol consumption. In this paper we test two hypotheses linking the perception of various work-setting problems to (1) the frequency of drinking, and (2) reasons proffered for drinking. Data on a national sample of workers are used to test these hypotheses, controlling for job status, education, age and sex. To test the first hypothesis a five-way analysis of variance was performed for each of eight possible work-setting problems; for the second hypothesis a series of log-linear models were estimated to determine whether attitudes towards drinking differ by the presence or absence of work-setting problems. Results on the first hypothesis were mixed, but the log-linear analysis indicated that a worker is much more likely to state a particular reason for drinking is important if he/she experiences any one of eight different work-setting problems.*

Although a substantial body of literature links problems in the work setting (excessive job demands, organization strain, insufficient control over job decisions) to mental strain, mental illness, or dissatisfaction (see recent work by Bacharach and Aiken; Karasek), little has been done so far to document the association between problematic characteristics of the work environment and problem drinking. (See Archer for a review of the literature on occupational alcoholism; and Cooper and Payne for a recent compendium of work on the effects of stress at work on worker satisfaction.) A

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notable exception is the ongoing work of Roman and Trice and related studies by O'Toole and McLean. This line of research suggests that selected features of the work environment may be causally related to the use of alcohol. Unfortunately, there is not much agreement as to which aspects of the work setting are important to consider, whether subjective or objective measures of work-setting problems should be used, or whether the effects of occupational status are more or less important than the effects of work-setting structure. In this paper we attempt to clarify some of these issues by looking at the relationship between worker perceptions of work-setting problems and drinking behavior and attitudes, controlling for occupational status, education, age, and sex.

### **Background: Research & Theory**

Despite the numerous studies in the 1960s documenting the economic costs of employee alcohol abuse, there is relatively little research on alcoholism and the work place compared to the richness of psychological and subcultural studies of alcoholism. Among the most comprehensive investigations have been those of Trice and associates, and Schramm and associates. Trice and Roman have argued that the true cost of occupational alcohol abuse far exceeds the monetary expense of absenteeism, medical benefits paid, and accidents, since the performance failures of the employee who drinks affect both the performance and morale of the person's supervisors *and* subordinates. Energy expended in hiding alcoholism in the work setting reduces productivity, although the opportunity and ease of concealment is hypothesized to vary with job characteristics such as independence, sporadic demands, isolation, and variable hours (Maloney; Trice), the obsolescence of skills, and internal power struggles obscuring lines of authority (Trice and Belasco). Significantly, the sum of this research suggests (but does not provide evidence) that an important determinant of drinking practices can be found in the immediate social context, rather than in early childhood experience, cultural differences, or physiology. In fact recent work by Greeley and McCready raises questions concerning the relationship between ethnicity and drinking practices.

A number of studies have tried to distinguish different patterns of alcohol use by occupational status or occupational group. The findings are mixed. Many report no differences in alcoholism by organization strata or by industry (Chodorkoff et al.; Falkey and Schneyer; Schramm et al.; Straus and Bacon; Warkov et al.; Wellman et al.).

However, Mulford and Miller found a higher prevalence in the ranks of management than among labor, a finding consonant in part with Cahalan's finding of greater proportions of drinkers among middle and upper income groups than among lower income and black groups. There

have also been reports of higher prevalence of heavy drinking among certain occupational groups which exhibit characteristics of closely bounded subcultures (Pilcher; Pittman).

Loss of effectiveness in the work setting as a result of alcoholism is differentially expressed in different positions, ranging from outright absenteeism among factory workers to physically present but mentally absent executives. This pattern has been shown *not* to be related to either reported levels of job satisfaction (Archer) or to employment instability (Maxwell; Trice). Trice hypothesized that the ability to hide alcoholism is related to structural features of the work setting. Using turnover rates as indicators of structural features of the work setting, Schollaert did not find lower termination or turnover rates among alcoholic workers in ". . . jobs having structural characteristics which reduce sanctions against excessive drinking" (Schollaert, 180) compared to workers in other jobs. Although marred by this use of turnover rates as an indirect measure of the presence or absence of problematic features of job structure, this study is significant since it focuses on the work setting, and points the way to an analysis of work-setting characteristics which could be related to problem drinking. The obvious next step would be to determine whether there is an association between structural characteristics of the job and drinking.

A careful analysis of the linkages between work setting and health are found in models proposed by French and Kahn and their colleagues (Kahn et al.; Katz and Kahn). The theoretical model they develop links the broad conceptual categories of objective work environment, psychological environment, worker response, and mental and physical health and disease, while controlling for the genetic, demographic and personality properties of individuals. This model provides the framework for a large number of empirical studies undertaken at the Institute for Social Research of the University of Michigan. These studies fall into two major lines of research: (1) studies of specific aspects of job environment and/or specific occupations and their effects on worker health (Haber; Levi; McGrath,a,b; McLean; Quinn and Shepard; Quinn et al.; Russek); and (2) studies of stress in the work role (Caplan; Caplan et al.; Cobb a,b; French; Kahn et al.; Sales). Unfortunately, neither of these two lines explicitly treats the effects of organization structure on mental health, and many of these studies use incomparable measures and differing conceptualizations of both independent and dependent variables. Further, there is no real focus on the use of alcohol as an aspect of worker health. Several individual studies, however, provide interesting results which are directly related to our intended study.

Work by Quinn and his colleagues on work conditions, life satisfactions, and behavioral responses of workers reveals two important findings. First, groups of workers who described their jobs as most beset by problems and themselves as being least satisfied nevertheless made the fewest suggestions to their employers compared to those in other worker groups.

When viewed against the overall conclusions of these studies (that dissatisfaction with different aspects of life are related, and that dissatisfaction with work is a strong predictor of the others) this suggests that job dissatisfactions are worked out in other areas of the worker's life. Second, job stress appears to be related to escapist drinking. Taken together, these results point to the importance of explicitly studying alcoholism and drinking behavior as dependent variables, affected by characteristics of the job setting.

A number of researchers (French; Weiner et al.) have considered the importance of social support in the work setting as a factor which lessens the physiological effects of job-related stress. Among the physiological measures which seem to be lowered by social support from one's peers, supervisors, and subordinates were serum cortisol, blood pressure, blood glucose levels, and number of cigarettes smoked. Drinking habits are conspicuous by their absence in this list of measures. Karasek has recently distinguished between the supportive styles of supervisors and co-workers as separate variables which can either buffer or aggravate the worker's felt mental strain and job dissatisfaction. Dependent variables considered in this study included self-reports of exhaustion, depression, and absenteeism. Again, drinking habits were not investigated.

A theory of organization-induced strain has been developed and tested by Dornbusch and Scott. Perceived inconsistencies or incompatibilities in the worker's task arrangements are predicted to lead to dissatisfaction for the individual worker. Among the important incompatibilities shown by these authors to produce strain are conflicting work demands, insufficient information or materials to complete an assigned task, dependence on unreliable or incompetent co-workers, unfair evaluations and allocations of rewards, and incompetent supervisors. Dornbusch and Scott identify three general ways of coping with such problems. First, the individual can leave the organization. Second, the worker can lower expectations as to what can be accomplished in that particular work setting. Third, the most likely response, according to the authors, would be a reaction to the authority system itself, such as communicating dissatisfaction to others in the organization, suggesting changes to others, or noncompliance with directives from supervisors. However, as we have seen from our review of other studies, it is highly likely that tensions produced by the work setting are not necessarily released through such constructive adaptive responses as making suggestions to one's superiors or attempting to change the adverse condition. Further, leaving one's job is rarely an easy alternative, and the difficulties of transferring work skills and benefits increase with job tenure (Batchelder; Palmer). Rather, it is more likely that such dissatisfactions are worked out in other areas of the worker's life, and drinking is a logical possibility.

Finally, Trice's recent review of occupational risk factors and em-

ployee alcoholism discusses several attempts to investigate the effects of various job characteristics on problem drinking. Although subject to problems of data interpretation, Manello and Seaman suggested that not being seen by, or lacking contact with one's supervisors is associated with problem drinking. Parker's results emphasize the importance of status inconsistency in the work setting as a source of job-related strain often coped with by resort to alcohol. Trice summarizes this set of studies as indicating increased interest in developing the job-conditions-drinking hypothesis, but concludes that in general "... no major effort has been mounted" to test these hypotheses (e,5).

In this paper we intend to explore the possibility that dissatisfactions due to perceived inconsistencies or incompatibilities in the work setting are coped with by drinking. We hypothesize that workers reporting perceived work-setting incompatibilities are likely to drink more frequently than other workers. We do not attempt to explain the entire range of drinking behavior: quantity of alcohol consumed, "binge" drinking, drinking in public or on the job in contrast to home consumption, or chronic drinking problems. Nor do we assert that drinking is the only possible response to stress experienced in the work setting, or that everyone experiencing such problems will drink more than usual. It is likely, however, that there is a relationship between the experience of work-related incompatibilities and the felt need to drink as a means of reducing such work-related tensions. Accordingly, we further hypothesize that workers experiencing work-related incompatibilities are more likely to consider a variety of reasons for drinking important, compared to other workers. Since attitudes about drinking, as well as actual drinking behavior, interact with the experience of work-setting problems, any given reason for drinking is more likely to be seen as a legitimate reason to do so by workers who perceive their work settings as problematic than by workers not subject to such stress.

The use of "reasons for drinking" as an outcome variable is well-supported in the drinking practices literature. Knupfer et al. reported strong associations between reported heavy weekly drinking (a measure of frequency) and tension-relief reasons for drinking. Mulford and Miller tested the hypothesis that heavier drinkers were more likely to report drinking for the personal effects of alcohol in addition to the typical social reasons acknowledged by lighter drinkers. The alcohol scale devised by these authors incorporates a greater range of possible meanings and uses of alcohol in order to differentiate heavy from light drinkers. Cahalan et al. demonstrated the complexity of drinking behavior in a triaxial quantity-frequency-variability classification. This classification system makes finer distinctions than simple frequency or quantity-frequency classifications. Using this system, heavy drinkers tended to rate more reasons for drinking as important than did other drinkers, and were more likely to choose escape reasons, such as "help me to relax," "to forget everything," "to

forget my worries," "in a bad mood," and "because I'm tense and nervous." These reasons formed a Guttman-type scale permitting the identification of "escape" and "heavy escape" subgroups. The subgroups showed no consistent association with social class when age and sex were controlled. Furthermore, proportions in each group did not vary significantly with age, and were strongly associated with higher scores on anxiety, depression, and alienation scales, independent of age, sex, and social class effects.

An additional advantage of considering both drinking behavior (frequency of drinking) and attitudes (reasons for drinking) is that we can then develop a more reliable picture of the relationships between perceptions of work structure and drinking, especially given the problems entailed in surveying sensitive topics such as drinking habits (Locander et al.). Furthermore, we will investigate the relationship between specific work-setting incompatibilities and particular drinking behaviors and attitudes while controlling for various measures of social and occupational status which might affect either our independent or dependent variables. Hence, we include measures of occupational status, level of education, age, and sex.<sup>1</sup> Finally, it should be emphasized that we are relying on self-reports of both drinking behavior and the experience of work-setting inconsistencies; therefore our measure of drinking frequency is likely to be subject to an underreport bias, and our measures of work-setting structure capture *perceptions* of job problems, not actual job structure.

## Data and Methods

Data to test our two hypotheses come from a recent national survey of workers in the United States: The Quality of Employment Survey<sup>2</sup> for 1972 conducted by the Institute for Social Research at the University of Michigan. The original survey had 1,496 respondents, all of whom were at least 16 years old and worked for pay 20 or more hours per week.<sup>3</sup>

In testing our first hypothesis concerning frequency of drinking, all respondents with complete data on a series of questions examining preferred and actual characteristics of one's work setting were used ( $N=1,422$ ). To test the second hypothesis, we drew from the original sample a subsample of all 931 respondents with complete data on the work-setting items and on a series of questions on reasons for drinking. Table 1 presents a summary of major demographic characteristics of both the original sample and the smaller subsample, as well as the distribution of the original sample on the first dependent variable considered, frequency of drinking, and the breakdown of the sample on the work-setting problems used as independent variables in this study. The subsample of 931 is comparable to the original sample on all basic demographic characteristics.

**Table 1.** SAMPLE CHARACTERISTICS (IN PERCENT)

	N = 1,422	N = 931
<u>Sex</u>		
Male	68	67
Female	32	33
<u>Education</u>		
Less than high school graduate	26	25
High school graduate	38	39
Some college	36	36
<u>Job Status</u>		
White collar	50	52
Blue collar	50	48
<u>Age</u>		
16-24 years	15	18
25-40 years	42	43
41-55 years	31	29
56-77 years	12	10
<u>Frequency of Drinking</u>		
1 Never	16	
2	7	
3	13	
4	9	
5	9	
6	21	
7	11	
8	10	
9	2	
10 three or more times per day	2	
<u>Work-Setting Problems (N = 1,422)</u>		
	<u>A Problem</u>	<u>Not a Problem</u>
Not enough information	38	62
Can't see results	35	65
Not enough time	57	43
Conflicting demands	65	35
Promotions unfair	67	33
No help from supervisor	51	49
No help from co-workers	52	48
Co-workers incompetent	54	46

Subjects were asked to rate the importance of various characteristics of one's job, such as having enough help and equipment to get the job done, being able to see the results of one's work, to be free from conflicting demands, etc. They also answered a similar set of questions asking how true these conditions were of the respondent's actual job. Responses to the first series were categorized as important/not important; and to the second series, true/not true of the job.<sup>4</sup> Workers saying that a particular (desirable) characteristic *was important and was not true* of their jobs were categorized as experiencing a work-setting problem, similar to the structural incompatibilities of Dornbusch and Scott. Respondents indicating a particular char-

acteristic *was important and was true* of their jobs, or *was not important* were categorized as free from that particular work-setting inconsistency.<sup>5</sup> On any given work-setting problem, from 35 to 67 percent of the sample was classified as experiencing a work-setting problem (see Table 1).

Respondents were asked to rate the frequency with which they had a drink of liquor, beer, or wine on a scale from 1 (never) to 10 (three or more times a day).<sup>6</sup> They were also asked to rate the importance of a series of reasons for drinking, and responses were dichotomized as important/not important.<sup>7</sup>

To test the first of our hypotheses a five-way analysis of variance was performed for each of the eight possible work-setting problems, decomposing the variance of frequency of drinking by whether the worker experiences a given work-setting problem, educational level achieved (less than high school graduate, high school graduate, some college education), occupation (white collar, blue collar), age (16–24 years, 25–40, 41–55, 56–77), and sex. Using a multiple classification analysis, adjusted means were obtained for the two groups of workers (no problem in the job setting, vs. experiences a problem), controlling for education, occupation, age, and sex.

To test whether attitudes towards drinking differ by the presence or absence of work-setting inconsistencies, we estimated a series of log-linear models (see Goodman a,b). In these analyses we were primarily interested in the extent to which indicating a particular reason for drinking as important is more probable when the worker experiences a particular work-setting problem, controlling for education, occupation, and sex of respondent. Age was not included among the control variables for these analyses for two reasons: (1) research findings seem to indicate that age has no consistent significant association with reasons chosen for drinking, and (2) the distribution of cases per cell with the addition of this four-category variable would result in too many empty cells. The log-linear specification of this analysis permits tests for statistical significance, provides exact parameters for the magnitude of the effects of each work-setting problem, and provides a ready method of testing for interactions among the independent and control variables.

For each of seven reasons for drinking rated by workers as either important or unimportant, two models were estimated and likelihood ratio chi-squares for each were obtained. The first represents a baseline model in which we include (1) the effects of the three control variables (education, occupation, sex) on the dependent variables (importance of reason for drinking), and (2) the association among the independent variables (presence or absence of a work-setting problem) and the controls. The second model represents the test model which contains both (1) and (2) above, plus the direct effect of the independent variable on the dependent variable. Net likelihood ratio chi-squares,  $\Delta X^2$ , were then calculated from the

difference between  $X_b^2$  (likelihood ratio chi-square for the baseline model) and  $X_t^2$  (likelihood ratio chi-square for the test model). The statistical significance of  $\Delta X^2$ , then, refers to the differences in the likelihood of considering a reason for drinking to be important by whether the worker experiences a work-setting problem. The  $p$ -levels obtained for each  $\Delta X^2$  represent the probability level that the chi-square statistic is due to chance.

Finally, we also calculated net percentage differences in rates of indicating a reason for drinking as important. These differences are interpreted as the increase in the likelihood of stating that a particular reason for drinking is important when the respondent experiences a work-setting problem. Net percentage differences are calculated using the formula:

$$100 \times ((\gamma a / \gamma b) - 1)$$

where  $\gamma a$  represents the ratio of the odds of citing a given reason if one experiences a work-setting problem, and  $\gamma b$  represents the ratio of the odds of citing a given reason if one does not experience a work-setting problem.

Results for the analysis of variance on frequency of drinking are presented in Table 2. Table 3 presents net likelihood ratio chi-squares from the log-linear analysis of the effects of work-setting problems on reasons given for drinking, and Table 4 contains the net percentage differences in rates of giving reasons.

**Table 2.** EFFECTS OF WORK-SETTING PROBLEMS ON FREQUENCY OF DRINKING CONTROLLING FOR EDUCATION AGE SEX, AND OCCUPATION F-SCORES AND ADJUSTED MEANS

Independent Variable	Value*	Entire Sample (N = 1,422)		Non-Drinkers Excluded (N = 1,199)	
		F	Adjusted Means	F	Adjusted Means
Not enough information	0	.00	4.75	.642	5.95
	1		4.75		5.41
Can't see results	0	2.23	4.82	.043	5.79
	1		4.62		5.65
Not enough time	0	2.56	4.63	4.749**	5.58
	1		4.84		5.86
Conflicting demands	0	.04	4.77	.511	6.06
	1		4.74		5.57
Promotions unfair	0	.06	4.77	3.598**	4.88
	1		4.74		6.18
No help from supervisor	0	.07	4.73	.233	5.58
	1		4.77		5.89
No help from co-workers	0	6.33**	4.58	1.005	5.39
	1		4.90		6.04
Co-workers incompetent	0	1.08	4.68	1.151	5.36
	1		4.81		6.06

\*0 = Worker does not experience this problem.

1 = Worker does experience this problem.

\*\* $p \leq .05$ .



**Table 3.** EFFECTS OF WORK-SETTING PROBLEMS ON REASONS GIVEN FOR DRINKING, CONTROLLING FOR EDUCATION, SEX, AND OCCUPATION. NET LIKELIHOOD RATIO CHI-SQUARES<sup>a</sup> (1 df, N = 931)

Independent Variables	Dependent Variables <sup>b</sup> Reason for Drinking						
	To Relax	To Forget About Job	Make R Feel Good	To Forget Everything	To Forget Worries	To Forget Problems	For Job Pressure
Not enough information	4.16*	1.87	1.16	4.46*	6.73***	3.90*	4.15*
Can't see results	.22	2.42	3.38	.5	8.75***	5.08**	1.94
Not enough time	9.37***	12.94***	19.60***	10.68***	11.17***	24.86***	17.85***
Conflicting demands	.65	5.13**	4.02*	6.41**	11.60***	12.2**	5.42**
Promotions unfair	4.02*	8.80***	2.92	3.54	10.78***	11.40***	6.73***
No help from supervisor	4.39*	10.16***	4.03*	4.94*	9.52***	22.41***	10.71***
No help from co-workers	12.02*	4.74*	12.05***	9.92***	9.01***	14.76***	13.44**
Co-workers incompetent	3.00	6.44**	4.72*	10.31***	22.30***	22.30***	9.40***

<sup>a</sup>Net likelihood ratio chi-squares,  $\Delta\chi^2$ , are the differences obtained from:  $\chi_B^2 - \chi_T^2$  where

$\chi_B^2$  = likelihood ratio chi-square for the base line model [ACDE][BCDE] which includes the effects of control variables (C, D, E) on the dependent variable (A), and the association among the independent variable (B) and the controls, with 12 degrees of freedom

$\chi_T^2$  = likelihood ratio chi-square for the "test" model [ACDE][BCDE] [AB] which introduces the direct effect of the independent variable on the dependent variable, with 11 degrees of freedom.

\* $P \leq .05$

\*\* $P \leq .025$ .

\*\*\* $P \leq .01$ .

Results

From Table 2 we can see that most of the F-values from the five-way analyses of variance on frequency of drinking using the entire sample are not significant. Only one work-setting problem, not receiving preferred levels of help from one's co-workers, reached acceptable levels of significance ( $p \leq .05$ ). The adjusted means from the multiple classification analyses show very little difference in frequency of drinking between the two groups of workers. Among both workers who experience work-setting problems and those who do not, reported drinking of alcoholic beverages averages about once or twice a month. In four of the eight problem areas the means are slightly larger for workers who experience work-setting problems. In general, results bearing on our first hypothesis appear to be mixed at best. There were significant differences in the frequency of drinking by sex (men drink more often), occupational status (white collar work-

**Table 4.** EFFECTS OF WORK-SETTING PROBLEMS ON REASONS GIVEN FOR DRINKING CONTROLLING FOR EDUCATION, SEX, AND OCCUPATION. NET PERCENT DIFFERENCES IN RATES OF GIVING REASONS<sup>a</sup> (N = 931)

Independent Variables	Dependent Variables: Reason for Drinking						
	To Relax	To Forget About Job	Make R Feel Good	To Forget Everything	To Forget Worries	To Forget Problems	For Job Pressur
Not enough information	33	31	16	53	55	47	59
Can't see results	7	38	30	16	67	57	38
Not enough time	55	115	87	97	81	194	192
Conflicting demands	12	63	33	71	87	114	80
Promotions unfair	36	95	30	51	89	113	99
No help from supervisor	36	95	33	57	73	168	122
No help from co-workers	78	55	62	88	67	116	142
Co-workers incompetent	28	67	35	91	129	198	107

<sup>a</sup>Net percent differences are interpreted as the increase in the likelihood of stating a particular reason for drinking is important, given the respondent experiences a work-setting problem compared to no-work-setting problem. Net percent differences are calculated using

$$100 \times \left( \frac{\gamma^b}{\gamma^a} - 1 \right) \text{ where: } \gamma^a = \text{ratio of the odds of citing a given reason if one experiences a work setting problem.}$$

$$\gamma^b = \text{ratio of the odds of citing a given reason if one does not experience a work-setting problem.}$$

ers drink more often) and age (younger workers drink more often). Furthermore, significant two-way interactions were found between age and not having enough information to complete the job, between sex and not being able to see the results of one's work, and between occupational status and not receiving preferred levels of help from one's co-workers.

As can be seen from Table 1, the distribution for the sample of 1,433 respondents was bi-modal, with 16 percent of the sample responding that they never had a drink of liquor, beer, or wine, and 21 percent indicating they drink once or twice a week. As was mentioned previously, self-report of drinking behavior can be problematic; this particular sample may, in fact, suffer an underreport bias. The validation of self-report drinking scales is itself a question generating considerable research. To date, the most comprehensive discussion of survey validity is provided by Pernanen. The lowest degree of intentional as well as unintentional underreporting of consumption was found to be among regular drinkers, and among drinkers surveyed by telephone and by mail. Typical surveys are likely to under-sample the heaviest and lowest-status drinkers, thus overrepresenting the higher status, lower-consumption drinkers. In this sample only 14 percent of the respondents report drinking once a day or more often, whereas 34 percent report drinking once a year or less frequently.

To test whether the skewness of this sample toward non-drinkers might affect the relationship between job problems and frequency of drinking, a second set of ANOVAs were performed in which the "never drink" category and respondents were dropped from the sample.<sup>8</sup> These results are shown in the right-hand columns of Table 2. Again, most of the F-values from the five-way analyses of variance on frequency of drinking are not significant. Only two work-setting problems, not having enough time to complete assigned tasks and perceiving promotions as handled unfairly, reached acceptable levels of significance. As before, the adjusted means show very little difference in frequency of drinking between the two groups of workers. Both among workers who experience work-setting problems and those who do not, reported drinking of alcoholic beverages averages about once or twice a week (slightly higher averages than those obtained for the entire sample). In six of the eight problem areas, however, the means are larger for workers who experience work-setting problems. Although men still tend to drink slightly more frequently than women in this adjusted sample, there were no significant differences in the frequency of drinking by age groups, occupation or educational level, nor were any of the two-way interactions among control and independent variables statistically significant.<sup>9</sup>

In general, although the results of both sets of analyses of variance for the first hypothesis are not overwhelming, there does appear to be some difference in the frequency of drinking reported by workers who experience job-setting problems. These differences seem somewhat clearer when non-drinkers are excluded from the sample, which suggests that underreporting bias may affect these analyses.

Tables 3 and 4 present results on our second hypothesis concerning the effects of work-setting problems on the likelihood of citing a given reason for drinking as important. From an analysis of the net likelihood ratio chi-squares shown in Table 3, we find that 45 out of a possible 56 chi-squares (or 80%) are significant at the .05 level or better, and 28 out of 56 (or 50%) are significant at the .01 level or better. Three of the work-setting problems (not receiving preferred levels of help from one's supervisor or from one's co-workers, and not having enough time to complete assigned tasks) yielded significant net chi-squares for every one of the seven reasons for drinking tested here. This can be interpreted to mean that experiencing these work-setting problems is likely to increase the probability of citing any one of a variety of possible reasons for drinking as important or legitimate reasons for drinking. Similarly, two other work-setting problems (experiencing conflicting demands on the job and having incompetent co-workers) yielded significant net chi-squares for all but one of the seven reasons tested.

The particular work-setting problems that seem most consistently to yield significant chi-squares (lack of help from one's co-workers and super-

visor) affirm results obtained by Karasek and others. Social support from peers and supervisors in the work setting apparently can buffer stress or tension on the job. Clearly, the lack of such support seems to be associated with high probabilities of defining at least seven different reasons for drinking as important. Not having enough time to complete one's work, experiencing conflicting demands on the job, and having incompetent co-workers are types of task-environment characteristics similar to the structural work arrangement problems discussed by Dornbusch and Scott. Again, our results confirm previous studies of the impact of such problems on tension and attitudes about drinking.

The importance of these consistently significant net likelihood ratio chi-squares for a variety of work-setting problems and a variety of possible reasons for drinking is enhanced by the fact that the models tested included measures of occupational status, level of education, and sex of respondent. Apparently, the effect of inconsistencies in the work place holds across levels of education, for white and blue collar workers, for both men and women.

Finally, Table 4 presents the net percentage differences in rates of citing reasons for drinking as important. The percentages shown are interpreted as the increase in the likelihood of stating that a particular reason for drinking is important, given the respondent experiences a work-setting problem compared to workers who do not experience that problem. In 51 out of 56 instances (or 91%) there is an increase of at least 30 percent in the likelihood of giving a particular reason for drinking as important, due to the presence of a work-setting problem. In other words, workers are 30 percent more likely to cite any one of these seven reasons for drinking as important if they report a job-setting inconsistency. Further, in 39 cases (or 70%) the net percentage differences indicate an increase of at least 50 percent in the likelihood of citing a reason as important due to the presence of a work-setting problem, and in 17 instances (or 30%) there is an increase of 90 percent or more in the likelihood of stating a reason for drinking is important. Again, these are increases in likelihood due primarily to the presence of work-setting problems, with the effects of sex, occupation, and education included in both baseline and test model.

In general, it would appear that our second hypothesis is supported; attitudes about drinking are seemingly related to the presence or absence of work-setting inconsistencies. These results are significant for a number of reasons. Drinking behavior and attitudes about drinking are infrequently used as measures of mental health or disease, even though much of the literature suggests drinking may provide a mechanism for the release of tension, including tension produced in the work setting. This is so perhaps because self-report measures of drinking behavior are subject to an under-report bias, thereby depressing variation in frequency of drinking. Measuring attitudes about drinking, such as why people drink, is less threatening,

therefore likely to be subject to less bias. Although reasons for drinking are poor substitutes for good measures of actual drinking behavior, they can provide us with a general picture of the conditions that promote drinking.

The apparently strong relationship between problematic work structure and reasons given for drinking suggests that perceptions of the work setting are instrumental in defining drinking as a tension-release mechanism. The more stress produced in the work setting, the more likely one is to agree that drinking will help one relax and forget job worries, problems, and pressures. Furthermore, these results demonstrate the need to focus on characteristics of task arrangements and performance evaluations as structural sources of stress in *any* work setting. Any occupation can be stressful if actual (or perceived) work arrangements do not correspond with expectations, regardless of the occupational status of the job, or the social status (education) of the job holder.

### Discussion and Implications

While we have demonstrated a relationship between perceptions of work-setting problems and attitudes about why people drink, there are several very important questions we have not answered: hence these caveats.

First, we have not conclusively demonstrated the existence of a relationship between actual drinking behavior and work-setting problems although there are trends in the data that would support such a hypothesis. There are, of course, a number of difficulties involved in total reliance on self-reported data, which may account for the lack of strong results obtained in testing the first hypothesis. Measures of both frequency of drinking and job conditions are suspect in this regard, but perceptions of job inconsistencies as measured here are probably less problematic. Nonetheless, we must develop more objective measures of both work-setting problems and drinking behavior if we are to have a valid test of hypotheses relating these two variables.

Second, although the results of our second hypothesis are persuasive, we cannot assume that measures of attitudes are automatically related to actual behavior. This caveat is especially germane in studying drinking-related attitudes and behavior. We cannot, for example, assume that people who rate many reasons for drinking as important are likely to drink more often, or to be heavier drinkers than people who cite only one or two reasons for drinking as important. Such a relationship has been demonstrated (see Cahalan et al.,<sup>a,b</sup>; Knupfer et al.; Mulford and Miller,<sup>a,b</sup>), but not clearly proven.

Finally, although we have demonstrated the association between perceptions of work-setting problems and attitudes about drinking, we have not shown causality. The present data are clearly inadequate to test

hypotheses causally linking aspects of the work environment to drinking attitudes or behavior. Ideally, a panel of workers should be followed longitudinally, measuring drinking behavior and attitudes and perceptions of job structure throughout their tenure on a given job. This would allow researchers to track changes in perceptions of job problems and changes in drinking habits, and thus determine the causal ordering of these related variables.

In conclusion, although we found preliminary support for our hypotheses relating work structure to drinking attitudes and behavior, much work remains to be done. Alcoholism is clearly a social problem of some magnitude, and Roman and Trice have shown how important the job setting may be as a focus of both theoretical research and practical intervention. As we have seen in our review of the literature, most studies of alcoholism have investigated psychological predispositions, or ethnic background, or occupational grouping as variables explaining differential rates of alcohol consumption. To change personality attributes, or family system dynamics, or subcultural values is difficult and implies a long-range approach to treatment. There are, however, more immediate interventions possible, those which deal with current situations and behaviors. Thus, the use of employee confrontation and the threat of job action has been regarded as a valuable tool in occupational programming (Beyer et al.; Trice; Trice et al.).

Effective treatment of alcoholism that is used to reduce stress, requires us to consider the source of stress. If structural characteristics of the work setting (such as conflicting work demands, inadequate supervision or support, and unclear or unfair systems of performance evaluation) increase job stress which in turn increases the likelihood of drinking for stress relief, then such structural problems should not be ignored; they should be investigated as factors to be considered in job restructuring for affected individuals. The work organization is itself now seen as a primary setting for the detection and referral of employees with drinking problems (Beyer and Trice), and structural considerations are built into the confrontation approach. Thus, further development of the links between structural characteristics of the work setting and drinking behavior promise both theoretical and practical benefits.

## Notes

1. Routinely used independent variables in previous studies of drinking behavior and attitudes have been age, sex and measures of socioeconomic status, such as occupation, income, and education. Cahalan et al., as well as most other alcohol-use surveyors, have found consistent patterns in both the aggregate proportion of drinkers and in the quantity-frequency-variation measures of consumption in age, sex, and social class groupings. The highest proportions of non-drinkers are found among women, older age groups and lower social classes, with highest consumption reported among men, younger age groups, and lower social classes (middle and upper class groups tend to be moderate drinkers, whereas large proportions of

both non-drinkers and heavy drinkers can be found in lower social classes). Age, sex, occupational status, and education are controlled for in this study to allow for an examination of the separate effects of work-setting problems on drinking behavior.

2. The data used in this paper were made available by the ISR Social Science Archive. The data for the Quality of Employment Study (1972-73) were originally collected by Robert P. Quinn, Thomas W. Mangione, and Stanley E. Seashore of the Survey Research Center, ISR, the University of Michigan. Neither the original collectors of the data nor the archive bears any responsibility for the analyses or interpretations presented here.

3. Personal interviews were conducted with 1,490 persons living in housing units within the United States and the District of Columbia, exclusive of military reservations, Hawaii, and Alaska. Approximately 70 percent of the households had one or more persons who met the eligibility criteria for respondent selection, of whom 1,982 persons were designated as respondents; of these 75.5 percent were interviewed. The basic sample design was that customarily used by the Survey Research Center to select national probability samples of dwellings.

4. Subjects were asked to rate a set of job characteristics as "not at all important/a little important/somewhat important/very important to me to have a job where . . ."—I have enough information to get the job done, I can see the results of my work; I have enough time to get the job done, I am free from conflicting demands other people make of me; Promotions are handled fairly, My supervisor is helpful to me in getting my job done; The people I work with are competent in doing their jobs. About each of these statements the respondent was asked: "How true (do) you feel each is of your job? this is not at all true of my job/a little true of my job/somewhat true of my job/very true of my job." This variable was collapsed into two values: either not true of my job or true of my job. The last category included all respondents checking either "a little true," "somewhat true," and "very true." Perceptions of job characteristics were collapsed in a similar fashion.

5. Indicators of work-setting problems for this study were primarily those items which seemed to tap incompatibilities of the authority system such as those suggested by Dornbusch and Scott, and for which there was a fairly even distribution of responses across categories. As a result, we are unable to test hypotheses suggested by Karasek and others concerning various risk factors, such as the extent to which jobs require mobility, are characterized by closeness of supervision and visibility of performance, or the extent to which jobs are tied in with other jobs.

6. The complete response scale is as follows: 1. never; 2. less than once a year; 3. less than once a month but at least once a year; 4. about once a month; 5. two or three times a month; 6. once or twice a week; 7. three or four times a week; 8. once a day; 9. two times a day; 10. three or more times a day;

7. Format of the questions was as follows: "People drink wine, beer or liquor for different reasons. Here are some statements people have made about why they drink. How important would you say that each of the following is to you as a reason for drinking?" The list of statements included: I drink because it helps me to relax, I drink when I want to forget about my job; I drink because it makes me feel good; I drink when I want to forget everything; A drink helps me to forget my worries; A drink helps me to forget the problems on my job, I drink because I need it when there is pressure on my job.

Each reason for drinking was categorized by respondents as either: not at all important; a little important, somewhat important, very important;

The last three categories were then collapsed into *important* and *not important*.

8. This analysis was suggested by a *Social Forces* referee.

9. These results (on the main effects of the control variables for each sample and on interaction effects) are available on request.

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# The Hawthorne Experiments: Statistical Evidence for a Learning Hypothesis\*

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## ABSTRACT

*The historical productivity data from the Hawthorne experiments permit the statistical evaluation of the hypotheses of the original experimenters and others by conducting appropriate time-series analyses. In this paper a recent hypothesis by H. M. Parsons that the output trends in the first relay assembly test room experiment were due primarily to increased skill and competence is evaluated empirically by fitting a standard learning equation to the data for each operator. The learning equation describes the overall data trends very well. Then, to assess the effects of the experimental manipulations and variables hypothesized by other investigators, the productivity data are detrended of the learning effect and the detrended data are analyzed. This analysis shows that the introduction of rest pauses in the test room led to the more productive use of work time. Overall, it appears that the conditions motivating increased learning—i.e., improved reinforcement conditions (increased status and new economic incentives) and regular performance feedback—were the primary causes of increased output.*

Because the mysterious findings of long-term overall increases in worker productivity stimulated widespread theorizing, research, and debate, the Hawthorne First Relay Assembly Test Room experiment has had far-reaching and continuing effects on management theory and policy. The many explanations, reanalyses and subsequent critiques of the Hawthorne research have extended the debate between contending management philosophies and in particular between the Human Relations (Mayo, a, b) and Scientific Management (Taylor) approaches. During the early years most of the debate was based solely on ideological grounds (cf. Argyle; Landsberger). Later analyses (cf. Blumberg; Carey) marshalled substantive evi-

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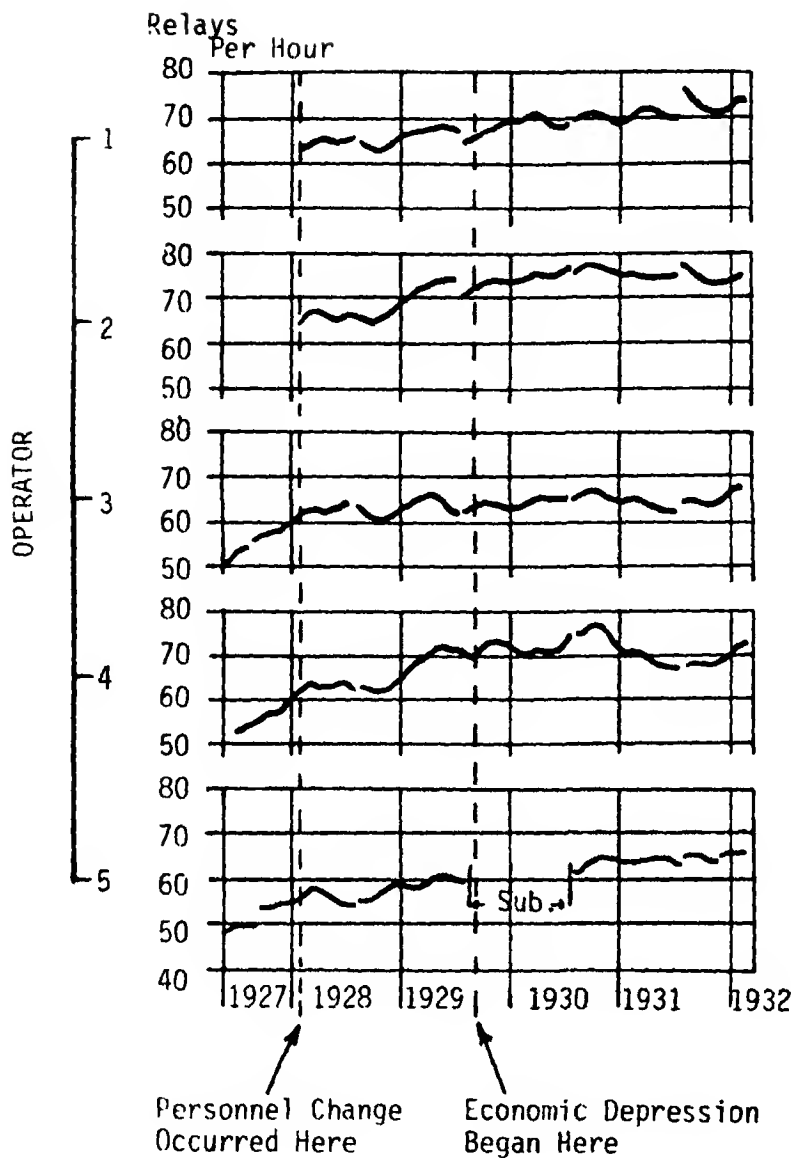
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dence to support ideologies. These various interpretations of the Hawthorne experiments are now being reappraised and supplemented by time-series statistical analyses of the historical productivity data. Franke and Kaul published one statistical interpretation of the relay assembly experiment, but did not directly test several of the alternative explanations. The purpose of this paper is to evaluate further the hypotheses of the original experimenters and analysts (Pennock; Roethlisberger and Dickson; Whitehead, b) and others (Franke and Kaul; Parsons, a, b) through alternative substantive and statistical analyses of the quantitative data.

### Explanations of the Productivity Trend

According to the interpretation given in the original reports of the research, the increase in workers' productivity was due to a positive change in mental attitude (Pennock, 279-309; Roethlisberger and Dickson, 189-90). Later studies have variously attributed the cause of this improved morale and attitude to the development of cohesive primary-group relationships (cf. Shils), the system of participative management (Blumberg), or the placebo/Hawthorne effect generated by willing participation in an experiment (cf. Dickson and Roethlisberger). Other researchers have discounted the effect of morale and have emphasized the importance of certain management decisions, such as the introduction of rest pauses, economic incentives (cf. Carey), or the replacement of the two insubordinate operators (Franke and Kaul).

Yet a major flaw in all these explanations is that in the debate over the Hawthorne experiments no research has ever been cited showing that any of these variables result in a progressive rise in productivity over a long period of time (Parsons, b). Nor do their theoretical rationales suggest continuously incrementing long-term effects. Theoretically, the factors that (1) were intentionally manipulated (frequency and duration of rest pauses, hours per day and week), (2) were inadvertently introduced (small group piecework incentive system, participative management, presence of observers, replacement of two operators), or (3) occurred during the experiment (primary group formation, economic depression) should result in rapid and somewhat discrete changes in output. For example, Franke argues that the change in personnel and the onset of the economic depression should have resulted in immediate "general productivity spurt(s)." Yet there were no such distinct increases in *individual* productivity when either of these events occurred (see Whitehead, a, Figure 1).<sup>1</sup> In fact, the most notable characteristic of the data from the relay assembly experiment is the overall upward and relatively continuous trend both within and between experimental periods (Parsons; Roethlisberger and Dickson; Schlaifer) (see Figure 1).



**Figure 1.** Hourly output rates of each operator in the Relay Assembly Test Room Experiment (sub indicates a substitute) (Source adapted from Mayo, a) Smoothed curves are presented here to illustrate the overall trend. Note the absence of any change in trend when the personnel change and economic depression events occurred

Lines of evidence from other Hawthorne experiments illustrate the positive but discrete effects on productivity of change to the piecework system and the introduction of rest periods. In the second relay assembly experiment output jumped 12.6 percent when the small group piecework rate was introduced. It then dropped 16 percent when the department-

wide method of payment was resumed (see Franke and Kaul; Parsons, b). Output in the mica splitting test room rose 15.6 percent after the introduction of rest pauses (Roethlisberger and Dickson), yet the increase was immediate with no evidence of a long-term trend as in the relay assembly experiment. Regarding the effect of participative management, Coch and French found that productivity increases accompanying the involvement of workers in decision-making leveled off within three weeks. Thus, the discrete change patterns evidenced by the traditional explanations are incongruous with the continuity of the observed relay assembly room output data. Because the impact of these variables tends to be rapid and discrete, they are not credible as adequate explanations for what happened in the first relay assembly experiment—i.e., more than three years of overall increases in productivity followed by almost two years of relatively constant output rates.

What continuous and enduring process might account for this overall productivity trend? In an insightful and persuasive analysis of the Hawthorne experiments, Parsons (a) suggested that the long-term increase in output rates followed eventually by somewhat asymptotic rates was mainly due to increased skill and competence in assembling relays (a learning effect). The nature of the task, the facilitative conditions, and the relatively low starting rate permitted operators to develop and vary procedures eliminating nonproductive activities.<sup>2</sup> Much of the learning was probably vicarious as evidenced by the high correlations in the output rates of girls who sat next to each other (cf. Hare, 31). This expertise learning resulted in gradual increases in productivity until the operators reached an apparent steady state level (at about period 16, May 1930) where their output rate fluctuated for the following 19 months (Parsons, a). Operator 5 was the only operator not to reach a steady state level. For some reason this operator was assigned many more different types to assemble and these frequent changes undoubtedly inhibited her learning.

The hypothesis that the overall trend in output was due to learning more productive work skills and habits is supported by the general topography of the operator's output trends, which are typical of well-documented collective and organizational learning curves (cf. Baloff, a, b; Hamblin et al., b; Jordan), and by the isomorphisms between the relay assembly test room and an ideal learning environment. Two necessary characteristics of environments that elicit increased learning are: (1) a change to a more valued reinforcement schedule and (2) more or less regular performance feedback (Hamblin and Kunkel).

Parsons (a, b) argues that the introduction of the small group piecework wages was the change in reinforcement that elicited the learning process. By itself, this change in reinforcement conditions was probably not sufficient, however, as evidenced by what occurred in the bank wiring room experiment. When the company instituted a small group piecework

wage rate (based on the 14 men in the department) the workers in the bank wiring room continued to produce at about the same rate as before. They were threatened by the incentive plan and the specter of higher production. Rewards sought by workers are far wider in scope than money alone and social rewards such as the status of the work group within the organization and positive interpersonal relations are also important (Dunphy; Homans). A worker's orientation to work depends, in part, on whether he feels himself to be a member of a high status group that is respected by management. In the first relay assembly experiment the workers' status was enhanced. They participated with management (the experimenters) in decisions, viewed themselves as respected and thus developed attitudes of cooperation and support. In the bank wiring room experiment the workers felt that management viewed them with indifference or hostility and as a result they responded with hostility in limiting output (Shils). The implementation of participative management and the small group piecework rate were undoubtedly both necessary changes in the conditions of reinforcement. Although these factors do not directly account for the continuous long-term increases in productivity, they were important in motivating the workers to learn more efficient operating procedures.

The fact that the operators in the relay assembly room were provided with regular feedback concerning their performance has been ignored in most analyses of the experiments. In interviews with Parsons, Donald A. Chipman, an observer-supervisor in the experiment, and Theresa Zajac, one of the five operators, both indicated that accumulated half-hourly output totals were available to the operators, that operators did get up and check these totals if they believed their production to be above or below normal and that the daily output of each worker was reported to her at the end of the day or the next morning (Parsons, b). There is ample research evidence that regular performance feedback results in improved efficiency (cf. Hamner and Hamner). The almost concurrent introduction in the test room of these two necessary preconditions for learning (better reinforcement conditions and performance feedback) is strong substantive support for the hypothesis that the overall trend was due to learning.

Presumably the complex relay assembly task permitted a considerable increase in production rate by developing and varying assembly procedures, by speeding up manipulation movements, or by reducing non-productive activities. Operating procedures did vary between operators and for each operator over time (Parsons, a). Thus, although the operators were experienced workers and had had fairly constant productivity in the "regular department," it is plausible that the facilitative conditions in the test room elicited a learning epoch.

Yet, the question remains—do the time-series data support the collective learning hypothesis? Neither Parsons (a, b) nor Franke and Kaul empirically tested this hypothesis. Since the process of cumulative collec-

tive learning over time or practice has been shown to be a very regular process that can be described mathematically (cf. Baloff, a, b; Towill), an appropriate empirical test of Parson's hypothesis would be to fit a standard learning curve to the output data.

### The Standard Learning Curve

One equation that has been shown to accurately describe trends of collective and organizational learning in a variety of experimental and natural settings, including industry (cf. Baloff, a, b; Hamblin et al., a, b; Jordan), is the power equation. A mathematical model of collective learning specifying some simplifying assumptions about the learning mechanisms and processes underlying the derivation of the power equation has been developed by Hamblin and Pitcher. The dynamic collective learning process underlying the power equation may be specified as follows:

$$dL/L = h dP/P. \quad (1)$$

This assumes simply that the relative improvement in performance ( $dL/L$ ) is a constant proportion ( $h$ ) of the relative increase in practice ( $dP/P$ ). To apply this learning model to the Hawthorne data, the variables are operationalized as follows: total output ( $Y$ ) is a measure of performance and the accumulated number of hours worked in the relay assembly test room ( $T$ ) is a measure of accumulated practice. Then, according to the power equation model of learning, any relative increment in output ( $dY/Y$ ) is a constant proportion ( $h$ ) of the relative increment in hours worked ( $dT/T$ ) as specified below:

$$dY/Y = h dT/T. \quad (2)$$

To estimate the collective learning rate ( $h$ ) and the fit of the learning equation to the data, the integrated equation corresponding to equation (2):

$$Y = a T^h, \quad (3)$$

is usually analyzed using cumulative data. However, to analyze the trend in hourly output ( $dY/dT$ ) we use the following equation which is obtained by differentiating equation (3):

$$dY/dT = aT^{h-1} \text{ or } \ln (dY/dT) = \ln a + (h - 1) \ln T. \quad (4)$$

With data on hourly output ( $dY/dT$ ) and cumulative hours worked ( $T$ ) it is possible to estimate equation (4) and thus empirically test the fit of the learning equation to the observed output data.<sup>3</sup>



## RESULTS OF THE LEARNING ANALYSIS

Substitutions and changes in operators make averaging across operators invalid for assessing a group learning process, therefore it is only appropriate to analyze the individual data for each operator. For the analyses on operators 3 and 4 data are available for all 23 periods. Data for the periods 14–16 were excluded in the analyses for operator 5 because she was absent from the test room during most of these periods. Operators 1 and 2 did not enter the test room until the beginning of period 8 so analyses for these operators only involved data on periods 8–23. The results for these learning analyses are presented in Table 1.

This set of analyses indicates that the learning trend accounts for well over 80 percent of the variance in output/hour for the three operators who were in the test room from the beginning. For the two replacements (operators 1 and 2), 60–70 percent of the variance in output/hour is accounted for by learning. The differences in  $r^2$  for these two sets of operators are mainly due to differences in the total variance in output. The standard deviations of the residuals are very similar for all five operators.

For operators 3–5 the level of fit ( $r^2$ ) is quite comparable to results of other learning analyses using rate data (cf. Hamblin et al., b). The fits for operators 1 and 2 are somewhat lower. Equation 4 assumes that the work conditions relevant to learning were constant throughout the experiment and to the degree that this assumption is not true—i.e., experimental

**Table 1.** RESULTS FOR THE LEARNING MODEL ESTIMATING OUTPUT RATE ( $dY/dT$ ) AS A POWER FUNCTION OF CUMULATED HOURS WORKED IN THE TEST ROOM ( $T$ )\*

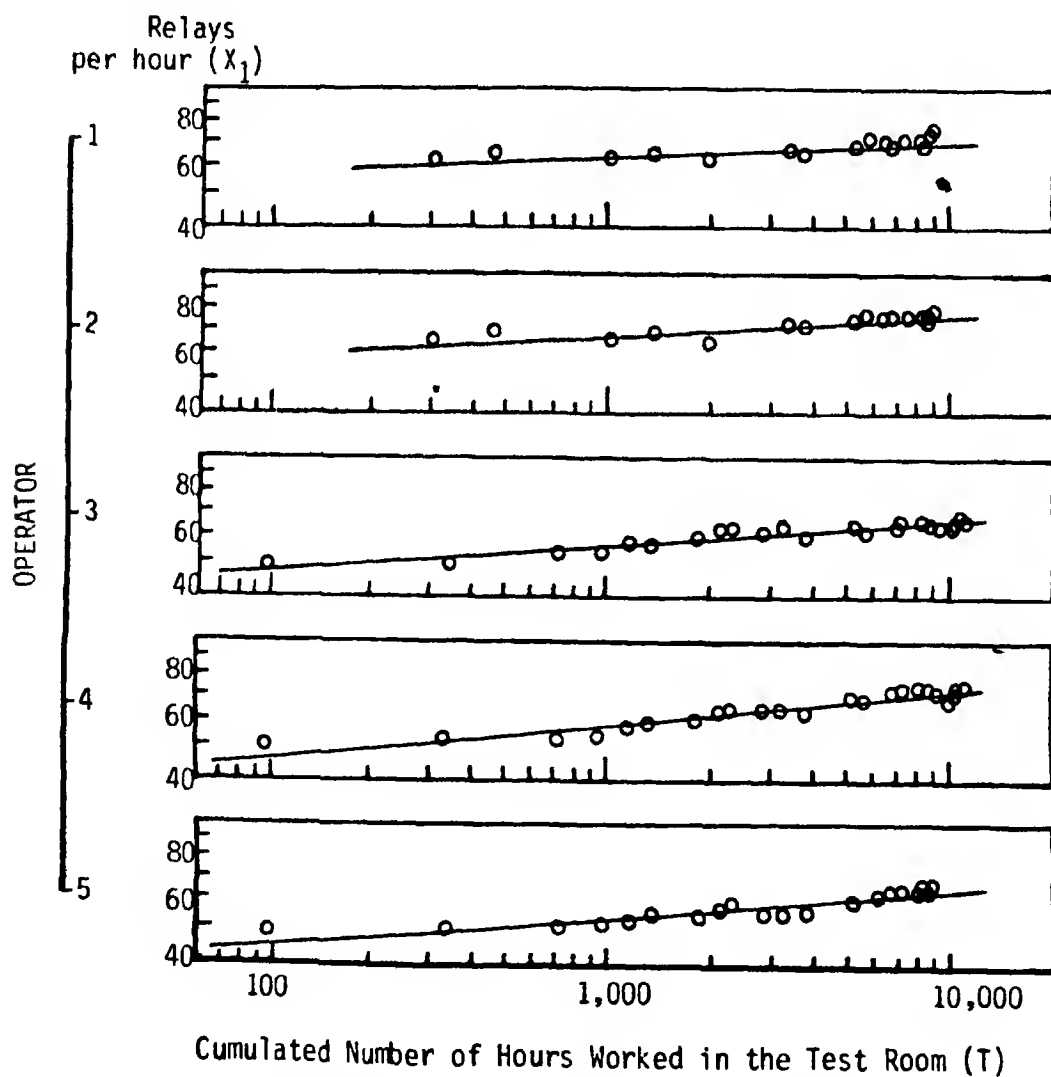
Data Set	a	Coefficient	$r^2$
		$h-1$	
Operator 1	46.83	.047 (.019)	.628
Operator 2	45.86	.055 (.018)	.736
Operator 3	34.44	.071 (.011)	.883
Operator 4	29.24	.097 (.015)	.887
Operator 5	30.34	.082 (.017)	.835

$$\text{Equation: } \ln (dY/dT) = \ln a + (h-1) \ln T$$

\*The number in parentheses is the value of two times the standard error of the estimate.

manipulations and other changes interfered with motivation and the normal learning process—then the level of fit should be lowered. Nevertheless, an examination of Figure 2 indicates that the power learning equation accurately accounts for the overall productivity trend for all five operators.<sup>4</sup>

This evidence that the trends in the output data are described by a substantively derived learning equation is strong empirical support for Parson's hypothesis that the long-term increases in productivity were largely due to an adaptive learning process. At the same time, however, the similar patterns among operators in the scatter of the residuals (see Figure 2) suggests the possibility that discrete changes during the experiment may have had significant independent effects on productivity. The effects of these experimental and inadvertent changes are evaluated below.



**Figure 2.** Plot of log-transformed learning curves for operators 1-5 predicting improvement in performance (hourly output of relays [ $X_1$ ]) as a power function of practice (cumulated number of hours worked in the relay assembly test room [ $T$ ]). The circles indicate the observed output for each period and the solid line represents the expected output according to the estimated learning equation. The learning equation parameters for each operator are given in Table 1.

## Evaluation of Other Hypotheses

In time series experiments it is advisable to use steady state designs where the experimenter waits until learning has reached asymptote and the response rate is steady before changing the experimental conditions. Comparisons are then made between the steady state levels. These procedures were not followed in the first relay assembly room experiment and thus the effects of the experimental manipulations were confounded with the learning effect. For this reason in order to accurately assess the effects of the experimental manipulations and other inadvertent changes, it is first necessary to control for the historical learning process, the trend.

In the causal analysis of time series data where one is investigating whether changes in one variable ( $X$ ) cause later changes in another variable the ( $Y$ ) investigators should always first examine for trends. According to the rules of plausibility and parsimony (cf. Mark), it is generally better to assume that present values of  $Y$  are due to some regularity in the behavior of  $Y$  over time rather than that they are due to another variable ( $X$ ). For these reasons the obtained relationships between trended series are frequently spurious and it is almost always necessary to detrend time series prior to causal analysis.<sup>5</sup>

Schlaifer suggests that the exact form of the continuous function used to model trend in the Hawthorne data is "not at all critical," as long as it is capable of representing increase at a decreasing rate which becomes nearly asymptotic sometime in the year 1930. In mathematical sociology, however, it is always preferable to use an equation that is derived from a substantive theory and which specifies the basic mechanisms of the growth process over time (Sorensen and Sorensen). For this reason, in this analysis the power equation derived from mathematical learning theory is considered preferable for detrending the time series.

As noted above, (see Figure 2), the power equation provides an accurate representation of the long-term learning effect. Therefore it will be assumed that the residuals from the OLS regression line reflect variations in output from sources other than learning. These residuals are indicators of fluctuations in productivity after controlling for the learning effect and they can be analyzed to assess the nonspurious effects of the experimental manipulations and the other inadvertent changes. With these detrended data, a set of analyses to reassess Franke and Kaul's hypotheses as well as those of the original experimenters were carried out and are summarized below.

## RESULTS OF THE ANALYSIS OF THE DETRENDED DATA

*Zero-Order Relationships*

The correlations between the detrended output data for each operator and various explanatory variables are presented in Table 2. There is considerable variation among individuals in the magnitude of the correlations but the directions of the relationships are as expected. For example, longer work days ( $X_4$ , median  $r^2 = -.423$ ) and longer work weeks ( $X_6$ , median  $r^2 = -.218$ ) are negatively associated with output. This evidence supports the effects of fatigue hypothesized by Argyle and Carey. Also, scheduling rest time ( $X_9$ , median  $r^2 = .348$ ) tends to increase productivity above the level expected by the learning effect. Ironically, this was one of the main working hypotheses of the original researchers, one which they concluded was not supported by their data. The effects on output of the personnel change variable ( $X_{11}$ , median  $r^2 = .274$ ), and the economic depression variable ( $X_{12}$ , median  $r^2 = .361$ ) both tend to be positive.<sup>6</sup>

For all but operator 5 the variable most strongly associated with the detrended output is the amount of voluntary rest time taken ( $VR$ , median  $r^2 = -.553$ ). This variable might be viewed as an adjustment to learning and work time. Time taken for voluntary rests (trips to the lavatory, visiting the water fountain, etc.) was time lost from learning but was counted as work time in computing output rates.

*Multivariate Analysis*

Not surprisingly, the variables  $X_4$ ,  $X_5$ , and  $X_6$ , alternative measures of duration of work, are highly interrelated. Also, the variables measuring scheduled rest time ( $X_8$  and  $X_9$ ) and voluntary rest time ( $VR$ ) are quite highly interrelated. Because of this multicollinearity if two or more measures of duration or two or more measures of rest time are incorporated in the same multivariate analysis the precision of the estimated coefficients will be reduced (see Johnston). For this reason, only one measure of variation in duration of work and one measure of variation in rest time were used at a time in the analyses described below.

In the first set of multivariate equations, the detrended output variable ( $R$ ) was regressed on the variables: hours per day ( $X_4$ ), scheduled rest time ( $X_9$ ), personnel change ( $X_{11}$ ), and economic depression ( $X_{12}$ ). Only for operator 5 was the regression coefficient for any of these variables statistically different from zero. The best equation for operator 5 seemed to be:

$$R_5 = 54.542 - 6.286X_4 - 2.487X_{11} \quad R^2 = .580 \quad DW = 1.615$$

Because an alternative measure of rest time, voluntary rest time ( $VR$ ), seemed to be a better predictor of output than scheduled rest time ( $X_9$ ), the detrended output variable ( $R$ ) was then regressed on the variables  $X_4$ ,  $VR$ ,  $X_{11}$  and  $X_{12}$ . The coefficient for the  $VR$  variable was statistically

Table 2. ZERO-ORDER CORRELATIONS FOR THE DETRENDED OUTPUT AND VARIOUS VARIABLES MEASURING EXPERIMENTAL, INADVERTENT AND NATURAL CHANGES DURING THE HAWTHORNE FIRST RELAY TEST ROOM EXPERIMENT

	R <sub>1</sub>	R <sub>2</sub>	R <sub>3</sub>	R <sub>4</sub>	R <sub>5</sub>	X <sub>4</sub>	X <sub>5</sub>	X <sub>6</sub>	X <sub>8</sub>	X <sub>9</sub>	X <sub>11</sub>	X <sub>12</sub>
X <sub>4</sub>	-.515	-.423	-.135	-.113	-.666							
X <sub>5</sub>	-.194	.016	.090	.006	.591	.648						
X <sub>6</sub>	-.370	-.218	-.047	-.069	-.631	.836	.943					
X <sub>8</sub>	.442	.631	.120	-.023	.064	-.117	-.101	-.232				
X <sub>9</sub>	.442	.631	.348	.210	.145	-.402	-.347	-.534	.762			
X <sub>11</sub>	. . .	. . .	.274	.311	.151	-.520	-.449	-.566	.063	.531		
X <sub>12</sub>	.377	.361	-.054	.202	.588	-.656	-.693	-.750	.119	.408	.527	
VR <sub>1</sub>	-.553					.175	-.003	.161	-.842	-.842	. . .	-.084
VR <sub>2</sub>		-.741				.538	.301	.528	-.899	-.899	. . .	-.533
VR <sub>3</sub>			-.670			.291	-.029	.220	-.220	-.672	-.447	-.021
VR <sub>4</sub>				-.485		.518	.272	.501	-.690	-.771	-.359	-.515
VR <sub>5</sub>					-.115	.055	-.305	-.087	-.053	-.498	.058	.349

R<sub>1</sub>-R<sub>5</sub> = detrended output for each of operators 1-5, X<sub>4</sub> = hours per day; X<sub>5</sub> = days per week; X<sub>6</sub> = net hours per week; X<sub>8</sub> = scheduled rest stops per day; X<sub>9</sub> = scheduled rest stops, minutes per day; X<sub>11</sub> = personnel change, X<sub>12</sub> = economic depression; VR<sub>1-5</sub> = voluntary rest time, minutes/day, for each operator 1-5.

significant for operators 1–4 but again none of the other variables contributed significantly to describing the variance in the detrended output. To check this finding the dependent variable ( $R$ ) was regressed stepwise first on the voluntary rest time variable ( $VR$ ) and then on the other seven independent variables identified in Table 2. For operators 1–4 after the effects of  $VR$  had been taken into account, no variable contributed significantly to account for variation in output ( $R$ ). These results indicate that voluntary rest time ( $VR$ ) was the only variable with a significant effect on variations in the detrended output (see Table 3).<sup>7</sup>

According to these analyses, rest time had an important effect on variation in output; however, reductions in voluntary rest time were more important than increases in scheduled rest time. The correlations between scheduled and voluntary rest time (median,  $r^2 = -.771$ ) implies that increasing scheduled rest time had the tendency to reduce voluntary rest time. It appears that the primary effect on productivity of scheduling rest stops was not to reduce the fatigue factor, as assumed by other investigators, but was simply to eliminate much of the nonproductive or lost time taken as voluntary rest time.

The differences between the results for operator 5 and others are noteworthy and should be considered further. Throughout the experiment operator 5 consistently ranked last in production, but as noted above, she had more different types of relays to assemble than the other operators.

**Table 3.** REGRESSIONS OF THE DETRENDED OUTPUT ( $R$ ) ON VOLUNTARY REST TIME ( $VR$ )<sup>a</sup>

Data Set	a	Coefficients		Durbin Watson
		b	$r^2$	
Operator 1	2.158	-.420 (.382)	.306	1.811
Operator 2	3.158	-.441 (.222)	.549	1.851
Operator 3	2.410	-.351 (.178)	.448	2.225
Operator 4	2.566	-.295 (.244)	.235	.939**

Equation:  $R = a + b VR$

<sup>a</sup>The number in parentheses is the value of two times the standard error of the estimate.

<sup>\*\*</sup>This Durbin-Watson test indicates that the null hypothesis of no autocorrelation should be rejected for operator 4. In conditions of autocorrelation OLS yields biased estimates of the coefficient variances. Since this bias is generally negative, the estimated variances and standard errors may be understated causing coefficients to appear significant when, in fact, they may not be. This possibility should be considered in interpreting the results for operator 4.

She tended to be quiet and participated less than the other girls in the social activities of the test room (Roethlisberger and Dickson). Consequently, she was probably affected less by the small group processes such as imitation, mutual support, or group pressures. Other differences she had with the other girls included the fact that she was a recent immigrant from Northern Europe, married, lived out of town and resided with friends during the work week, and did not have to work because her husband earned enough to support them both at their accustomed standard of living. Given these conditions, it is not surprising that it was the shorter work days and weeks that tended to stimulate her production. She was very deliberate and persistent in her work. Also, her voluntary rest time was minimal and varied much less than for the other operators.

## Conclusions

The results of the above analyses are consistent with conclusions of others (Franke and Kaul; Schlaifer) that most of the variance in production rates during the first relay experiment can be explained by measurable variables. The alternative statistical time series analyses presented here, however, indicate that the rise in production rates was due to quite a different mechanism: a learning process elicited by the new work conditions within the test room.

The first analysis reported in this paper shows that most of the variance in production rates was due to the long-term trend. It was hypothesized with Parsons (a) that this trend was due to cumulative learning motivated and facilitated by (1) improved reward conditions within the rest room—i.e., economic incentives for increased production, participative management, and positive interpersonal relations—and (2) regular information feedback. The hypothesis that learning or improved skill in assembling relays occurred in the test room is clearly supported by the information available on the experimental conditions (cf. Hamblin et al., a; Parsons, a, b). In addition, the good fit of the previously established power equation model of learning to the output data for five operators is empirical evidence of a learning effect. Further analyses indicate that although the experimental changes of shortening the work day and introducing rest pauses were positively correlated with the detrended output data, the effects of these variables were not statistically significant in a multivariate model. The only variable that had a consistent significant effect on variations in the detrended output data was voluntary rest time.

The hypothesized effects of Franke and Kaul's main factors, managerial discipline and the onset of the economic depression, were not supported by the analyses in this paper. In the first place, the replacement of operators 1a and 2a was probably welcomed by the other operators and

thus it is tenuous to consider it an act of managerial discipline. Nonetheless, this variable had a significant effect on the detrended output data only for operator 5 and in that case the effect was not positive as predicted. Also, the economic depression variable did not contribute significantly to accounting for the variance in the detrended output data for any of the operators. Franke and Kaul's own evidence for the effects of these variables was based on faulty epistemic and statistical assumptions (see notes 1, 5, and 6).

An implication of the analyses in this paper and the assumptions on which they were based is that a multiple causation approach (cf. Shepard) is necessary to account for the results of the Hawthorne experiment. The variation in output rates during the first relay assembly experiment are not fully explained by focusing exclusively on either cooperation or conflict management perspectives. The substantive and empirical analyses provide evidence that learning was responsible for the overall trend and this learning was stimulated by a combination of causes. The almost concurrent changes in economic incentives and statuses of the operators created new reinforcement contingencies rewarding and thus motivating cooperation and task productivity. The regular information feedback was necessary to permit operators to evaluate their performance, to identify their effectiveness, and to eliminate nonproductive activities. Thus, all of these conditions were probably necessary for the learning that occurred and there seems to be no direct way to isolate and evaluate their separate effects in this experiment. Further empirical analyses in this paper suggested that the change in physical work conditions resulting from the introduction of rest pauses also led to the more productive use of work time in assembling relays. It is evident, however, that close managerial control, an important variable according to the conflict perspective, was not associated with the increase in productivity.

As noted by Franke and Kaul, the statistical analysis of quantitative information is a useful tool for investigating and evaluating processes and theories. Therefore, to reappraise the various interpretations of the Hawthorne experiments the data should be subjected to statistical analysis. Statistical analysis is meaningless, however, when the substantive evidence does not support the assumptions underlying the analysis. In any investigation the substantive as well as the empirical evidences for various explanations need to be critically evaluated before drawing conclusions. Franke and Kaul in their analysis interpreted the replacement of operators 1a and 2a as a managerial act threatening to the other operators. The qualitative information recorded by the experimenters does not support this assumption. Franke and Kaul also failed to recognize that categorical changes do not create long-term, almost uninterrupted growth trends such as those evidenced for the output data in this experiment. Finally, apparently Franke and Kaul did not consider whether the trend in the data was



somehow associated with regularities over time. Franke and Kaul's emphasis on the importance of quantitative analysis is apt, but only *when the analysis is appropriate and is supported by the available qualitative evidence*. As we have seen, when appropriate and thorough analyses are conducted for the first relay assembly experiment, the zero order relationships that supported the experimenters' original hypotheses (concerning the effects of rest stops and shorter work days and weeks) as well as the *post factum* hypotheses of Franke and Kaul (about the effects of the personnel change and the onset of economic depression) are spurious—i.e., insignificant in the context of other variables in the multiple regression analyses.

Although this analysis supports Parsons' thesis that the overall trend in productivity was due to adaptive learning, it should not be concluded that learning is the main explanatory variable. The important explanatory variables are rather (1) the increased economic status, and interpersonal rewards that motivated the operators to learn and to adopt more efficient work procedures and habits, and (2) the regular performance feedback which facilitated the expertise learning. Learning was probably a regulating process or mechanism, rather than a cause of increased productivity.

## Notes

1. Although Franke and Kaul's two dichotomous variables (managerial discipline and economic depression) correlate highly with the upward trend in output, given the continuity of the trend, one would not ordinarily infer a causal effect for these dichotomous variables (Campbell and Stanley). Such relationships are usually spurious. Franke and Kaul did not analyze the data in a way that tests for that spuriousness.
2. Increases in output rate were certainly within the potential of operators assigned to the relay assembly task. In the test room when operators were requested to work as fast as they could for short periods, their rates rose considerably (Parsons, b).
3. The data for this analysis are available from Franke and Kaul (Appendices). Their variable  $X_1$  is the measure of hourly output ( $dY/dT$ ) used in this analysis. Cumulative number of hours worked ( $T$ ) was calculated by multiplying weeks per period ( $X_7$ ) by net hours worked per week ( $X_8$ ) and cumulating these products across all the periods of the experiment.
4. Other analyses were conducted to determine whether alternative learning equations—i.e., a linear equation or a decaying exponential equation—would do a better job of describing the trend. The linear equation did not provide a good fit to the data for operators 3 and 4. Both of these data sets are clearly curvilinear. For this reason the linear equation was rejected. The fits ( $r^2$ ) for the decaying exponential equation were very comparable to the fits for the power equation. In this case, however, the parameter estimates for the asymptotic level of learning were unreasonably high, thus the decaying exponential equation model was also rejected.
5. It appears that Franke and Kaul did not consider the problem of trends in the time series data. This is a major flaw of their analyses, and, as we shall see, apparently led to faulty conclusions.
6. In their paper Franke and Kaul refer to the replacement of operators 1a and 2a as managerial discipline. As argued by Wardwell and Schlaifer, the effect of replacing the two insubordinate workers was mainly an improvement in ability and cooperation and should not be interpreted as an abrupt reassertion of aggressive supervision that increased insecurity in the remaining workers. The removal was a punishment for operators 1a and 2a but not for operators 3, 4 and 5. The latter three girls had been openly critical of the other two for not doing their share and had even requested the authorities to remove one group or the other group from the experiment (Roethlisberger and Dickson, 53–55, 169). The fact that management removed operators 1a and 2a was undoubtedly rewarding for operators 3, 4 and 5 and increased their

received status relative to management. For these reasons in this paper the variable corresponding to the replacement event is referred to simply as "personnel change."

7. Because of evidence indicating that social stimulus-response relationships are frequently multiplicative rather than additive (cf. Hamblin), all the variables in Table 2 (except the dummy variables,  $X_{11}$  and  $X_{12}$ ) were log transformed. The analyses detailed in the above section were then replicated using the log transformed data. The multiplicative equation results concerning the relative effects of the alternative variables were consistent with the results of the analyses using additive equations but in all cases the amount of variance explained ( $R^2$ ) was lower. For these reasons and to facilitate comparisons with Franke and Kaul's analyses, only the results from the analyses using additive equations are reported here.

An alternative procedure that was not tried here would be to use a nonlinear regression program to regress output on time (with an exponential coefficient, i.e.  $T^{a-1}$ ) and all other predictors together.

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# Wives' Employment, Household Behaviors, and Sex-Role Attitudes\*

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## ABSTRACT

*Using spouses' telephone interview responses (N=1364, December 1978), we test the effect of wives' ten-year work attachment on their current employment status, and the effect of wives' work attachment, current employment status, and earnings on perceptions of household decisionmaking, the household division of labor, and on sex-role attitudes. Our most important findings are that work attachment, current employment status, and earnings affect husbands' but not wives' perceptions of decisionmaking, that both spouses' perceptions of the household division of labor are affected more by wives' current employment status than by their work attachment or earnings, and that attitudes most closely related to wives' employment are most responsive to it. These findings imply that attitude and behavior change tend to occur on pragmatic rather than ideological grounds.*

The massive increase in women's labor force participation (LFP) during recent decades is having major consequences for U.S. society in such seemingly diverse matters as fertility rates (Ward and Butz), the reappearance of the women's movement, and the rise in property crime rates owing to the increase in the number of dwellings unoccupied during the day (Cohen and Felson). Norms for men's and women's behavior are also changing (Mason et al.), presumably due in large part to new work patterns. Just how these trends relate to behavior and attitude changes of individuals in households remains unclear. Do people who experience new patterns also change other behaviors and attitudes, or are the consequences of these trends more diffuse and long term?

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Research on the consequences of women's LFP for individuals has tended to focus on effects of their current employment status on attitudes (Huber et al.; Mason and Bumpass) or on household behaviors (Bahr). Yet current employment status is only one aspect of a woman's LFP that could affect spouses' behavior and attitudes. In our view at least three aspects of wives' LFP could have important individual effects: current employment status, work attachment during a given period, and level of earnings.

First, a wife's current employment could induce pragmatic changes due to rescheduling of household activities and time constraints. A husband might wash the supper dishes and bathe the children because his wife had a night job. Hence her current employment status might exert mainly a temporary effect on the household division of labor rather than a longer-term effect that would include attitudes about role obligations. Family differences therefore would be most clear between those in which the wife was currently employed and those in which she was not employed, whatever her past labor force status.

Second, wives' employment could cause gradual and cumulative changes such that wife's work attachment over time (LFP in the last 10 years) would affect household behaviors and attitudes more than would her current employment status. A woman's current labor force status, unlike a man's, may not adequately indicate her labor market behavior over time (Maret-Havens). In contrast, a work attachment index could reflect seasonal, part-time, discontinuous aspects of women's LFP patterns. A woman's current employment status might imply little about her participation next year or last. An overall female LFP rate of 50 percent might imply that half the women work continuously or that every woman works half-time. If wives' employment induces behavioral and attitudinal changes that persist even if they are not employed currently, family differences would occur mainly between those in which the wife had been employed throughout most of a recent time period and those in which she had not, whatever her current status.

Third, a wife's employment may have few effects unless her earnings are high enough to affect the family balance of power. If so, a wife's earnings, or earnings potential, would relate more strongly to attitudes and behaviors than either her current employment status or work attachment, implying that attitude and behavioral change needed for women's social equality may depend more on their chances to enter the best-paid jobs rather than on their chances to do market work.<sup>1</sup>

Using a national probability sample of married couples, our purpose is to test the effect of work attachment on current employment status, and the effect of work attachment, current employment status, and earnings on both spouses' attitudes and behaviors. We now outline a theoretical rationale for the specific hypotheses tested in this study.

#### THE RELATION OF WORK ATTACHMENT AND CURRENT EMPLOYMENT

Past research reports that women's past and current LFP relate strongly (Ben-Porath; P. Cain; Macke et al.; Mahoney; Waite, b). This finding seems trivially true but in fact it implies either that factors determining past employment remain unchanged or that past LFP itself affected current employment status, reinforcing the likelihood that a woman will or will not work for pay.

The factors that affect women's LFP are less stable than those affecting men's because of women's domestic responsibilities: current family status, including ages and number of children (Sweet), husband's income (G. Cain), his attitudes about her employment (Morgan et al.), her marketability—education, job training, experience (Sweet), local labor market conditions (Bowen and Finegan), and her personal market work preferences (Dowdall; Waite, b). Although education may remain constant after a certain age, job skills may improve or deteriorate (Rosenthal). Women may move to regions with different female employment opportunities (Long). Children age; others may be born. Further, some of these factors may respond to labor force experience: a wife's or her husband's preferences, the presence of preschoolers, increased labor market knowledge, or the opportunity cost of nonemployment (Waite, a).

The efficacy of work attachment in predicting current employment status should depend on the stability of the factors noted above, on other unmeasured factors, and on the quality of the work attachment measure. The first part of our analysis will therefore examine the relation between work attachment and current employment status. We expect to find that a wife's work attachment will affect her current employment status independently of the relatively stable factors captured by the measure.

#### HOUSEHOLD BEHAVIORS

Next we examine the extent to which women's current employment status, work attachment, and earnings affect marital decisionmaking and the division of household labor. Studies of decisionmaking or family power have been plagued by methodological problems with substantive consequences. Typically, only wives are interviewed; resulting biases are then incorporated into the research (Safilios-Rothschild, a). Moreover, decisionmaking scales have often combined trivial daily decisions, such as what to serve for supper, with major infrequent ones, such as whether to move to another city (Blood and Wolfe; Safilios-Rothschild, b). Not surprisingly, analyses of the relation between decisionmaking and wives' employment report mixed results; some researchers report more power for employed wives (Bahr) and others report high levels of egalitarianism across the board (Blood and Wolfe).

Due to time constraints, employed wives should make fewer trivial daily decisions than do nonemployed wives but time constraints should not affect major decisionmaking. On commonsense grounds, we would expect that the higher the absolute value of the wife's earnings and the longer her record of work attachment, the more voice she would be perceived to have in family decisions. We thus expect wife's earnings and work attachment to affect perception of decisionmaking most and wife's current employment status to affect it least.

We then examine the effect of wives' employment on the division of household labor. Research based on time budget studies shows that employed wives' husbands spend little more time doing housework than do other husbands and neither group does very much (Vanek; Walker and Woods). But employed wives work fewer hours at home than do nonemployed wives; hence their husbands' *relative* contribution is higher (Pleck). Since time budget data are costly to collect, such studies to date have included few other variables—at most, a few demographic but not attitudinal ones (Berk and Berk; Stafford and Duncan).

We expect that wives' employment will affect husbands' and wives' relative contributions to household tasks for several reasons. First, a currently employed woman, having less time at home, may cut corners or receive more help from her husband, changing their relative contributions. Second, her long-term employment may affect spouses' attitudes about appropriate home work roles for husbands and wives such that one or both favor a more equal division of labor. And third, her earnings may give her the bargaining power to demand more help from her husband, whatever his attitudes or preferences (Farkas). As with our decisionmaking hypothesis, we expect current employment status to exert weakest effects and wife's earnings and work attachment, the strongest.

#### SEX-ROLE ATTITUDES

Last, we examine effects of current employment status, work attachment, and earnings on five sex-role attitudes. A wife's employment may change her own or her husband's attitudes about sex roles for several reasons. An employed wife or her husband may be cognitively uncomfortable if he opposes married women's employment. Particularly if the household needs her earnings, the husband may change his views; hence, the higher her earnings, the more likely such a change might be. Further, attitudes may change gradually but permanently over time as a woman's employment becomes more acceptable to her family and as their definitions of women's roles broaden. We expected long-term consequences to be more diffuse than immediate ones from current employment, perhaps spanning a wider set of issues.

We examine five issues ranging from those specifically related to a

wife's employment to general sex-role issues. First, a husband's attitude to his wife's employment is both immediate and specific and should respond to both her current and past employment (Ferber). Second, each spouse's attitudes toward married women's employment are more general but still pertinent to the wife's current employment status (Macke et al.; Mason and Bumpass; Mason et al.). Third, a norm on family financial support as part of a wife's role may have begun to develop. If wives' employment promotes such a norm, we would expect the change to occur over the long run, not in response to brief fluctuations in her labor force status.

The fourth and fifth issues are more remote from household affairs. The ERA relates to women's employment as well as to other general issues. Indeed, a wife's current employment status affects her husband's but not her own ERA attitude (Huber et al.). Whether ERA attitudes are affected by work attachment or earnings would again be determined by how diffuse such effects are. Finally, abortion availability, which feminists and many social scientists see as a necessary condition for women's equality, may also be a religious issue. Attitudes about abortion may be an ultimate test of how diffuse are any effects of work attachment on current attitudes.

The causal order of current sex-role attitudes and current employment behavior must be justified because, while women's employment decisions cannot be affected plausibly by attitudes on such issues as the ERA or abortion, they could be affected by attitudes toward maternal employment. The only adequate nonrecursive test of this relation reported no significant effect in either direction but a near-significant path from labor force status to sex-role attitudes (Molm).<sup>2</sup> On logical grounds, we also assume that any impact runs from current or past employment to household division of labor and decisionmaking, rather than the reverse.

In sum, this research tests five hypotheses: (1) that a wife's work attachment affects her current employment status independently of the stable factors measured; (2) that current employment status will have least effect, work attachment and wife's earnings the strongest effect on spouse decisionmaking; (3) on the division of household labor; and (4) on sex-role attitudes. We now add a fifth hypothesis: husbands' attitudes will be less affected by wives' employment than will wives' attitudes. Whatever changes wives' employment makes in their husbands' lives, it necessarily makes more in their own; also, some of their work experience may have occurred before marriage.

## **Data and Methods**

Our data come from a national U.S. probability sample ( $N=2,002$ ) interviewed by telephone in November 1978. If possible, both husbands and wives were interviewed. This study uses data only from these 682 couples



because we want to test the effect of wives' employment on both husbands and wives.<sup>3</sup> The wives ranged in age from 18 to 66 years, with about one-third under 30—thus with less than 10 years potential work time, particularly for the college-educated. The late twenties are also likely years out of the labor force for those who remain nonemployed when preschool children are present. This would present problems if some of the variables used here interact with age in their effects on attitudes or behavior, i.e., if the meaning of work varies during different life stages. When equations in the present analysis were reestimated including work experience–age interaction terms, none of these terms was significant. But this problem should be kept in mind when interpreting the findings.

Another interpretation problem concerns the use of cross-sectional data. Ideally, we would examine *changes* in attitudes and behavior over a time period from work experience during that period (e.g., see Macke et al.; Spitze and Waite); however, no such data are available for the variables we used or for a sample of married couples. Thus when we discuss effects of work attachment on attitudes and behavior, this limitation must be kept in mind.

We estimate three separate sets of equations with the following dependent variables: wife's current employment status; husbands' and wives' perceptions of household decisionmaking and the division of household labor; and both spouses' attitudes on issues related to women's employment. We use regression analysis; the only equations with dichotomous dependent variables (ERA attitudes) have splits well within the range in which regression and other techniques have similar results (Goodman). Unstandardized regression coefficients are reported to allow cross-equation comparisons.

An adequate measure of a woman's work attachment should reflect a relatively long period of time so as not to be affected by a single brief labor force absence; it should also reflect seasonal and part-time variations (Maret-Havens). The first characteristic can be captured by years worked over a period (P. Cain; Macke et al.; Morgan et al.) and the second, by months (Mahoney) or weeks (Shaw) worked over the period. Using 1967–71 data for the National Longitudinal Survey of Mature Women 30–44, Maret-Havens captured all three characteristics by calculating an index based on number of years worked between leaving school and 1967 (maximum contribution to index=.50) and on number of hours per week and weeks per year between 1967 and 1971 (maximum combined contribution to index=.50). While the index varies nicely between 0 and 1, it has no direct intuitive interpretation.

Our index, based on a recent ten-year period, is more easily interpretable and varies between 0 and 10. Full-time year round (i.e., over 6 months, over 35 hours per week) work for one year counts as 1, while one-third of a point is subtracted for part-year work and one-third of a

point for part-time work for a year. These somewhat arbitrary choices resulted in only a 9 percent decrease from total years worked.

Current employment status is coded 1 for full-time, .5 for part-time, and 0 for not employed. Wife's earnings potential (most recent earnings) is coded from 1=less than \$5,000 to 7=more than \$30,000. Approximately two-thirds of wives' earnings were measured within 2 years of the survey date and most of the remainder were measured within 4 years of that date.<sup>4</sup>

In the equation for current employment status, control variables are wife's age and education in years; race (1=black, 0=other), husband's earnings (coded same as wife's); and dummy variables for age of youngest child, 0 to 2, 3 to 5, and 6 to 12 (the omitted category includes the childless and those with youngest child over 12).

Control variables for attitudes and behavior include the respondent's age, education, race, age of youngest child (as above), and husband's earnings. These equations are estimated alternatively including wife's current employment status, work attachment, and earnings. Their relative predictive power is determined by the increment in  $R^2$  when each is added to the equation.

Other dependent variables are measured as follows. Perception of marital decisionmaking was the sum of the husband's (H-DECISION) or the wife's (W-DECISION)<sup>5</sup> responses to the following questions, coded 1=wife always, 2=wife usually, 3=both equally, other, not applicable, 4=husband usually, 5=husband always: "We'd like to know who makes the following decisions in your family. Who decides:

- a. Where to go on vacation?
- b. What job the husband should take?
- c. What house or apartment to live in?
- d. Whether the wife should have a job?
- e. Whether to move if the husband gets a job offer in another city?"

These infrequent issues are of major importance to the family; they are therefore unlikely to be delegated by a more to a less powerful family member.

Reported perceptions of the household division of labor by the husband (H-HOUSEWORK) or the wife (W-HOUSEWORK) were measured as the sum of responses to the following items, which account for about three-quarters of the weekly time spent in housework (Walker and Woods), coded the same as the question on decisionmaking. "Certain things have to be done in every household. Please tell me who does the following tasks in your family. Who:

- a. Prepares regular meals for your household?
- b. Shops for food for your household?

- c. Takes care of the children or old people in your household?
- d. Does the daily housework?
- e. Cleans up after meals?"

Husband's attitude about his wife's employment (WIFEWORK) was measured by the following item, coded from 4=strongly opposed to 1=strongly favor: "Overall, how (do/would) you feel about your wife's working?"

Attitudes about women's employment (EMPATT) are measured as the sums of responses to the following questions, recoded so that 1=traditional, 4=nontraditional response.<sup>6</sup>

- a. It would be better for American society if fewer women worked.
- b. A woman who works can be just as good a mother as one who does not.
- c. A married woman should be able to have a job even if it is not always convenient for her family.
- d. It is more important for a husband to have a good job than for a wife to have a good job.
- e. There should be free childcare centers so that women can take jobs.
- f. By nature women are happiest when they are making a home and caring for children.
- g. A preschool child is likely to suffer if his or her mother works.
- h. It is much better for everyone involved if the man is the achiever outside the home and the woman takes care of the home and family.

Attitudes regarding normative pressures for women to work for pay (SHOULDWORK) were measured by total positive responses to the following four questions, new to research in this area: "In our society most people think that any able-bodied man should have a job. Do you think that an able-bodied woman should be *expected* to work if she is:

- a. Unmarried and has completed school?
- b. Married and has no children?
- c. Married and has no children under age 18?
- d. Married and has no children under age 6?"

Attitudes about ERA, coded 1=favor, 0=opposed, .5=don't know, were measured by this item: "Do you favor or oppose the Equal Rights Amendment?"

Attitudes about ABORTION were measured as total positive responses to "Please tell me whether or not you think it should be possible for a pregnant woman to obtain a legal abortion

- a. If there is a strong chance of serious defect in the baby?
- b. If she is married and does not want any more children?
- c. If the woman's own health is seriously endangered by the pregnancy?
- d. If the family has a very low income and cannot afford more children?

- e. If she became pregnant as a result of rape?
- f. If she is not married and does not want to marry the man?"

Findings

CURRENT EMPLOYMENT STATUS AND WORK ATTACHMENT

As expected, work attachment strongly predicts current employment status. For each year of the past 10 that a woman was employed, she is 7 percent more likely to be in the labor force currently (see Table 1, column 2). This relation is not perfectly linear but is monotonic (see Table 2).

Why does past labor force behavior so potently predict current status? Is it because the causes are fairly stable or does employment simply perpetuate itself? The answer is, some of each. A number of the standard predictors of women's labor force participation lose their predictive capacity when work attachment is included in the equation: race, husband's income, and education (see Table 1, columns 1 and 2). Presumably these factors are relatively stable in adulthood. Factors which fluctuate over time (age, and age of youngest child) still operate, even with work attachment in the equation. Thus work attachment predicts effectively in part because it captures effects of potent stable predictors.

Yet adding work attachment to the equation increases the explained variance from 14 to 37 percent, indicating either that important, unchanging causal factors have been omitted (e.g., regional or areal factors) or that

Table 1. UNSTANDARDIZED REGRESSION COEFFICIENTS  
PREDICTING WIFE'S CURRENT EMPLOYMENT STATUS

Independent Variables	1	2**
Age	-.010*	-.010*
Race	.193*	.072
Youngest child 0-2	-.496*	-.423*
Youngest child 3-5	-.271*	-.168*
Youngest child 6-12	-.137*	-.056
Education	.273*	.010
Husband's income	-.033*	-.015
Work index	. . .	.067
R <sup>2</sup>	.14	.37

\*p < .05.

\*\*Column 2 adds work attachment to the equation.

**Table 2.** WIVES' CURRENT WORK STATUS BY TEN-YEAR WORK ATTACHMENT (PERCENT)

Years Worked of Past Ten	Employed	Unemployed	Not Employed
0	3	3	92
1	26	12	60
2	40	4	55
3	58	8	33
4	58	19	23
5	60	4	35
6	62	13	20
7	72	12	14
8	72	15	13
9	75	4	21
10	90	6	4

employment is in some sense self-perpetuating; it may affect factors that are unmeasured here, such as wage potential, which tend to perpetuate a woman's being in or remaining out of the labor force.

#### DECISIONMAKING

We expected that the wife's current employment status would affect decisionmaking least and that the effects of all three variables would be stronger for wives than for husbands. We found, however, that current status, work attachment, and earnings affect only husbands' but not wives' perceptions of the relative decisionmaking power between spouses and that all three variables exert about the same effect on husbands (see Table 3, columns 1 and 2). Husbands' and wives' perceptions diverge; the correlation between the two scales is .27. This divergence may result from the influence of unshared norms between spouses. Husbands may feel that they earn the right to dominate decisionmaking by supporting the family and that wives earn the right to share decisions if they also work for pay. Wives, in contrast, may feel that their housekeeping contribution entitles them to as much decisionmaking power as would employment. But we cannot assess the accuracy of this interpretation.

Significant effects of other demographic variables on spouses' perceptions of decisionmaking are also inconsistent. While both less educated husbands and wives are more likely to see decisionmaking as husband-dominated, such perceptions are shared by older wives and younger husbands. Wives whose husbands earn less view their husbands as more

**Table 3.** UNSTANDARDIZED COEFFICIENTS PREDICTING HUSBANDS' AND WIVES' PERCEPTIONS DECISIONMAKING AND DIVISION OF HOUSEHOLD LABOR\*\*

Independent Variables	H-Decision	W-Decision	H-Housework	W-Housework
Age	-.025*	.016	-.007	-.023
Education	-.084*	-.068	.160*	.140*
Race	.448	-.270	.942	.921
Husband's earnings	.081	-.123*	-.153	-.012
Youngest child 0-2	-.004	.601*	-.810*	-1.551*
Youngest child 3-5	.004	.918*	-.965*	-1.504*
Youngest child 6-12	.005*	.240	-.660*	-.987*
R <sup>2</sup>	.04	.04	.09	.10
Wife's current employment	-.756*	-.161	2.434*	1.750*
R <sup>2</sup> increment	.02	.00	.11	.05
Wife's work attachment	-.087*	-.023	.224*	.155*
R <sup>2</sup> increment	.02	.00	.06	.03
Wife's earnings	-.304*	-.100	.703*	.529*
R <sup>2</sup> increment	.02	.00	.05	.03
N	681	681	682	681
Mean of dependent variable	16.2	16.2	9.9	8.9
Range of scale	5-25	5-25	5-25	5-25

\*p &lt; .05.

\*\*Coefficients for Age through Youngest child are taken from equations which include current work but neither the significance of coefficients nor the signs of significant coefficients differ when employment history or income is substituted.

dominant, inconsistent with the power-conferring function of earnings; but their husbands do not share this perception. And, finally, wives see themselves as less powerful if preschool children are present, a view not shared by husbands.

#### HOUSEHOLD LABOR

We expected current employment status to exert weakest effects and wife's earnings and work attachment, the strongest, on the division of household labor. But for both husbands and wives, wife's current status influences household labor more than do her earnings or past employment, consistent with a pragmatic interpretation of the situation. Wives working for

pay, particularly if full-time, must cut down their housework hours in order to get enough sleep. Whether husbands in turn perform more housework due to a shift in relative power or ideology is not clear from these data (see Table 3, columns 3 and 4).

However, perceptions of household labor are more highly correlated between husbands and wives ( $r = .65$ ) than are perceptions of decision-making, reflecting the more objective basis for them. Consistent with past research using this type of scale (Pleck), wife's employment is related to husband's relatively higher contribution. But husbands' perception of their contribution is more influenced by wives' employment than wives' perception is: effects of wife's current status are greater for husbands' than wives' perceptions, and wives' current status explains consistently more variance for husbands' than wives' perceptions. Again, whether this reflects overestimation of their own performance by husbands of employed wives or underestimation by wives is unknown. A few more hours of work a week may seem like a lot to husbands but little to employed wives. In any event the differences reflected here are not great.

Perceptions of household labor also relate to respondent's education and to the presence of young children. Highly educated wives received more "help" from highly educated husbands, and husbands help less when young children are present.

#### SEX-ROLE ATTITUDES

The effect of wives' employment variables on husbands' and wives' sex-role attitudes can be summarized briefly. First, attitudes most closely related to wives' employment are most strongly affected by wives' employment. Husbands' attitudes toward their wives' working and both husbands' and wives' attitudes to married women's employment are affected positively by the wife's employment. Attitudes less clearly related to employment are less affected (see Table 4).

Second, differences in explained variance between wives' current status, work attachment, and earnings are small and thus imply only slight differences in predictive power.

Third, wives' employment effects on husbands' and wives' attitudes are similar for employment-related attitudes but differ for peripheral issues. Both husbands' ERA attitudes and wives' attitudes toward abortion are affected by wives' current status. While these effects are small enough that they should not be overinterpreted, it seems reasonable that wives would more readily perceive a relation between abortion availability and employment since they are more likely than husbands to be aware of the consequences of an unwanted pregnancy. We have no explanation for the positive effects of wives' employment on husbands' but not wives' ERA attitudes.

Table 4. UNSTANDARDIZED COEFFICIENTS PREDICTING ATTITUDES OF HUSBAND AND WIFE\*\*

Independent Variables	Husband Attitudes				Wife Attitudes			
	Wifework	Empatt	Should Work	Abortion	ERA	Should Work	ERA	Abortion
Age	-.009*	-.064*	.007	-.010	-.001	-.092*	-.007	-.010
Education	.057*	.244*	-.013	.157*	.015*	.387*	-.029	.035
Race	.097	.687	.464*	.138	.192*	.687	.911*	.123
Husband's earnings	.035	.225*	-.014	.142*	.005	.297*	-.045	.260*
Youngest child 0-2	-.367*	.028	-.089	-.464	.031	-.556	-.133	-.244
Youngest child 3-5	-.284	.362	.121	-.170	-.002	-.483	-.229	-.446
Youngest child 6-12	-.211	-.563	.012	-.432*	.001	-.254	-.225	-.260
R <sup>2</sup>	.07	.16	.02	.10	.02	.02	.01	.01
Wife's current employment	.529*	1.557*	.041	.038	.086*	1.328*	.208	.434*
R <sup>2</sup> increment	.04	.03	.00	.00	.01	.02	.01	.01
Wife's work attachment	.059	.187*	-.001	.019	.010*	.213*	.005	.047*
R <sup>2</sup> increment	.03	.03	.00	.00	.01	.03	.00	.01
Wife's earnings	.180*	.416*	.008	.000	.041*	.864*	-.022	.262*
R <sup>2</sup> increment	.02	.01	.00	.00	.01	.04	.00	.01
N	679	669	682	682	664	678	682	682
Mean of dependent variables	2.84	19.60	1.26	4.04	.63	20.49	1.36	3.98
Range of scale	1-4	8-32	0-4	0-6	0-1	8-32	0-4	1-6

\*p &lt; .05.

\*\*Equations include either wife's current employment, wife's employment history, or wife's income. Coefficients for Age through Youngest child are taken from equations which include current employment. However, neither significance of coefficients nor signs of significant coefficients differ when employment history or income are substituted.



Effects of demographic variables on sex-role attitudes are relatively consistent for husbands and wives. Husbands' attitudes toward wives' employment and both spouses' attitudes toward married women's employment are more favorable among the young, the highly educated, and households in which husband's earnings are higher. Only race predicts a newly developing norm regarding women's employment as a responsibility. Blacks favor it more, probably because black women have borne, historically, more responsibility for family support. ERA and abortion availability are supported more by husbands who are more highly educated or who have higher earnings, and ERA by black husbands.

#### ANALYSIS BY CURRENT WORK STATUS

Since the high intercorrelations among current employment status, work attachment, and wife's earnings (ranging from .44 to .59) make it hard to compare their effects, we reestimate these equations separately for currently employed and nonemployed wives, using work attachment and earnings potential as a predictor for both groups. For the currently nonemployed, work attachment and earnings affect wives' EMPATT, while past earnings affect husbands' perceptions of household labor (see Table 5). For the currently employed, effects resemble those in the previous analyses with two interesting exceptions. Husbands' perceptions of decisionmaking are affected by wives' earnings but not their work attachment, supporting a power interpretation of earnings effects. In contrast, EMPATT for husbands is affected only by work attachment, implying a gradual, cumulative attitude shift. For wives, both perceptions of housework and abortion attitudes are influenced by earnings but not by work attachment. Thus from wives' viewpoint, earnings may allow them to demand household help in a way that work attachment does not. On the other hand, this may simply imply that wives with high earnings decrease their own housework time due to job demands, with a consequent increase in husbands' *relative* time. However, the effect of nonemployed wives' *past* earnings on husbands' perceptions of housework would tend to support the notion that earnings potential increases wives' power to make demands, even during periods of nonemployment.

#### Discussion

Women's massive entry to market work has had major social consequences. This research tries to determine household-level counterparts in attitudes and behavior by examining whether differences related to wives' LFP tend to be specific or diffuse, immediate and pragmatic or longer-lasting and cumulative, or whether they tend to occur mainly in families in which

**Table 5. UNSTANDARDIZED COEFFICIENTS, ATTITUDES AND PERCEPTIONS OF HUSBANDS AND WIVES: CONTROLLING FOR WIVES' CURRENT EMPLOYMENT\*\***

Independent Variables	Decide	Housework	Wife Work	Empatt	Should Work	ERA	Abortion
<u>Husbands' Attitudes</u>							
Wife's work attachment	-.031	.031	.008	.096	-.018	.005	.044
Wife's earnings (wife not employed)	-.205	.532*	.016	.267	-.061	.043	-.258
N	304	304	302	295	304	295	304
Wife's work attachment	-.033	.133*	.016	.145*	-.004	.005	-.004
Wife's earnings (wife employed)	-.203*	.380*	.091	.242	.013	.029	.067
N	377	378	377	374	378	369	378
<u>Wives' Attitudes</u>							
Wife's work attachment	.037	.050		.248*	-.030	.005	.039
Wife's earnings (wife not employed)	-.246	.110		.727*	-.157	-.028	.116
N	304	304	301	304	304	294	304
Wife's work attachment	-.018	.064		.183*	.007	-.002	.047
Wife's earnings (wife employed)	.002	.432*		.831*	-.019	-.002	.290*
N	377	377	377	378	378	372	378

\*p &lt; .05.

\*\*Control variables and method of analysis same as in Tables 3 and 4.

wives' earnings are higher. Our main hypothesis was that wives' work attachment over time, because it reflects part-time and discontinuous aspects of women's LFP, would have more effect than their current employment status on household behaviors and sex-role attitudes. We also expected wives' earnings to affect behaviors and attitudes more than would their current employment status on the grounds that level of earnings should affect power and prestige in the household as it does in the larger society.

As expected, we found that a measure of wives' work attachment over a recent 10-year period captured effects of stable predictors of women's LFP such as race, education, and husband's income as well as effects of unmeasured variables such as wage potential and areal factors.

Unexpectedly, we found that wives' current employment status, work attachment, and earnings similarly affected husbands' perceptions of decisionmaking. Yet these variables had no effect on wives' perceptions. Controlling for wives' current employment status, we found that only wives' earnings affected these perceptions for husbands of employed wives, supporting the notion that earnings, not just employment per se, positively affected husbands' perceptions of wives' decisionmaking power.

Although work attachment, current status, and earnings all affected both spouses' perceptions of the division of household labor, the effect of wives' current employment status was, unexpectedly, greater than the effects of work attachment and earnings. Although these effects are hard to separate, it appears that past work attachment does not induce more help from husbands of currently nonemployed wives. But for employed wives, the longer their work attachment and the higher their earnings, the less housework they do relative to that done by their husbands. Yet translated into actual tasks, these effects are minimal.

Unexpectedly, work attachment, current status, and earnings differed only slightly in predicting sex-role attitudes although, as expected, both spouses' attitudes were more strongly affected the more they specifically pertained to women's employment, consistent with the attitude-behavior literature (Fishbein; Schuman). More diffuse effects were limited.

Finally, our expectation that husbands' attitudes would be affected less than wives' attitudes by wives' employment was not supported. With a few exceptions, effects were similar for husbands and wives.

These findings imply that attitude and behavior change tend to occur on pragmatic rather than on ideological grounds. As Converse observed, nicely developed ideologies occur mainly among intellectuals and academics. Wives do market work because the family needs money; their husbands do a little housework to enable the household to continue to function. Approval of married women's employment increases because, otherwise, the wife's daily market work would make both spouses uncomfortably aware of inconsistency in belief and behavior. And both spouses

come to approve married mothers' employment—the idea that such work harms children declines. The husband's attitude toward his own wife's employment is more favorable.

But family comfort does not require that attitudes change on issues less closely related to women's market work; it is easier for spouses not to see them as related. As other studies have noted (Mason and Bumpass; Welch), sex-role attitudes are multidimensional. Women's LFP affects attitudes about employed women but not about the ERA or abortion. Furthermore, it is not pragmatically necessary that decisionmaking become more equal and, indeed, wives do not perceive that it does become so. Presumably, for sex-role attitudes not directly related to women's employment to change, or for the household division of labor or decisionmaking to become more equal, other changes must occur in people's lives, changes which force them to confront these issues. For example, a woman who lives alone after a divorce, who is forced to make all decisions for a time, might prefer to continue doing so upon remarriage.

In sum, we expected wives' work attachment and earnings to affect household behaviors and attitudes more than would their current employment status because strong work attachment indicates commitment to steady employment outside the home and earnings indicate the level of reward. But we were wrong. That current employment generally has more effect implies that married couples are remarkably resistant to changing household norms and behaviors. This implies, in turn, that women's social equality remains a distant goal. Perhaps this should not be surprising. For husbands, the temptation to preserve current household norms must be overwhelming. As Gilman observed nearly 80 years ago, the present system gives each husband a whole live private cook to himself. Why would any sensible man want to change? An important question for future investigation, therefore, is whether the current division of household responsibility is somehow fixed for all time and, if it is not, what kinds of economic and demographic conditions might induce change?

## Notes

1. Farkas argues that the ratio between husband's and wife's earnings is a more important determinant of the balance of power but he finds no effect of that ratio on the division of household labor.
2. One other nonrecursive test (Smith-Lovin and Tickamyer) found a near-significant path in the opposite direction but their measures make this path hard to interpret. Employment behavior was measured for years since the woman married; sex-role attitudes, at age 30. Hence the authors assumed that attitudes retroactively affected behavior.
3. This analysis does not use the husband-wife data to maximum advantage. A later paper will do so.
4. While it might be argued that only current earnings should be measured, that would limit the analysis to the currently employed and would therefore make comparisons less clear. Further, it can be argued that the ability to earn a high wage or salary yields power to a spouse, even if it is not currently being exercised. Readers should know that the results re-

ported here are not adjusted for inflation; however, an analysis measuring change in income levels by using the index of Consumer Buying Power yielded almost identical results

5. A number of researchers have documented the moderate to high levels of inconsistency found between husband and wife responses to questions about household functioning (Berk and Shih; Booth and Welch; Douglas and Wind). While these responses are similar in the aggregate, it is not yet clear whether the inconsistencies are systematic or random, whether data must be gathered from both spouses for an accurate picture of household behavior and, if so, how the dual responses should be handled analytically (Burr et al.) Rather than combining responses in some way which might obscure systematic reporting inconsistencies, we analyze husband's and wife's responses separately and describe them as each spouse's perception of household functioning

6. All had a minimum .40 loading on a principal components factor analysis

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# The Sexual Division of Labor: Evidence from Childhood\*

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## ABSTRACT

*This research reports the childhood antecedents of the sexual division of labor, analyzing the family chores and paid employment of 669 boys and girls aged 2–17. Data from a statewide random sample of parents show that sex typing begins very early in work roles and that, by the time children reach adolescence, sharp differences exist between "boys' work" and "girls' work." Multiple classification analysis indicates that family background characteristics and family structure have relatively little impact compared to sex and age of child as determinants of sex typing.*

Both at home and in the marketplace men and women do different kinds of work: they have different working conditions, rewards, hours, and tasks. While some of these may be ascribed to external factors, others may be attributed to learned differences in the attitudes and aspirations of men and women—the fact that a significant proportion of women would rather do the laundry than mow the lawn, rather be a teacher than an engineer, rather type than do construction (Marini).

The processes through which these differences are learned have been the subject of a burgeoning literature on sex-role socialization. While much of this literature indirectly bears on the maintenance of a sexual division of labor, there has been little research on direct childhood socialization into sex-typed work roles. Yet children *do* work, both at home and in paid employment outside the home. What are the socialization implications of these childhood experiences? We seek to provide leads to the mechanisms through which the sexual division of labor is passed from one generation to the next by examining the differentials in work experience of boys and girls.

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## BACKGROUND LITERATURE

A careful review of the literature reveals few clues to the existence, determinants, or consequences of differentials in children's work behavior. An exception is the recent monograph by Goldstein and Oldham, who use intensive interviews with 900 schoolchildren to provide documentation of sex differences in children's work-related attitudes, aspirations, and experiences. Our research seeks explanations for the development of these sex differences and, in particular, the effects of the family as a socializing agent.

A large literature shows that parental childrearing goals, including training for independence, responsibility, achievement, obedience, cleanliness, etc., vary by such factors as education, occupation, religion, sex of parent, and sex-role orientation of parent (cf. Kohn; Wright and Wright). Moreover, these parental childrearing values vary importantly by sex of child (Duncan and Duncan). These studies suggest that parental background and values should be important determinants of differentials in children's work experiences. These expectations are buttressed by research showing these same background factors to be related to adults' own sex-role values and behaviors (Scanzoni). From this literature, we draw the hypotheses that, other things equal, parents who are better educated, are of higher social class and have liberal sex-role values, will differentiate less between the work of their sons and daughters. The latter hypothesis assumes that parents will translate their professed values into childrearing practices, a consistency between value and action that is worth critical examination.

In the case of these hypotheses, "other things equal" refers particularly to structural characteristics of the family that may have an influence on childrearing practices independent of parents' backgrounds and values or which may interact with parental values. These factors (mother's employment status, number and sex of siblings, age of child, and place of residence) may place structural constraints on the work experience of children, affecting both the amount and kinds of work available.

## STUDY DESIGN

The first objective of this study is to document the existence of sex differentials in children's work. Second, we want to examine factors associated with variation by sex in children's work experiences. The hypotheses drawn from the literature reviewed above will be used to guide these explorations.

Data for this study are based on a statewide random sample of 669 children living in Nebraska in the Spring of 1979. The sample represents children of all ages from 2 to 17, both sexes, farm kids and urban kids, children from all social classes and settings. They are almost entirely white

(98%) and, compared to the nation as a whole, disproportionately rural. They are, nevertheless, much like a sample of the nation: 54 percent have working mothers, 10 percent live with a single parent, 62 percent are urban and over one-third live in an SMSA. The interviews were conducted by telephone and all responses are based on parental report.

## Findings

### CHILDREN'S INVOLVEMENT WITH WORK: THE EFFECTS OF AGE AND SEX

The data from this sample indicate that children are extensively involved with work and that this involvement increases with age (Table 1). Overall, 84 percent of the children in this sample regularly do chores around the house and 40 percent also have some sort of paid employment outside the family. (These categories are largely overlapping: 95 percent of the children with paid employment also have regular chores.) The median number of hours worked around the house each week is not large (3.3 hours per week), but represents a considerable block of time when added to school-work, extracurricular activities, and outside employment.

Overall, there are more parallels than differences in the *extent* of work experiences of boys and girls. The data in Table 1 indicate that both boys and girls are introduced to work at an early age. For both sexes there is an orderly progression toward more responsibility at home and more opportunity outside of the home as they grow older. Two differences in the experiences of boys and girls are worth noting, however. First, for both kinds of work, parents report that boys begin working earlier than girls and that, overall, slightly more boys than girls are perceived of as working.

**Table 1.** PERCENT OF CHILDREN REGULARLY REQUIRED TO DO CHORES, MEDIAN HOURS SPENT ON CHORES, AND PERCENT WITH OUTSIDE EMPLOYMENT, BY AGE AND SEX

Age Group	Household Chores				External Employment			
	Percent		Of Those With Chores, Median Hours/Week		Percent		Number	
	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls
Total	86	81	2.8	3.9	42	37	350	319
2-5	66	55	2.3	2.1	2	0	87	87
6-9	86	89	2.3	3.8	23	4*	70	74
10-13	99	90*	3.9	4.2	56	53	80	62
14-17	95	92	3.6	5.7	74	84	113	96

\*Difference between boys and girls significant at  $p < .05$ , two-sample test for difference of proportions.

Second, as they grow older, girls are perceived to put in substantially more hours of household work than boys.

#### CONTENT OF CHILDREN'S CHORES: AGE AND SEX DIFFERENCES

In evaluating the work experiences gained within the family, we are interested as much in what children do as in whether they work. In asking parents to describe the work of their children, we used a simple open-ended question to assess chores regularly required of the child, followed by a probe. This question did not elicit a complete list of children's chores, but rather their more salient ones. For example, three-quarters of the parents of children under 6 said that one of their child's chores was to pick up after him/herself; only 1 percent of the parents of teenagers listed this as a chore. Most probably these older children are still expected to pick up after themselves, but parents are likely to begin taking this for granted and to focus on more responsible chores such as mowing the lawn or cleaning. The answers to this open-ended question were coded into 18 specific chores which were then combined in the 8 general categories of chores reported in Table 2. Because these data represent salient chores and not an exhaustive list, the number of different chores each child does cannot be calculated. Rather, Table 2 presents, by age and sex, the percent of children for whom each general category of household work was mentioned.

Interestingly, these data for Nebraska children show a sex differentiation very like that which Goldstein and Oldham report from interviewing children in New Jersey. The strong similarity in sex patterns of household chores over two quite different states, six years apart (1973 to 1979), and most importantly, over two different perspectives (children's as opposed to parents' reports) gives strong support to the validity of these sex patterns.

A unique feature of the data in Table 2 is the simultaneous control for age and sex which demonstrates that there are important interaction patterns between age and sex. While some chores appear to begin and end as sex-linked work, there are others which are largely undifferentiated at younger ages, but strongly sex-typed for older children. This is most obvious in the categories of kitchen work and taking out the garbage. Both begin with similar levels of involvement for boys and girls, but kitchen work becomes predominantly female while the garbage becomes an exclusively male province. In general, the chores which show the greatest sex segregation show a pattern of decreased involvement of the atypical sex during late adolescence: girls' already low involvement in outdoor work drops further as does boys' involvement in kitchen and housework. By the time children reach ages 14-17, both processes have converged to produce fairly large differences between boys and girls in the content of their work.

Because most of the chores to be done around the average household are traditionally women's work, this sex-typed division of labor leads

**Table 2.** PERCENT OF CHILDREN WITH CHORES DOING EACH TYPE OF CHORE, BY AGE AND SEX

Type of Chore and Sex	Age				Total
	2-5	6-9	10-13	14-17	
<u>Outdoor Work</u>					
Boys	14	38	75	80	58
Girls	0	21	49	36	28
<u>Garbage</u>					
Boys	14	60	62	44	46
Girls	8	24	25	6	15
<u>Taking Care of Own Things</u>					
Boys	88	72	62	39	61
Girls	92	82	58	55	70
<u>Take Care of Pets</u>					
Boys	2	22	18	12	14
Girls	4	21	14	10	13
<u>Babysit Sibs</u>					
Boys	4	3	2	1	2
Girls	0	8	0	7	4
<u>Farm Work</u>					
Boys	5	8	11	20	13
Girls	0	8	18	15	11
<u>Housework</u>					
Boys	26	35	40	28	32
Girls	42	42	67	79	60
<u>Kitchen</u>					
Boys	24	33	34	22	28
Girls	35	61	71	72	62
<u>Number</u>					
Boys	(57)	(60)	(79)	(107)	(303)
Girls	(48)	(66)	(56)	(88)	(258)

to the fact noted earlier: older girls put in more hours on their chores than do boys. This parallels findings from surveys on adults which show that, when adults have equivalent outside obligations, women put in more time around the house than do men (Hedges and Barnett). This conclusion, though provocative, should be tempered by the fact that this survey was undertaken in early Spring (when boys' typical chores of mowing lawns and shoveling walks are both at an ebb) and it is possible, though we feel unlikely, that boys' work is underreported. Further research is required to document the finding that girls put in more hours of household labor than do boys.

## SEX DIFFERENCES IN JOB OPPORTUNITIES

In asking about the outside employment of children, the same open-ended approach was used. In this case, replies are probably more exhaustive as outside earning opportunities are more limited. Table 3 presents distribution of boys and girls by general job categories. Each child may appear in more than one classification so the numbers add to more than the total number of children with jobs.

The data in Table 3 show that job opportunities outside the home are even more sex segregated than jobs at home: the largest number of girls babysit, while boys mow lawns and shovel walks. While both boys' and girls' jobs are concentrated in one classification, boys show somewhat greater diversity of opportunity than girls and are more apt to have multiple jobs. It should be noted that while boys earn money outside of the household doing the same kinds of things they have learned to do at home, this same continuity is less obvious for girls: few earn money doing housework or related jobs.

## DETERMINANTS OF TASK ASSIGNMENT

Overall, these data on the content of children's work point dramatically to a very early and strong segregation in work tasks. While the sex typing of tasks outside of the home may be laid to employer preference, the strong sex segregation at home is the result of family decisionmaking. In the

**Table 3.** NUMBER OF CHILDREN DOING PAID WORK OUTSIDE THE FAMILY BY SEX AND TYPE OF WORK

Type of Work	Boys	Girls
Lawns and walks	86	10
Babysitting	15	74
Farm work	33	12
Manual labor	30	7
Restaurant work	12	24
(Very) odd jobs	24	9
Paper route	22	6
Office work	5	13
Total number working	147	118
Total number of jobs	227	155
Average number of jobs held	1.54	1.31
Median income 1978	\$240	\$212

following section, we explore the independent effects of age of child, structural characteristics of the family, and parent values and background on the extent to which parents differentiate between the work of their sons and daughters. While we will be comparing the sons of one household with the daughters of another, the random processes used to select the households and children under study allow us to make inferences about children not selected.

*Work Segregation Index*

In order to summarize the extent to which a child's total work load is concentrated in either boys' or girls' work, we have created a Work Segregation Index (WSI). Rather than relying on either conventional or expert definitions of what is boys' or girls' work, we have used our respondents as judges and let their actual practice define sex-linked work. The 18 different tasks done by these children were assigned a value from 1 to 5 on the basis of the sex distribution of the children who were reported to do them. A value of 1 was assigned if twice as many girls as boys did the chore; 2 if 50–100 percent more girls did the task; 3 if neither boys nor girls exceed the other by 50 percent; 4 if boys exceeded girls by 50–100 percent; and 5 if twice as many boys as girls performed the chore. According to this scheme, tasks were weighted as follows:

- |                       |   |
|-----------------------|---|
| 1. Girls work         | Bathrooms, laundry, dishes, cooking,<br>general housework, general kitchen work |
| 2. Mostly girls' work | Clear/set table, babysit, farm animal care                                      |
| 3. Undifferentiated   | Clean own room, pick up after self, pet<br>care                                 |
| 4. Mostly boys' work  | Garden, general yard work   |
| 5. Boys' work         | Lawns, snow shoveling, garbage, farm<br>work                                    |

A mean score was then computed for the entire set of tasks performed by each child. These mean scores on the WSI range from a maximum of 5.0 (all boys' work) to a minimum of 1.0 (all girls' work). A score of 3.0 indicated no sex typing in task assignment: either the child performed all undifferentiated tasks or a balance of boys' and girls' work.

*Multiple Classification Analysis*

In the following section, we use MCA to examine the independent effects of child's age, family structure, and parental background and values on task assignment. The focus of the analysis is factors associated with differential task assignments to boys and girls. While we are concerned with the degree of masculinity or femininity of task assignment (columns 1 and 2 of Table 4), the real concern is with the difference between boys and girls (column 3). For example, while children with working mothers are likely to

have more housework to do and hence more feminine scores on the average, the question is whether working mothers make more or less distinction between the chores of their sons and daughters.

### *Age of Child*

The first panel in Table 4 shows the independent effect of age on task assignment. This analysis shows that the sex differentials in task content observed in Table 2 exist independently of family structure and parental background. After controlling for all of the other variables in this table, age still has a powerful effect on sex-typing: the older children get, the more stereotypical become their domestic work assignments and hence the greater the differential in their work content.

### *Family Structure*

Four measures of family structure were hypothesized to have an influence on task assignment independent of parental background: mother's employment, place of residence, presence of opposite sex siblings, and number of siblings. The first two factors influence the amount and kind of work available and the last two provide an indicator of parents' flexibility in using sex as a criterion.

The results in Table 4 indicate that mother's employment has a significant feminizing impact on the work of both her sons and her daughters, but does not reduce the amount of discrepancy between the work of boys and girls. While women's employment may broaden the experiences of their sons, it sinks their daughters even deeper into the domestic role. Overall, the structural impact of having more housework to distribute is greater than any ideological impact on breaking traditional roles.

The differentials in Table 4 by place of residence are unexpected. While urban families are less traditional than rural nonfarm families, it appears that those living on farms are least likely to distinguish between the work of boys and girls. (Of the two chores specific to the farm, one was coded as mostly girls' work and the other as boys' work.) We speculate that any rural traditionalism which may exist is overborne by the practical necessity to assign work on the basis of age and ability. In the face of a substantial work load (farm kids work an average of two hours more per week), sex may be a luxury criterion in work assignment.

The last two measures of family structure show only weak effects. Interestingly, the larger the number of siblings, the more feminine are the task assignments of both boys and girls—resulting in no change in the amount of distinction parents make between boys and girls. In regard to presence of opposite sex siblings, the expected finding (that parents are more likely to make sex-typed work assignments when they have at least one of each) is present, but very weak.

Table 4. MULTIPLE CLASSIFICATION ANALYSIS OF WORK SEGREGATION INDEX BY AGE OF CHILD, FAMILY STRUCTURE, AND PARENTAL BACKGROUND

	Adjusted Mean Work Segregation**			Number	
	Boys	Girls	Difference	Boys	Girls
Grand Mean	3.36	2.27	1.09	303	258
Child's Age					
2-5	2.79	2.48	.31	38	34
6-9	3.10	2.29	.81	75	76
10-13	3.53	2.27	1.26	98	70
14-17	3.69	2.14	1.55	75	68
B <sup>2</sup>	.35*	.13*			
Family Structure					
Mother's Employment					
Not employed	3.50	2.39	1.11	131	118
Part-time	3.28	2.25	1.03	69	58
Full-time	3.21	2.09	1.12	86	72
B <sup>2</sup>	.15*	.16*			
Residence					
Farm	3.09	2.21	.88	47	37
Rural nonfarm	3.50	2.12	1.38	63	47
Urban	3.38	2.33	1.05	176	164
B <sup>2</sup>	.15	.10			
Opposite Sex Siblings					
No	3.30	2.26	1.04	160	134
Yes	3.43	2.28	1.15	126	114
B <sup>2</sup>	.07	.01			
Number of Siblings					
None	3.49	2.38	1.11	88	72
One	3.32	2.27	1.05	106	92
Two or more	3.29	2.18	1.11	92	84



<u>Parental Background</u>				
Parent's Education				
Less than 12 years			2.19	32
12 years	3.37		2.21	139
13 or more years	3.52			114
B <sup>2</sup>	3.16		2.35	115
	.19*		.09	
Subjective Family Income				
Below average			2.38	53
Average	3.20		2.25	165
Above average	3.41		2.23	68
B <sup>2</sup>	3.37		.07	
	.09			
Parent's Sex				
Male			2.34	95
Female	3.40		2.23	191
B <sup>2</sup>	3.34		.06	
	.03			
Parent's Sex-Role Ideology				
Traditional			2.19	197
Nontraditional	3.40		2.43	89
B <sup>2</sup>	3.27		.14*	
	.07			
R <sup>2</sup>			.111	
	.214			

\*Significant at the .05 level.

\*\*Mean values are adjusted for each other variable in the table. When used as a control and in the regression analysis education, number of siblings, and age of child were measured at the interval rather than categorical level.

*Parent's Background and Values*

Four variables are included here to gauge the influence of parent's own values and background on sex differentials in children's work assignments: responding parent's sex, education, sex-role values, and subjective family income. Two of these variables show significant effects on task assignment and important variability in the distinction drawn between the work of boys and girls: parents with high education and with more liberal sex-role values make substantially less distinction between the work of their sons and daughters. Examination of the mean WSI scores for boys and girls on these two variables indicates that the relative lack of differentiation among the well-educated and the more liberal is achieved by moving both their sons and daughters toward a less sex-typed division of labor as opposed to merely feminizing the task assignments of their sons.

Neither the measure of subjective family income or responding parent's sex has a significant effect on task assignment. The measure of income behaves opposite to education, indicating that the lower the income, the less the distinction between boys and girls. This suggests that it may be the liberalizing effect of education which is active above, not the correlation of education with socioeconomic status. Sex of respondent is included in this table primarily as a control variable. Although earlier analysis (not shown here) indicates that mothers report more tasks than fathers for both boys and girls, the results in this table show that mothers and fathers report very similar differentiation between boys and girls.

Finally, the last line in Table 4 is the explained variance. The nine variables included in this table explain 21 percent of the variance in boys' work and only 11 percent for girls. The low explained variance is, of course, due to the control for the major determinant of task assignment: sex itself. After removing the variance due to sex of child, age is the strongest predictor of sex typing. The evidence presented in this table strongly suggests that sex typing in children's work is an ubiquitous feature of family life, being determined largely by children's age and sex. While there are some differentials in degree of sex typing, notably by parent's education and sex-role values, age and sex are the primary determinants of sex-typing in all categories of families.

**Summary**

Using data from a statewide random sample of 669 children, this study demonstrates that boys and girls are extensively, and nearly equally, involved in work, both inside and outside of the home. The similarity ends abruptly, however, when we look at what they do, for here there are considerable differences in the work experiences of boys and girls. They live in an occupational world, both at home and away, that is every bit

as sex segregated as their parents' world of work—perhaps more. While analysis of background and structural factors indicates some variation in degree of sex typing, notably by parents' education and sex-role ideology, these types of variables explain little of the variance in task assignment. The consistent direct effect of child's age on sex typing, independent of family structure or background, suggests that the sex-linked assignment of children's chores is a powerful societal norm which becomes more intense as the child matures.

These data indicate that the major changes in women's adult roles within the last two decades have yet to reach childhood. While it is possible that sex-typing in children's work is less now than it was twenty years ago, evidence from this 1979 survey indicates that sex typing remains strong in children's work socialization.

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# **Belief Constraint and Belief Consensus: Toward an Analysis of Social Movement Ideologies—A Research Note\***

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The purpose of this research note is to point toward procedures for the comparative analysis of social movement ideologies. The need for such procedures follows from our conviction that, to describe the outcomes of recent social movements, we need to understand their ideologies and the differential impact of those ideologies on subgroups within the general public. We have found, however, that adequate procedures for comparative study of social movement ideologies do not exist in the resource mobilization paradigm currently favored for the study of social movements. The techniques we use to characterize social movement ideology are borrowed from the study of political belief systems in political science (Converse). In adopting these procedures, we identify a methodological flaw in them that is particularly troublesome when studying groups that exhibit a great deal of consensus in their beliefs (e.g., social movements).

Converse and his colleagues studied belief system differences between political elites and the public at large. They focused on differences in the consistency or constraint of political beliefs, where constraint is defined as the extent to which various beliefs can be predicted from each other. Typically constraint has been assessed by intercorrelating attitudes on different political issues and then averaging the resulting correlations across issues. Elites have generally been found to show much greater constraint than the public at large, although this conclusion and the procedures employed to reach it have been criticized. The greater belief constraint of political elites is assumed to be due to their high level of political involvement. Political activity and opposition make political issues salient and results in consistency across issues.

The factors that lead to high levels of constraint in political elites are also found in social movements. Social movements typically demand high

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levels of participation and involvement from their members. In addition, that participation occurs in a position of challenge or opposition to broader segments of the society. Indeed, we might expect that members of social movements would exhibit even greater belief constraint on beliefs relevant to the movement than members of a political elite.

Another characteristic of beliefs among social movement members prohibits our use of the traditional measurement of constraint. Belief consensus has long been regarded as one of the defining characteristics of social movements, although its centrality in motivating action has been disputed. We therefore would expect very little variation in beliefs relevant to the movement among movement members. It is well known that the magnitude of correlations is affected by the variance of the correlated variables. Hence, when groups differ in consensus, comparisons of the traditional constraint measure between groups may lead to inaccurate conclusions. In other words, belief constraint and belief consensus are confounded in the traditional correlational approach to constraint.

Barton and Parsons have suggested procedures for examining belief constraint and consensus independently. If constraint is defined as the consistency of an individual's beliefs across a variety of belief questions, and if those questions are all in the same ideological direction (e.g., more liberal responses are always represented by higher numbers), then the responses of an individual with a highly constrained belief system should be less variable across questions than the responses of a less constrained individual. If consensus is defined as the consistency of a group of individuals in their responses to a belief question, then groups with higher consensus should show less variability in responses to a given question than groups with lower consensus. In other words, a difference in belief *constraint* between two groups is indicated by a difference in response variance across questions within individuals, averaged across all group members. A belief *consensus* difference is indicated by a difference in response variance across individuals within questions, averaged across all questions.

To assess whether social movement members exhibit greater belief constraint and consensus on movement relevant beliefs than members of a political elite, mail surveys were conducted among members of the Boston Chapter of the National Organization for Women and among members of the Massachusetts Federation of Republican Women. In the analysis of the resulting data, we first demonstrate the pitfalls of using the traditional procedure for assessing constraint in a social movement characterized by high consensus. We then examine constraint and consensus using the more appropriate measures.

## Method

### SAMPLES

Data from social movement members were gathered by a mail survey to the members of the Boston Chapter of the National Organization for Women (NOW). One hundred eighty-eight questionnaires were returned with complete data (44%). For the comparison group, we wanted a group of politically active women who were unlikely to be members of the social movement. One hundred thirty-three (35%) completed questionnaires were returned from a mail survey of the Massachusetts Federation of Republican Women (MFRW). Although these response rates are characteristic of mail surveys, they make generalization to the organizations problematic.

### QUESTIONNAIRE

The data used consist of responses to 27 belief statements concerning women's role in society with which all respondents expressed agreement/disagreement on a seven point self-rating scale. The statements were selected from scales constructed by Smith et al., and by Spence et al.

### METHODS OF ANALYSIS

Prior to analysis, questions were reversed where necessary so that they were all ideologically in the same direction (i.e., lower responses being more favorable to women's rights). Belief constraint was then compared between the groups using the traditional procedure of averaging pairwise correlations. Following this, we compared constraint and consensus between groups using the more adequate variance measures. In this analysis, demographic characteristics of the respondents are controlled, since the groups differed in mean level of education, marital status, and employment history.

## Results

Following the traditional technique for assessing constraint, the average intercorrelation between pairs of questions for the NOW sample is .129 ( $t [186] = 1.77; p < .05$ , one-tailed). For the MFRW sample, the average intercorrelation between pairs of questions is .208 ( $t [131] = 2.42; p < .01$ ). According to this traditional assessment of belief constraint, the social movement members have a less constrained belief system on the issue of women's rights than respondents who are not members of the social movement. The pairwise correlations in the two groups were also factor ana-

lyzed. This analysis was consistent with the average correlations in that it revealed a simpler factor structure among the MFRW members than among the now members.

We have argued that lower correlations in the social movement group may be produced by quite high consensus in that group. To examine consensus and compare it between groups, the variance of responses to individual questions across respondents was calculated separately for the two groups. For now members this variance averages 3.49 across all 21 questions. For MFRW members, it averages 7.83. Using a *t* test for matched samples, these variances differ significantly,  $t(26) = 7.47$ ;  $p < .001$ . Thus it is clear that there is more belief consensus among respondents from the social movement than among respondents from the political party organization.

The constraint hypothesis was then reexamined more adequately and independently of consensus differences by comparing between groups the average within individual variances across questions. For the now respondents this average within individual variance is 4.26. For MFRW respondents, it averages 7.92. These two mean values differ significantly from each other,  $t(319) = 11.14$ ;  $p < .001$ . Thus, the typical now member shows less variability in her responses or is more consistent across questions than the typical MFRW member. If we define belief constraint as individual consistency in responses, then there is more belief constraint among members of the social movement organization. This group difference continues to be significant when respondents' education, marital status, and employment history were controlled through multiple regression (see Table 1).

As we would expect, the average beliefs of the now respondents are significantly more extremely feminist than the average beliefs of the MFRW respondents. This difference held up, also, when demographic characteristics were controlled (Table 1).

## Discussion

In this note we have begun to evaluate the utility of studying the structure of ideology among social movement members by borrowing concepts and techniques developed in the study of belief systems of political elites. We have shown that respondents from a social movement organization exhibit more constraint, more consensus, and more extreme positions, than members of a politically active comparison group, so long as procedures are used that permit independent assessments of constraint and consensus. We believe that the mechanisms responsible for producing high belief constraint in the ideology of social movement members are similar to those responsible for higher levels of constraint in political elites. That is, political

**Table 1.** RESULTS FROM REGRESSIONS OF BELIEF CONSTRAINT AND POSITION EXTREMITY ON GROUP MEMBERSHIP, EDUCATION, EMPLOYMENT STATUS AND MARITAL STATUS

Standardized Regression Coefficients from Equations			
		Equation #1	Equation #2
Dependent Variable: <u>Constraint</u> <sup>a</sup>			
Predictors:	Group <sup>b</sup>	-.530 <sup>±*</sup>	-.472 <sup>±*</sup>
	Education <sup>c</sup>		-.023
	Employment <sup>b</sup>		-.043
	Marital status <sup>b</sup>		-.094
R <sup>2</sup>		.281	.291
Dependent Variable: <u>Extremity</u> <sup>a</sup>			
Predictors:	Group	-.727 <sup>±*</sup>	-.625 <sup>±*</sup>
	Education		-.011
	Employment		-.086 <sup>±</sup>
	Marital status		-.181 <sup>±*</sup>
R <sup>2</sup>		.528	.566

<sup>a</sup>Lower predicted values indicate higher constraint; lower predicted values indicate more extremely feminist positions.

<sup>b</sup>Measured by dummy variables as follows: group membership (NOW = 1, MFRW = 0), work status (employed = 1, other = 0) and marital status (single, divorced, separated = 1, married, widowed = 0).

<sup>c</sup>Education was measured as: high school graduate, or less; some college, college graduate; graduate or professional school.

\*p < .05.

±\*p < .001.

activity and a stance of issue opposition encourage a unified and consistent belief structure on issues relevant to the social movement.

While the concepts developed in the assessment of elite belief systems are useful in examining social movement ideologies, the techniques are flawed because of the unusually high degree of consensus exhibited by social movement members. This points to a weakness in the conventional procedures typically used in the assessment of belief structures, a weakness that is fatal when used in the analysis of social movement belief structures.

The purpose of this note has been to suggest concepts for the assessment of social movement ideologies. It remains only a beginning, however. The generality of our conclusions needs to be examined with more adequate samples and in other social movements. Nevertheless, we believe that the study of social movement ideology would benefit from more precise methods which permit comparison of belief structures between groups within movements and between movements. In so doing, it is worth ex-



perimenting with the concepts and modified techniques developed by those who have examined ideology in other contexts.

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# Secret Societies and Social Structure

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## ABSTRACT

*Since Simmel's seminal essay on secret societies, little systematic work on the topic has been attempted even though the empirical basis for such work has greatly improved. The present paper uses six case studies with unusually good structural detail in analyzing one aspect of secret societies: their organizational structure. In contrast with Simmel, I argue that secret societies under risk must be differentiated from others; that societies under risk have a wide range of structural forms; and that the major sources of structure are found in preexisting social structures rather than in psychological factors. Risk enforces recruitment along lines of trust, thus through preexisting networks of relationships, which set the limits of the secret society's structure. Structure can still vary considerably, depending primarily on the centralization of control of recruitment, in turn dependent on the control of key resources.*

This paper discusses the structures of secret societies operating under risk, with an emphasis on how and why such structures vary in the extent to which they are hierarchical. To date, the only theory of secret society structure is that of Simmel. His discussion of secrets and secret societies has not generated as much later work as most of his other contributions; see, for example, a recent review of Simmel's impact on North American sociology (Levine et al. a, b). Further, the few scholars discussing secret societies since Simmel have not systematically analyzed the structure of secret societies. This topic need no longer be neglected, given the growing number of good case studies with detailed structural information.

I will use six such studies to develop an argument which differs from Simmel's in several fundamental ways. Simmel discusses secret societies in general. I argue that secret societies operating under risk are quite different from others, and my analysis is confined to such cases. Simmel's discussion of the sources of secret society structure emphasized psychological factors, especially a love of hierarchical planning. I argue that, for societies under risk, the crucial motive is a desire to maximize security. Simmel implies that psychological factors are far more important than structural ones, while I argue that preexisting social structure constrains the forms secret societies can take. Finally, Simmel assumes that all secret societies are hierarchical. I argue that their structures vary, in part because

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of their varying structural contexts. Indeed, this paper can be seen as a contribution to the study of the emergence of new social forms from old ones.

### Defining and Classifying Secret Societies

A secret society is here defined in social network terms as a persisting pattern of relationships which directly or indirectly links the participants in related secret activities. There must be some secret activities in order to have a *secret* society; there must be a persistent pattern of relationships among participants in order to have a secret *society*. Consider one example of secret activity that does not meet this definition of a secret society. Lee describes getting an abortion at a time when that was a criminal offense. Most of the abortions were obtained by women who asked for help and information from a few trusted friends, who sometimes asked friends of theirs in turn, and so on until the connection was made. These activities were not part of an enduring organization of relationships carrying out similar activities over time, but rather they were one-time collaborations of a few people who may never again have been part of the same action set.

Thus the proposed definition requires secret societies to have persisting structures, but it does not restrict the form such structures may have; their form should be a matter for investigation. In this paper varying forms are roughly classified by their degree of approximation to a hierarchy. This classification suffices for the present argument and allows us to assess the adequacy of Simmel's stress on hierarchy. The most hierarchical structure possible is a tree in the graph-theoretic sense, with careful separation of both levels and branches in the organization, a form Simmel (357) suggests in a key example. In a less rigid form there are distinct ranked orders (Simmel; 356–7), with access only between contiguous levels (366–7) and control centralized at the top level (371). Branches, however, are less well defined; people at the same level may have rather free access to each other. In still more loosely structured cases, not considered by Simmel, both levels and branches are difficult to define and the structure is more like an acquaintance structure than a hierarchy in the more or less rigid sense.

Secret societies may also be classified in terms of the conditions under which they exist. If conditions include risk, as when the members of a secret society risk imprisonment or injury or death, the processes generating the society's structure are distinctive ones. The analysis of cases under risk, which is the business of this paper, need not apply to secret but safe societies. In turn the analysis of safe secret societies would probably benefit from a close reading of Simmel<sup>1</sup>, whose ideas are not as applicable to the structure of secret societies under risk.

## **The Variability of Secret Societies Under Risk**

### **THE DATA AND THEIR LIMITS**

For the purposes of this paper, secret societies under risk must be distinguished from safe ones but they need not be further subclassified. They vary in many ways,<sup>2</sup> but I argue that these variations will affect only the details of the distinctive processes to be analyzed. Risk is so important a consideration that it sets similar processes in motion even for societies differing in time, place, goals, and so on, as do the examples used here.

I will describe the structures of six secret societies in detail, both to demonstrate the variability of secret society structure and to provide background for the analysis of the sources of such structure. The case studies used provide unusually detailed and reliable information about structure and its suggested sources. For the most part the studies are based on accounts by multiple observers from several different parts of the secret society; the observers are not randomly obtained but biases in obtaining them can be seen and allowed for; the observers' own biases in both observation and reporting can be judged; some supplementary data sources are used; and the kinds of ties reported as links in the secret society structure are clearly indicated. A more detailed discussion of these topics for each case is given in a methodological Appendix. Overall, the data and their quality suffice to demonstrate that structural variability exists and to illustrate my analysis of the sources of this variability. The data quality is also far superior to that available when Simmel was writing. Simmel does not make his own sources clear, but they were probably similar to those described in great detail in Heckethorn, who first published at the turn of the century. The bulk of Heckethorn's material consists of scanty and highly colored accounts by untrained and often biased observers, with the original accounts often filtered through several untrained and even more biased intermediaries. Roberts clearly shows the paucity and inaccuracy of nineteenth century information blaming mistaken beliefs current among the ruling classes and their agents (like police forces), especially the belief in the extent and power of secret societies.

The quality of available data has improved greatly. However, the data still do not permit a rigorous testing of theory because a random sample of cases cannot be obtained. Secret phenomena are in general impossible to sample; for similar comments on a different kind of covert activity, see Marx (404). In the present paper, there is the added difficulty that the cases used to illustrate the argument are also to a large extent those used to develop it in the first place. The paper must therefore be seen as an argument with roots in good data sources, but not as a formally demonstrated theory.

THE CASES: FROM HIERARCHY TO DIFFUSE NETWORK

The first example is one which comes closest to Simmel's planned, hierarchical secret society. This example is the underground organization in Auschwitz, described by Garlinski on the basis of a variety of memoirs, recollective interviews, and documents. The most active organizer, Pilecki, assumed the underground ought to be a rigid hierarchy with five-man cells, and tried to organize it in just that way. The widespread conception of the underground as a resistance army, as well as some other possible biases, probably leads to a somewhat overly hierarchical trend in the observer accounts. Even allowing for this, the Auschwitz example is clearly one of the most hierarchical ones, with distinct levels and branches and centralized control. Levels included Pilecki himself as effective leader, other leaders and organizers, people directly recruited by organizers, and people indirectly recruited who could often have been subdivided into still further levels (e.g., Pilecki at one point had recruited 100 people himself and they in turn perhaps 500 more; Garlinski, 110). Access to vital secrets, such as knowledge of underground memberships, was limited to the inner circle or one or two people like Pilecki himself. There were several kinds of segregated branches in the organization. Branches to some extent paralleled divisions in the official camp organization (hospital branches, medical workers vs. craftsmen, residential blocks). And some branches of the underground grew as separate undergrounds and remained distinct after the leadership merged. Centralized control was reflected in such matters as escapes, with underground members either refraining from them, or helping in them as their leaders decreed. The overall structure was thus hierarchical, though not the rigid hierarchy of five-man cells Pilecki intended. Recall that he recruited 100 people personally; one woman could recall over 20 people she knew in the underground, "occasionally" people belonged to two different branches.

Naquin provides an admirable account of the 1813 Eight Trigrams rebellion in China; here I will emphasize the best-documented branch of the rebels, the White Lotus sect in the Peking area. Sources for this branch include some 200 confessions by participants in various parts of the organization (Naquin, 285-6), plus palace memorials, official communications, and captured sect documents. Again there may be some trend toward an overemphasis on hierarchy in these materials, in part because both the sect members and government officials had a hierarchical conception of orderly social relations in general and the sect in particular. Even allowing for this, the sect was almost as hierarchical as the Auschwitz underground. The White Lotus had levels clearly distinct in terms of authority, with a beginner deferential to his teacher and that teacher in turn deferential to his teacher and so on up to the sect leader. Only rarely were these rankings reversed, as in the case of the exceptional leader, Li, who supplanted his

one-time superiors. These levels were probably more distinct than those in the previous example in the sense of lower mobility, but on the other hand, they were less distinct in the sense of security since it was common for people to know the identities of those above and below them. Indeed, such knowledge was an aspect of the legitimacy of the sect teachings, since each teacher drew authority from his link with his own teacher. Even at a high point of the sect's size, while still underground, with thousands of members in several areas, lower ranking members knew the name and general location of their top spiritual leader, Lin. Branches were reasonably distinct, as reflected for example in the tendency to break down into separate sects (one per branch) when the succession to overall leadership was unclear. But branches in different locations nevertheless had some cross-linking: the geographically mobile might join different groups in different places, healers moved around as part of their work and had links in many communities, and boxers were mobile and also liked to study with a series of teachers to expand their repertoires. Centralized control was reflected in hierarchical control of sect finances with donations flowing upward for the leaders to use as they thought best; in the top-level planning of the eventual rebellion and the downward flow of information and orders concerning it; and in the graduated access to sect secrets, such as special chants and scriptures.

The next two cases have much less clearly hierarchical organization, with at least a few levels but possibly less distinct ones and with markedly less branch separation.

First is the Lupollo crime "family" reported by Ianni from several years of fieldwork and information from dozens of informants. The major bias problem here is that the higher levels of the organization are much better observed than the rest. Ianni directly observed relationships among the kin group from which the higher levels were recruited and recorded various kinds of ties in detail; but lower ranks are reported largely from indirect information limited to "organization charts" (a,106) which may or may not have fully reflected reality. It is clear that there are several layers of authority, with members of the ruling kin group at the top and subordinates arrayed in up to half a dozen layers in the numbers and loansharking enterprises. Subordinates know who their employers are, though not all that is done higher up. The separation of branches is more doubtful. Branches are presented as separate in organization charts but some of the concrete detail suggests otherwise. Ties across branches are very dense at the top, where ties are most closely observed, and they exist, to at least some extent, lower down as well. For example, the two major illegal enterprises are cross-linked at least through the practice of having loansharks hang out near the gambling sites. Control over the enterprises is centralized.

The second intermediate case is the San Antonio heroin market, studied by Redlinger from fieldwork and 40 interviews touching on all

levels of the distribution network except the top one and the marginal light user. Higher levels are less well represented and are reported in somewhat different terms, with more stress on economic transactions as opposed to trust and friendship ties. Thus (just the opposite of the preceding case) it is the upper levels whose hierarchical organization may be overestimated. The market has at least four levels: consumers, retailers, wholesalers, and big dealers or importers. The distinctions among the levels are fluid. Retailers are usually also consumers and return to the consumer level often; wholesalers may be retailers expanding business. There is some insulation between levels stemming from the desire of inferiors to keep their higher-level connections secret in order to profit from monopoly access to suppliers, and from the desire of superiors to restrict potentially dangerous knowledge of their illegal activities to a trusted few. But these desires are to some extent frustrated by the fact that most participants are recruited from the same community and could not operate at all without the network of contacts the community provides (Redlinger, 337–47). There is some branch separation on ethnic lines, with blacks and Anglos getting heroin primarily from other blacks or Anglos, but most market members are Mexican-American and branch separation within this ethnic group is not clearly indicated. Overall, the structural information is incomplete and biases are probably extensive; it is simply a reasonable conjecture that levels and branches exist but are less separated than in previously discussed cases. Control, however, is still fairly centralized through central control of heroin supplies.

The Lupollo crime family and the San Antonio heroin market are already rather far from pure cell structures, since branches are poorly separated and even levels are imperfectly insulated. The next sample takes us far indeed from pure hierarchy. Plant did fieldwork and interviews with 200 drug users (primarily marijuana users) in Cheltenham, England. Plant treated any kind of contact within the network as a tie, which raises some problems of comparability with studies using more restrictive definitions if the latter tend to select more hierarchical relationships. On the other hand, ties of different kinds (e.g., drug transaction links vs. friendship) seem to be little separated in this network. Plant pursued ties so thoroughly that he was able to construct a sociogram for his 200 contacts (107). As a hierarchy this sociogram is unrecognizable, but as a friendship network it is typical. There are a few well-known dealers who are connected to a relatively large number of people, but this hardly constitutes distinct layering; and Plant reports some trend to greater within-class interaction, but there are far too many cross-class links for distinct branches to be visible. Plant gives no indication of centralized control of activities in the network, though of course some members are more active than others. Important information diffuses freely and rapidly.

The last example has one especially interesting feature: the analyst

(Aubert) is both a sociologist and a former full-fledged participant, a member of the underground in his native Norway during the German occupation. This unusual immediacy is the main reason for including the case even though Aubert is somewhat vague about the basis of his generalizations and the overall direction of bias is difficult to judge (for details see Appendix). The structural pattern is also hard to identify.

Aubert refers to a hierarchical structure of command in which superiors were more likely to know the real identity of subordinates than vice versa. But he also refers to egalitarianism, decisions made largely by consensus, and friendship ties linking people in different positions. The existence of separate branches is suggested by the remark that subordinates had only limited contacts with other subordinates but this could be true for a number of different possible structures, including one of overlapping small cliques rather than distinct branches. Probably this structure fits in just before the marijuana network, more hierarchical than that, but less hierarchical than the first four examples.

Even allowing for the gaps and ambiguities in our knowledge of these cases, the variety of organizational structure is striking. Further, no simple explanation for the variation seems to hold. For example, whatever the details of the Norwegian underground structure, it was certainly far less elaborate and hierarchical than the Auschwitz underground, even though both organizations were political secret societies engaging in similar resistance work under similarly severe risk. Again, the San Antonio heroin market and the English marijuana network are both organizations distributing illegal drugs, but one is moderately hierarchical and the other not hierarchical at all.

## **Explaining the Structure of Secret Societies Under Risk**

### **SUGGESTIONS FROM SIMMEL**

Simmel sees the secret society as an exceptionally rational, deliberate, planned construction. It is built from scratch by a central power and reflects the builders' fascination with control and hierarchy. Thus the structure is always hierarchical and preexisting relationships are not important.

Evidently, the fact that the secret society must be built up from its basis by means of a conscious, reflective will, gives free reign to the peculiar passion engendered by such arbitrarily disposing, organizational activities of planning important schemata. All system-building . . . involves the assertion of power . . . especially true of the secret society, which does not grow but is built, and which can count on fewer preformed parts than can any despotic or socialist system (357).

Clearly there is some weakness in this line of thought, since we have just seen that secret society structure can vary greatly for societies



under risk. I shall argue that the presence of risk leads to different motivations in building secret societies, to a far greater part played by "preformed parts," and indeed to a quite different line of analysis from that sketched by Simmel; this analysis is consistent with variability in secret society structure. The argument takes a starting point that echoes a comment by a sophisticated analyst with more practical experience than Simmel: "Secrecy is such a necessary condition for this kind of organization that all other conditions (number and selection of members, functions etc.) must be made to conform to it" (Lenin, 73).

#### RISK, TRUST, AND PRIOR NETWORKS

When secrecy is indeed a *necessary* condition, when it stems from need to reduce risk rather than from the fun of having a secret, trust becomes a vital matter and hence preexisting networks set the limits of a secret society. To amplify this, suppose for simplicity that one member is thinking of recruiting one new member. The established member is under risk, since the potential member could betray him; the potential member is also under risk, since he is invited to participate in a dangerous activity about which he knows very little. Clearly recruitment is not likely to take place unless the two people first trust each other, unless they have a prior tie outside the secret society and that tie is a reasonably strong one. (Trust was important in all our examples; see Garlinski, 37; Naquin, 32; Ianni, a, 77-82; Redlinger, 338, 343; Plant, 76; Aubert, 291.) And trusted prior contacts are important in general, as reported over and over again: in accounts of other political undergrounds (e.g., Kogon), in accounts of other drug users (for heroin, see the review in Hunt and Chambers; for marijuana, Kandel) and so on.

Strong ties are always preferred as the building blocks of secret societies, but the kind of tie actually used varies. Relatively weak ties may be used if the degree of risk is relatively low; for example, marijuana users are willing to share information or drugs or activities with a wider range of people than the more endangered and hence more cautious users of heroin (compare Plant, or Goode, to Redlinger, or Hughes). And the nature of a strong tie varies with culture and context. To the nineteenth-century Chinese and to twentieth century migrants from Southern Italy, kinship is the strongest kind of relationship, and recruitment to secret societies often followed kin lines. In Auschwitz, on the other hand, recruitment took place between former army comrades-in-arms for the military men and between former political co-workers for the political underground. For drug use, the key relationships are close peer ties especially among young males. Component links of the secret society are selected from links in prior networks, and just which prior networks depends on the participants' understanding of which ties are most trustworthy.

Since the secret society is based on prior networks, cleavages in those networks set limits for the structure of the secret society. The cleavages do not have to be extreme in order to have an effect. For even if there are a few bridging ties crossing prior relational cleavages, these bridges are likely to be weak (Granovetter) and thus unlikely paths of recruitment to a risky enterprise. Thus groups disconnected or poorly connected outside a secret society are also separated within (or between) secret societies. I earlier described several secret societies with relatively well-separated branches; the separation rests at least in part on the cleavages in prior networks. Consider the Auschwitz underground branches formed in different parts of the camp, or different political or army groups, or different nationality groups. Similar branching was also noted for other camps. The White Lotus branches were territorial, like the social relations of the time. Cleavages may lead not just to separate branches but to separate societies; many of the Auschwitz branches were originally separate undergrounds and some undergrounds stayed separate to the end, there were many different White Lotus sects in different places, the pattern of heroin epidemics strongly suggests that cleavage lines like race are reflected in different drug networks (Hunt; Hunt and Chambers). By contrast, Plant's soft drug users were drawn from a dense network of young people living mostly with other young people in a small area so that cleavages were not marked, and neither was the drug network internally split. On the other hand, prior cliques, as well as prior cleavages, may be reflected in the secret society. This effect appears to be more erratic, since it is easier not to use an existing tie than to bridge the gap where no tie exists, but several accounts include occasional examples of entire households, cliques, or other strong components of prior networks beginning secret work.

#### PLANNING AND THE LOCUS OF CONTROL

Important as relational cleavages are, it should be clear that these set very broad limits which by no means determine the shape of the secret society. Any prior network is the potential basis of a large variety of secret structures, depending on how relationships are selected from the prior network for inclusion in the secret society. In particular, societies with different degrees of hierarchical structure are usually possible given the same pre-existing relationships. Suppose for example, that a new recruit to a cell has prior ties to people in other cells. If these ties are allowed to become part of the secret society (if the recruit learns that his friends are also members and he begins to interact with them on that basis), then a pure hierarchy cannot be maintained, since people's ties do not naturally occur in suitable hierarchical patterns. On the other hand, a hierarchy can be maintained if ties are incorporated into the secret society only when they fit an overall hierarchical pattern. I suggest that the critical question here is the locus of

control over recruitment, with more centralized control a prerequisite for highly selective incorporation of prior ties which is in turn a prerequisite for hierarchical structure.

Centralized control of recruitment can take several forms, varying in detail. Simmel (349–51, 366–7) describes one form in which new members enter at the lowest level and move upward only when their superiors feel they have been sufficiently tested and trained. The essential element here is some kind of control from above. In some cases, people at one level control just those at the next level below (the San Antonio heroin market probably fits here). In others, those at the highest level may centralize yet further by controlling recruitment and promotion for several levels below them (the Lupollo "family" is closer to this version). Recruits may have to start at the bottom and work their way up (the usual pattern for White Lotus members), or they may be recruited directly to higher levels (as in the case of many heroin wholesalers with capital and connections to established upper level people). A little of everything may be included; for example, Pilecki did a lot of recruiting himself, encouraged recruits to recruit in turn, tried however at least to know who had been recruited indirectly, and facilitated recruitment to various levels in Auschwitz. But in all these cases, we see two common elements: these are our more hierarchical cases, and recruitment is somehow controlled from above. This control does not in itself guarantee a hierarchical outcome, since the control could be put to various uses, but it is a prerequisite for a hierarchical structure.

The more decentralized the control over recruitment, the more recruitment simply spreads like a diffusion process and generates a structure with very few hierarchical features. In the English marijuana network, for example, there was no centralization of recruitment. People not only "turned on" their friends whenever they chose, but also created new links within the network at will by introducing people, known to be users. It would be most unlikely for such decentralized use of ties from a nonhierarchical prior network to produce a hierarchical structure, and this improbable outcome did not occur. Ianni's (b) report on several street quasi-groups appears to be another instance of free-for-all recruitment generating nonhierarchical results, though in this case the pattern is a set of loosely connected cliques rather than the one dense network in the Cheltenham example. The Norwegian underground is perhaps an intermediate case; recruits were supposed to be carefully vetted but "very frequently a member was co-opted by a friend without any kind of procedure at all" (Aubert, 290–1).

It should be noted that a secret society may draw on more than one network in more than one way. This may in turn have an impact on the society's structure, rendering it more complex internally than the discussion above might suggest. For example, Ianni (a) describes three broadly defined levels of the Lupollo "family business" with three different prior networks as recruitment bases. The highest of the three is drawn from the

central kin group; the intermediate level, from near and distant relatives; the bottom level from those with some kind of nonkin personal tie to a member of the central kin group. The higher levels are recruited from more narrowly defined and more dense prior networks, and hence one would expect them to be more tightly knit. We know that the central kin group and upper level of the family business is indeed tightly knit, but the details of the lower level structure are not clear. Yet it seems reasonable to conjecture that the effect of varying prior networks, in this particular example, is to strengthen central control by fostering stronger and denser ties at the top than at the bottom. In other examples, the effect may be to induce structural variation from branch to branch (e.g., Kogon, 201–10 on differences between nationality groups in camp undergrounds).

#### BASES OF CENTRALIZED CONTROL: COMMAND OF RESOURCES

We have seen that the locus of control over recruitment combines with the nature of the prior network used for recruitment to shape both the overall structure of a secret society and some of its internal variations. But centralized control over recruitment (or other matters) requires a centralized power base of some kind. In all our more centralized and more hierarchical examples, there was central control of some key resource critical to the secret society's members. The leaders of the Auschwitz underground were often able to influence appointments to the more desirable camp jobs with better conditions and better chances of survival; through their agents in important administrative posts they were able to influence job allocation, to get advance warning of important changes in policy, and (through the camp hospitals) to save lives or to arrange the deaths of informers. White Lotus leaders drew on authority and also often on scriptures inherited from previous leaders, on special skills such as arts of healing, boxing, or meditation, and on knowledge of secret chants of supposedly great benefit. As rebellion drew nearer, they also emphasized promises of rank and reward to be given to the faithful after taking power. Turning to the more modified hierarchies, the central kin group in the Lupollo family business kept central control in part through the command of financial resources (e.g. the capital for the loansharking business), in part through their extensive patronage in legal and illegal branches of the business, in part through their carefully tended protective connections. In the San Antonio heroin market, higher level participants, like wholesalers, control the supply of heroin through restricted possession of capital and connections to producers or importers. However, neither capital nor connections are monopolized in this market, and it has been conjectured that it is less centralized and less hierarchical than Eastern markets (Moore). It is also probably the least hierarchical of the first four examples.

Turning to the most diffusely structured cases with least centralized

control, we also find the least centralization of important resources. In the English marijuana network, drugs are available on a fairly steady basis from several dealers and are also brought in sporadically by others so that no one person or group controls supplies. Even the poorest members of the network can afford drugs, and even the more marginal ones can find access to them. No basis for central control exists. Finally, in the Norwegian underground, most of the necessary resources for underground work, like ration cards, were dispersed among the members and less essential but still potentially relevant resources like social status were if anything inversely related to rank within the underground itself. This dispersion of resources went with frequent decentralized recruitment and decisions made by consensus.

Centralized control of resources makes for centralized control of organizational processes like recruitment and promotion and hence promotes a hierarchical structure. In turn, a hierarchical structure itself contributes to the centralized control of resources because higher level members have greater access to essential information and to other resources and benefits. In the Auschwitz underground, more central members knew more about the organization itself and about events inside and outside the camp; and they tended to get the better jobs. In the White Lotus, the leaders again knew far more about the sect organization and plans than the followers; the benefits of rank included status, sometimes sex, and control of considerable income which was partly turned to personal use. In the Lupollo family business, more central men knew more of the family affairs, had considerable income and prestige, and a valued solidarity based on their dense multiplex ties. In the San Antonio heroin market, some admittedly spotty evidence suggests that both profit and profit margin may increase with rank, and certainly security from arrest does so. In the Norwegian underground higher level leaders knew more about the information gathered for intelligence purposes and about the membership and organization of the underground and they thus enjoyed a greater sense of knowledge and control in a perilous situation; they had prestige and some ability to give orders within the underground world. It is not clear that they had any other rewards, including greater security. Finally, the English marijuana network did not show much rank-based benefits, although key dealers and older users had some status and notoriety. Overall, the more hierarchical organizations are also the most centralized in control of recruitment, control of resources and distribution of information and benefits.

Recruitment, resources, and structure have complex feedback relationships. I have just argued that hierarchical structure makes for centralized control of resources; these together are the basis of centralized control of recruitment; and, as argued in the previous section, centralized control of recruitment is a prerequisite for hierarchical structure. Thus hierarchy and centralization tend to be correlated cross-sectionally and tend to rein-

force each other over time. To refine this rather broad model of causal feedback it would be useful to have cases in which one of the three components (recruitment, structure, or resources) changed sharply so that its independent effects on the other two components could be more clearly seen than is possible for the examples used here. Finally, the earlier stages of this argument dealt with more asymmetric effects. Risk, which comes from sources beyond the secret society's control, starts the processes in motion. The need for trust then leads to the use of prior networks, which predate the secret society and set constraints on it.

### **The Dynamics of Secret Societies**

The theoretical discussion above paid little explicit attention to time and change; yet every example with well-documented time depth shows some kind of change. A full investigation of this topic would take us well beyond the scope of this paper, but a few remarks on matters directly connected with the present theoretical framework are in order.

First, some change is of little interest here because it is not structural. Naquin reports that White Lotus sects waxed and waned as new branches were set up, old ones merged or fissured, or branches simply gained or lost numbers, partly in response to the degree of government repression. None of these changes involved any alteration in the basic structural form of the sects.<sup>3</sup> Also excluded from current analysis are changes in policy, like the Lupollo family's shift toward more and more legal enterprises or the Auschwitz underground's varying policy on escapes, although I note in passing that deliberate shifts of overall policy are most likely for centralized structures. Second, some changes stem from sources outside the current area of analysis, primarily from the actions of the authorities (e.g., see Marx).

Some change is generated by the ongoing activities of the secret society and may include change of structure as well as change of size. As the members of a secret organization pursue their goals, they must engage in behaviors that cannot always be completely disguised or buried in the stream of mundane activities. Unusual activities are most easily recognized by others engaged in the same kind of pursuits, so that members may come to be aware of each other as members. Consider members of the same secret society first. Heroin addicts provide an extreme example: even if they are not showing such obvious signs of addiction as withdrawal symptoms, they engage in the frequent (often daily) routine of buying supplies and both the routines and the sources are well known to other addicts. Over time, one would expect the addicts in a distribution structure to come to know each other. Hughes provides some cross-sectional comparisons which fit this expectation, with newer users in smaller clusters and long-established users involved in large heroin communities. The

English marijuana network is also quite dense. The growth of dense ties among members is often made possible through prior networks: if members are recruited from one network, then they will have many additional ties there which may be absorbed into the secret society as the interaction and attraction and trust involved in the outside ties helps members to notice and acknowledge their mutual involvement in secret activities. Aubert notes that "narrow" recruitment in a small city implied that one underground member was quite likely to encounter another known under his real identity, which posed problems for the maintenance of a sparse hierarchical structure. In general, one would expect the proliferation of ties within the secret society to lead to a less and less hierarchical organization.

Next consider people in the same line of work, as it were, but in separate secret societies. Such people may also recognize the tell-tale traces of each other's activities and hence the two secret societies may learn of each other's existence, however incompletely; in turn this may lead to rivalry and conflict or to co-operation and merger. Garlinski reports that Pilecki, the Auschwitz underground organizer, was "naturally" aware of other military undergrounds besides his own and of political undergrounds as well. At first glance it does not seem so natural that undergrounds in such desperate circumstances would be known to outsiders; but it was so, and Kogon reports a similar pattern for other camps. Mutual awareness does make sense, however, in light of the fact that the various undergrounds often were trying to do much the same kinds of things. For example, several of the Auschwitz undergrounds were very eager to have communication links outside the camp in order to pass out information about conditions there and in order to get what outside help was possible. But the opportunities for outside links were rather limited, so that the same person was used as a messenger by two different groups in the camp communicating with two different bodies outside. Such overlapping people played a key role in the merger of different undergrounds. In a similar way, White Lotus sects tended to have a vague awareness of each other in part because practitioners of skills taught within the sects, like boxing or healing, often moved around and met colleagues from other branches. Thus links to other groups could be tracked down and expanded with some effort, and the leader of the Peking group did this so effectively that he expanded from one small local sect to a merger of five formerly separate sects in sixty villages in the Peking area, let alone his connection with groups to the south. The structural effect of such encounters between separate sects can vary. Separate hierarchical sects may simply merge into larger hierarchies; or the key linkage people may be lower down in the hierarchies and take advantage of their crucial bridging role to expand their own networks and power positions.

It has often been noted that participation in a secret society tends to be absorbing: the excitement and comradeship of shared risk can lead to

stronger and stronger affiliation with the secret society and with other members. Participants may gradually restructure their networks, interacting more and more with other participants and less and less with outsiders (Plant). This, and the tendency to mutual recognition discussed above, lead to increasing density of ties and thus decreasing clarity of hierarchical structure over time. It is in a way surprising that secret societies ever manage to stay very hierarchical for any length of time. When they do, one or more of several counteravailing tendencies may be at work. The levels and branches of the hierarchy may parallel cleavages in prior networks (e.g., drug organization in geographically separated networks); or lines of authority may be kept hierarchical in spite of dense ties, which requires firm central control of key resources (as in the White Lotus); or the presence of great danger may convince participants to avoid surplus ties in their own interests (as was often the case in undergrounds).

### **Future Directions**

Before the present argument can become a theory, there must be further data and analysis for two crucial topics: risk, and secret society structure. Risk has been the prime mover, defining a universe of discourse of organizations subject to particular organizing processes triggered in the first instance by danger. It is risk that makes trust imperative, and thus leads to the essential part played by preexisting networks, and so on. Now risk can certainly be conceptually decomposed and fruitfully analyzed. For example, there may be a connection between the basis of the threatened negative sanctions and the scope of secrecy. For members of the White Lotus, everything had to be kept secret because any form of participation in the sect was a crime and any official suspicion of such participation could lead to arrest and torture. For members of the Lupollo family a general secrecy is cultivated as a useful habit but many things (like the existence of the numbers game, its general organization and the family's leadership) are not well concealed; since none of this information is basis for legal action, it is not crucial to hide it.

A more complex set of possibilities is raised by the traditional belief that greater risk requires and evokes a more rigidly hierarchical structure. Does greater risk in fact go with tighter structures? In favor of this, one could note that risk may induce greater willingness to accept discipline and refrain from building surplus ties; also greater risk suggests use of more trusted others, or stronger prior ties, and strong ties may be easier to build into hierarchies because they are more sparse and more tightly cliqued (Granovetter). On the other hand, great risk may go with a need for wide-ranging resources to carry on the society's activities, which may in turn call for a wide variety of personal contacts, which are much more readily ob-



tained through less inbred weak ties and opportunistic recruitment than through inbred strong ties and recruitment in accord with a rigid hierarchical plan. Further, it is not clear that a more rigidly hierarchical structure is the best answer to security problems. In risky situations, members and hence the links they are part of, may be frequently deleted: they may be informed on, they may lose courage and leave, they may be removed at random by a hostile regime (like the Auschwitz inmates who were arrested in street round-ups, or were shot in the camp for getting a parcel). Without much link redundancy, the secret society would be in continual danger of being cut to pieces. Note that one of our most hierarchical examples, the Auschwitz underground, was far more dense than the planners' original visions of five or even three man cells in a sparse hierarchy; and a good thing too, since the underground was able to carry on even after the sudden arrest and execution of most of its top leaders.

The second major area for development is morphology. With only half a dozen well-reported cases, and in the early stages of theoretical development, it suffices to work with a crude single dimension like the degree of approximation to a hierarchy. But this may well not suffice forever. What aspects of hierarchical structure are important here? How shall we classify the many different ways that an organization can be something other than hierarchical in form? Empirically, how much variation does there tend to be in secret society structures? How can this variation be best conceptualized?

## Conclusions

The study of secret societies is important in part for intrinsic interest and in part, as Simmel (363) reminds us, because the topic illuminates extremes and hence is theoretically important. One could see secret societies as extreme cases of interest groups or purposive self conscious organizations as Simmel does; as organizations especially sensitive to their environment; as uniquely well encapsulated subcultures; or as the limit of personal recruitment to organizations. Already secret societies are prominent in discussions of rebellion and revolution or of crime and deviance.

The particular approach to secret societies taken here is, I believe, especially likely to prove helpful because it is especially general. The focus throughout has been on structure and not on content, on patterns of ties and relatively abstract features such as their strength rather than on culturally specific definitions such as the content of a strong tie. Nadel has most elegantly made the case for potentially wide applicability of structural analysis. The relevant prior network may vary from case to case, depending on participant understanding of the ties most to be trusted for a given purpose in a given context; but the structure of the prior network will

always have consequences for the structure of the secret society. We can thus use the same framework to discuss a nineteenth-century religious sect in Imperial China, or a network of soft drug users in contemporary England, or a Second World War underground in Auschwitz or Norway.

Appendix: Some Problems in Studying the Structure of Secret Societies

The study of secret societies raises some knotty and intriguing methodological problems. Clearly the task is, in general, a hard one, since the very secrecy that distinguishes these societies obscures them, especially when disclosure is truly dangerous. In this Appendix, I describe biases especially important for the study of structure, the central concern of this paper. For more wide-ranging discussions of research problems in fieldwork, there is a growing body of methodological work (see Becker; Weppner); for historiography, see the discussion in substantive reports (e.g., Garlinski; Naquin).

To help the reader keep track of this information Table 1 summarizes kinds of

Table 1:A. MAJOR BIASES—"HARD"

"Hard" Biases	Auschwitz Underground	White Lotus	Lupollo "Family"
<u>Ultimate Observers</u>			
Number	Many	200	Several major ones, dozens in all
Structural location	Varied, tends to be higher up	Varied, may tend to be higher	Top level
Training	General education	None; illiterate	One field-worker, several well-educated participants
Selection bias	Accessible survivors; Polish non-Jew non-Communists, higher in underground	Males, more active in rebellion	Males in central kin group
<u>Inter-mediaries</u>	For some accounts, political organization	Experienced Imperial officials	Lower levels seen through reports of upper level
<u>Other Data Sources</u>	German and Polish documents	Memorials, captured documents	Business listings
<u>Kind of Tie Reported</u>	Known as fellow members	Teacher-pupil; joint activity	For top level: kinship, authority, business interaction; for lower levels, lines of command

bias by secret society. Some of the biases are grouped together as "hard" biases because they refer to relatively clear-cut aspects of what was or could have been observed. I have assumed here the hearsay is not evidence, especially for secret societies; an observer *can* only report accurately on the relationships he has seen himself, quite aside from other biases that may creep in.<sup>4</sup> Thus I have noted the number of ultimate observers, their position in the secret society structure, their training if any, and obvious biases in the process by which they or their information came to the researcher's attention. Often the observer information comes to the researcher through intermediaries, who may filter or distort it, so the intermediaries are also noted. The distinction between observer, intermediary, and researcher roles is, of course, a very rough one since these roles can be combined in various ways. I have also noted whether the researcher was able to triangulate to some extent by drawing on other sources of information such as archives or police accounts. Finally, it is essential to be aware of the kinds of ties that are being recorded; the observers and researchers pay varying amounts of attention to varying kinds of relationships, which can have a considerable effect on the kind of structures portrayed. "Soft" biases, on the other hand, refer to ways in which observable things may be misrecorded because of preconceptions or distortions. The observers may

San Antonio Heroin Market	English Marijuana Market	Norwegian Resistance
40 interviewed, more observed	200	One main observer, the author, number of others unknown
All but top level and casual users on fringes, street levels most observed	Everywhere	Unknown
Redlinger, a sociologist; rest poorly educated	Some college educated, some not	Aubert, sociologist; others unknown
Heavier, longer- established users	Biased against isolates, lighter users, and possibly females and lower classes	Unknown
Some reports of highest levels from immediate subordinates		Unknown
Clinic records; social workers; police	Field observation; casual talk; introductions	"Published reports" (no references)
Economic, for higher levels; economic plus friendship and interaction lower down	Any tie including mutual awareness of drug use	Mutual or one-way awareness awareness of participation in underground; superior- inferior lines of command

Table 1:B. MAJOR BIASES—"SOFT"

"Soft" Biases	Auschwitz Underground	White Lotus	Lupollo "Family"
<u>Perceived Structure</u>	Underground army, hierarchy	Hierarchy	Kin-based hierar- chical enterprise
<u>Perceived Kind of Ties</u>	Fellow fighters	Teacher-pupil links	For top level, multiplex kin ties
<u>Prejudices re:</u>			
Categories	National, political and sex divisions	Female participation under-recorded	Other ethnicities, especially Blacks and Jews derogated
Groups	Rivalry among political organizations	Sect and officials hostile	
Vested interests	Claims to under- ground leadership of current political and prestige value	Members minimized participation of selves and others	Incriminating details concealed

misperceive relationships because they have strong beliefs about what the relationships are, or ought to be. Thus they may believe that they belong to a hierarchical organization, and report it as such, or they may believe they are among friends and report a much looser structure. The informants' conceptions of the structure and of the nature of the ties within the structure are noted. The informants may also be subject to prejudice, leading to misperception of the roles played by other organizations or by members of various social categories. And finally, they may of course distort information deliberately in order to serve their interests as they see them. This is by no means an exhaustive list of problems in studying secret societies; our interest here is in biases of general importance for the study of organizational structure.

For Auschwitz, observers include the major underground organizer reporting through detailed memoirs and dozens of others who provided documents and interviews. Most were educated though not trained in social sciences; they tended to be higher up in the underground and to be non-Communist non-Jewish Poles, hence likely to survive and to be available. Political rivalries, including claims to major roles in the now prestigious underground, may distort some accounts including Garlinski's. Prejudice between nationality groups was also common. Most observers were men, who took a rather paternalistic view of women in spite of their experience and opportunities so that the actual role of women may be underestimated. Participants, especially the many army men, thought of their organization as an underground army. All in all, the biases in this case are likely to lead to an overestimate of the rigidity of the hierarchical structure, both through the hierarchical images of structure and relationships so common to participants and through past and current hostilities that may lead to overemphasis on separations between groups.

The uprising by the Peking White Lotus sect was quickly crushed and ably investigated; the result is some 200 participant accounts, most of which can be

San Antonio Heroin Market	English Marijuana Network	Norwegian Resistance
Business, to higher levels; network <u>cum</u> market, to lower; market, to Reglinger	A subculture without explicit organization	Underground; probably hierarchical
Trust stressed by lower participants; not clear for higher	Friendship	Comradeship in danger; interdependence
Non Mexican-Americans disliked and distrusted	Possibly sexism; class barriers	Unknown
		Nominal top level, outside Norway, mistrusted and disliked
Probably many ties concealed in own and others' interests	None structurally relevant	None reported

cross-checked. The interrogated sect members had positions in varying levels and branches of the sect and were mostly illiterate; women were underrepresented since they were less likely than men to be arrested or recorded. Other sources include official and sect documents. Both the sect members and the government investigators saw the sect as a hierarchy. Officials may have overemphasized the organizational strength of the sect to excuse its partial success. Members had a vested interest in minimizing their own participation and in implicating as few others as possible, which may have led to some underestimate of the density of ties in the sect even though the investigators pressed for information about them. Again, the net effect of the biases is probably an overestimate of hierarchy.

Ianni, a trained fieldworker, spent several years observing the kinship group from which the top level of the Lupollo "family" was recruited. This group also supplied most of his generally well-educated informants. Supplementary information was taken from standard business sources for the legal parts of the family enterprise. For the central kin group, a wide variety of ties is recorded: kin ties, lines of command, power, deference, and business exchanges. For lower parts of the organization, lines of command were emphasized, e.g., in the organization chart of the numbers enterprise. In Ianni's view, the organization is definitely not a formal organization like a business. Nevertheless, both he and the participants see the family and the family business as hierarchical structures with clear lines of authority and degrees of access to information. Finally, informants tried to conceal incriminating details and Ianni was careful to avoid recording them, but such details did not include patterns of organization. Overall, the structure is probably less tidily hierarchical than it is painted, especially for the more indirectly and incompletely observed lower levels.

Redlinger, a sociologist, got 21 interviews with heroin users in a clinic and also interviewed and observed 19 others in the field. Most of his informants were poorly educated, seriously addicted, and from lower levels. Supplementary sources

(clinic records, social workers, and police) were also best informed about heavier users rather than casual users or top-level wholesalers and importers. The participants see the market "as a network of friendships, relations, constraints and crisis," while Redlinger sees it as a market. Where the market aspects are stressed, as for higher levels, the structure appears more hierarchical; where the trust and friendship aspects are stressed, as for lower levels, the structure appears more loose. Almost all the participants are Mexican-Americans who mistrust and exclude other ethnicities, possibly underreporting their participation as well. Informants probably concealed many of their connections to protect others and their own valuable knowledge about contacts. Overall, the information is fragmentary and apparently subject to various biases of often uncertain effect.

Plant did two years of fieldwork, tracing 200 drug users through informal snowballing. There is probably a bias toward more socially active and well-connected members, heavier users (61), middle-class users (105), and men (65). Information on ties in the network comes from the 200 informants, introductions that lead to the informants in the first place, observation, and remarks in conversations, with the various sources quite consistent. Any contact was included if the two actors knew each other and were aware of each other's membership in the drug subculture. The inclusiveness poses problems in comparison to other studies with a narrower focus on possibly more hierarchical subsets of ties. The members themselves saw users as a distinctive group and subculture knit by friendship ties. There is no indication they thought in terms of an organization or a hierarchy. These perceptions may have led to some underreporting of, say, the power and relative access to information of larger dealers. Prejudices may include the taken-for-granted assumption that women play only a marginal role as adjuncts to their men. Middle-class users often knew but infrequently interacted with lower-class users; Plant worked longer with the former so may be less informed about ties among the latter, who include many of the larger dealers. The main vested interest of the informants was to present themselves as good people even though users. A structurally relevant interest, self-protection, played little of any part here because risk was low. Overall, some of the biases may veil hierarchical elements somewhat but this network cannot be very hierarchical.

Aubert, a sociologist, participated in the Norwegian underground. He does not clarify his position, the extent to which he drew on other informants, nor the position of other informants. He screens his recollections through social theory, especially Simmel's, though he feels that his underground was not a secret society in Simmel's sense. Participants generally believed they joined in patriotic, dangerous enterprises requiring some separation of levels and branches for security, yet rendered egalitarian by a strong sense of "communality of fate." Those in Norway preferred localized control because only those on the spot could really understand and deal with security problems and localized control was more democratic. Possible biases are not sufficiently well described for their overall impact to be judged. Comradeship, interdependence, and democratic values may have led to an underestimate of hierarchical aspects; awareness of the need for security and belief in the traditional hierarchical model of security maximization may have led to an overestimate.

In considering the above discussion and Table 1, it is clear that each kind of bias touched on here must be considered in evaluating a report of a secret society's structure. Each of the biases has some impact for more than just one or two of the cases. This is no cause for despair, since good accounts tend to have the necessary information for a reasonable judgment.

## Notes

1. For example, Simmel's discussion of psychology may be useful when secret society participants are not constrained by the exigencies of danger. Organizations like the modern Freemasons show an elaboration that may reflect the love of planning (Simmel, 357) and Cohn's analysis of the Jehovah's Witnesses supports Simmel's (355) discussion of the importance of having and sharing secrets. On more structural issues, one might follow up the suggestion that secret societies be seen as a particular kind of voluntary organization, an extreme instance of a self-conscious special-purpose group ("the opposite of all spontaneous groups . . . Its social-psychological form is clearly that of the interest group," Simmel, 363). Safe secret societies may require further subclassification such as Wedgwood's distinctions based on manifest and latent function.
2. One distinction noted by several readers is that between criminal and political organizations. This distinction does usefully remind us of variables that might play a part in further analysis, such as: goals (personal vs. collective), type and pervasiveness of shared ideology, similar vs. shared interests. The impact of such variables is much less striking than that of risk so that analysis of their effects will require later work with an expanded data set. Meanwhile, note that these theoretically promising variables are poorly related to the more popular criminal-political dichotomy. Participants in some of the resistance organizations were motivated by personal benefits, not just collective goals, while participants in the marijuana network saw themselves and behaved as members of a community. Further, the definition of a secret society as criminal or political is itself a political decision varying with the stance of the observer. All the examples in this paper were officially defined as criminal by the prevailing regime. With one possible exception (Redlinger, where the relevant data are not given) participants in the organizations saw their activities as justified and the regime as wrong. As observers, our use of the criminal label is likely to reflect our sympathies. Thus the criminal-political dichotomy is both difficult to apply objectively and a poor guide to variables of theoretical interest.
3. When there are enough data, such routine fluctuations may follow stochastic processes; see for example, the interesting model of the diffusion of heroin use developed by Hunt and Chambers.
4. It is possible that secret society members are more accurate in observing these directly visible ties than people usually are in observing the relationships they encounter in more mundane settings, like workplaces. After all, danger sharpens the wits, the secret society member can hope to reduce his risk by careful observation and such observation will be enhanced by arousal. However, just the opposite may be true for relationships not directly observed; that is, the secret society member may be especially inaccurate. Information is likely to be especially well screened from him, while his need for a sense of control in a risky situation will lead him to think that he knows more than he does.

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# Youth, Voluntary Associations and Political Socialization

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## ABSTRACT

*A model to investigate the effects of adolescent participation in voluntary associations on early adult political activity is estimated using longitudinal data from a national sample of adolescents who were followed up some two years after being initially surveyed as high school seniors. Results indicate that adolescent participation in voluntary groups increases selective forms of political expression, both directly and as its effects are mediated by adult participation in voluntary associations. The effects of voluntary organizational involvement on political participation were strongest for members of instrumental voluntary groups.*

Through involvement in organizations, it has been argued, people come in contact with public issues (DeToqueville; Lane; Lipset) and develop skills, knowledge and dispositions conducive to various forms of political action. People may become politically conscious in other ways, but certainly participation in voluntary associations is one way to increase political interests and activity. This is the mobilization hypothesis of organizations and political participation (cf. Lane; Olsen). In fact there is much evidence to suggest that variations in the manner and degree to which people participate in the political process are linked to differentials in citizens participation in voluntary associations (e.g., Erbe; Gordon and Babchuk; Hanks and Eckland; Olsen; Sallach et al.). Yet this research has been concerned primarily with *adult* involvement and has generally overlooked the possibility that participation in organizations at the *adolescent* level may have subsequent effects on measures of adult political action. Political thinkers since Plato and Aristotle have felt that the most important political orientations have their sources in the preadult years (Beck et al.; Hyman). The family, and to a lesser extent the school (through its curriculum), peer groups, the mass media, and other adults show some evidence of being influential in the political socialization process (Jennings and Niemi). However, the thesis that people learn to participate in the political life of their community by

participating in one or more voluntary organizations *before* they reach adulthood has not been sufficiently explored. A few researchers (Jennings and Niemi; Niemi and Sobieszek; Ziblatt) using cross-sectional data have concluded that participation in school voluntary associations is generally unrelated to the formation of students' political ideas and attitudes. One obvious limitation of these studies, however, is they do not contain a measure of political *behavior* or of citizens' future participation in politics.

In more recent work, Hanks and Eckland had appropriate data which allowed them to analyze the relationship between adolescent participation in voluntary associations and a later measure of adult political behavior. Their analysis indicated that such participation did have positive effects on subsequent political behavior and that these effects were primarily indirect, as mediated by respondents' degree of participation in adult voluntary associations. Only a single political participation item, voting behavior, was available for their analysis. In a similar inquiry, Otto found a positive relationship between participation in school extracurricular activities and a later measure of adult social integration. The latter construct, adult social integration, was measured by combining two adult indicators: memberships in voluntary associations and political participation rates.

The present study continues to explore the possibility that youthful involvement in organizations contributes to the process of socialization to citizenship. A path analytic model is presented that both estimates the effects of youths' participation in voluntary associations on various forms of early adult political behavior and evaluates the likelihood that self-esteem and adult participation in voluntary groups may mediate between adolescent involvement in organizations involvement and early adult political behavior. Finally, the significance of *type* of organization involvement for political participation is assessed in terms of instrumental and expressive characteristics of secondary associations that affect the form and extent of early adult political participation. The study exploits two-year interval panel data drawn from a national probability sample of the U.S. population. The present study differs from those cited above mainly in scope: it contains a more inclusive set of political outcomes, relatively detailed coding of the voluntary association items, and a larger and more heterogeneous population.

## The Model

The substantive model used for this analysis incorporates three causal steps.<sup>1</sup> Beginning the sequence are six exogenous variables which include parents' socioeconomic level, academic aptitude, class rank, self-esteem as measured in students' senior year, and measures of both instrumental

and expressive modes of youth participation in voluntary groups. The instrumental-expressive conception of voluntary associations used in this paper is to be found throughout the sociological literature on adult organizations (e.g., Gordon and Babchuk; London; Rogers et al.; Rose). The political relevance of this conception is that instrumental groups are generally expected to have a higher political content than expressive groups. Accordingly, it should be through participation in the former activities that the development of skills and attitudes which foster political activity in adulthood are most likely to be fostered.

The selection of organization types was directed not only by theoretical considerations but by past research as well. In general, previous studies have found that adult members of instrumental groups are more likely than others to be found in activities defined as political (Jacoby; London; Rogers et al.; Verba and Nie). What remains unclear, however, is whether different types of *adolescent* participation in voluntary groups yield different outcomes (see Spady). This paper will examine whether there are later differences in political activity between students who participate in instrumental as opposed to expressive voluntary associations.

The second step includes three  $T_2$  (time two) variables, self-esteem, instrumental and expressive organization affiliations, as measured two years after students' graduation from high school. The inclusion of the first, self-esteem, is an attempt to evaluate a psychological interpretation of the process mediating between adolescent social participation and early adult political behavior. Some (Neal and Seeman; Otto and Featherman) have argued that organization involvement mediates between the individual and the larger society, with resultant salutary effects on individuals' feelings of personal efficacy and well-being. Others have stressed self-esteem as a psychological factor related to characteristics of a person's self which hinder or promote involvement in politics (Litt; Orum).

The remaining two intervening variables, early adult participation in instrumental and expressive groups respectively, stress the educative functions of voluntary organizations for the structuring of citizens participation in politics. Hanks and Eckland reported empirical support for the idea that participation in youth voluntary associations affects "adult participation in voluntary associations directly by providing a setting for the rehearsal of interpersonal and organizational skills which can be immediately applied to role performance in adult associations . . ." (482). At the same time, adult voluntary associations are known to be effective in mobilizing their members for political action (Olsen; Rogers et al.). However, the literature has suggested that organizations may differ in their capacities to stimulate political participation. Thus voluntary association activity at the adult level, as at the adolescent level, is specified by type of organization function, i.e., instrumental or expressive. For this purpose, however, it is necessary to consider the pattern of linkages among adolescent and

adult organizational types. Since there are similarities in the makeup of the measures of adult and adolescent, instrumental and expressive organization types, it is expected that adolescent participation in instrumental groups will be more closely related to adult participation in instrumental groups (and likewise with expressive groups). Perhaps the clearest example of this is the linkage between participation in interscholastic sports and participation in various sports clubs and organizations at the adult level. In other words, the learning of adult instrumental and expressive roles is probably carried out somewhat differently in the instrumental and expressive voluntary organizations associated with adolescence. One other aspect of this stage of the model must be mentioned: no causal structure between any of the  $T_2$  measures of organization involvement and self-esteem is proposed but, as independent variables, their correlation will be taken into account.

The final step measures three separate forms of political participation: discussion of the issues, participation in campaigns, and voting. Several authors (Milbrath; Orum; Verba and Nie; Zipp and Smith) have emphasized that voting and other modes of political expression "are qualitatively different forms of activity that need to be examined separately" (Zipp and Smith, 167). Underlying this multidimensional view of political participation is the concept of citizens' involvement. Prior research identifies voting as "the most popular but also the activity that involves the least effort on the part of citizens" (Orum, 288). At the other extreme is the act of campaign participation, which attracts fewer people but requires greater commitment and involvement (Milbrath; Zipp and Smith). In view of these differences, it is hypothesized that the voluntary association factors will have a greater impact on political activities other than voting.

## Methods

The data for this study come from the "National Longitudinal Study (NLS) of the High School Senior Class of 1972." The survey was conducted by the Educational Testing Service and Research Triangle Institute and sponsored by the National Center for Education Statistics. Data were collected from a representative national sample of high school seniors who were enrolled in some 1,200 public, private, and church affiliated schools within the 50 states of the United States and the District of Columbia. Students who participated in the Base-Year Survey (1972) were administered an approximately eighty-minute questionnaire and a sixty-minute test battery which measured verbal and nonverbal skills. A one-year follow-up survey was begun in October 1973 and completed in April 1974 and a second follow-up survey was conducted between October 1974 and April 1975 by the Research Triangle Institute. Of the seniors who had participated in the first-

year follow-up survey, 94.6 percent also participated in the second follow-up survey—72 percent by mail and 28 percent by personal interview.

The sample used in this study is composed of 10,245 black and white respondents from the second follow-up with no missing data.

The *social class background* measure is based on an index constructed by the NLS staff of five equally weighted items: father's education, mother's education, parents' income, father's occupation, and household items. Reliance on an equally weighted item index for social class has been questioned (cf. Sewell and Hauser), but other analyses conducted with separate social class items produced no significant departures from the major findings presented below. *Academic aptitude* was measured by an equally weighted linear composite of standardized test scores in four areas: vocabulary, reading, letter groups, and mathematics. *Senior class rank* data were obtained from school records and coded in deciles.

The  $T_1$  and  $T_2$  measures of *self-esteem* were measured by an index constructed by the NLS staff from summing four Likert-type items: (a) "I take a positive attitude toward myself," (b) "I feel I am a person of worth, on an equal plane with others," (c) "I am able to do things as well as most other people," (d) "On the whole, I'm satisfied with myself."

*Adolescent participation in voluntary associations* is treated as two separate constructs, instrumental and expressive (cf. Gordon and Babchuk; Rose). The former includes memberships in those organizations considered to be externally oriented, whose primary activities serve as means to an end. The five Likert-type activity items included in this category were: (1) "Honorary clubs such as Beta Club or National Honor Society," (2) "school newspaper, magazine, yearbook, annual," (3) "school subject matter clubs such as science, language, history, business," (4) "student council, student government, political clubs," (5) "vocational education clubs such as Future Homemakers, Teachers, Future Farmers of America, Distributive Education Clubs of America, Office Education Association, Future Business Leaders of America, Vocational Industrial Clubs of America." The other type, expressive participation, includes memberships in those organizations considered to be internally oriented, whose primary activities serve as ends in themselves. The four Likert-type activity items included in this category were: (1) "athletic teams," (2) "cheerleaders, pep club, majorettes," (3) "debating, drama, band, chorus," (4) "hobby clubs such as photograph, model building, hot rods, electronics, crafts. These two indices of instrumental and expressive participation respectively, were operationalized by summing within-category responses. *Adult participation in voluntary associations* is also treated as being instrumental or expressive. Into the instrumental category went seven Likert-type items: (1) "union, farm, trade or professional associations," (2) "political clubs or organizations," (3) "community centers, neighborhood improvement, or social-action associations or groups," (4) "educational organizations—such as Parent Teacher Asso-

ciation or an academic group," (5) "service organizations—such as Rotary, Junior Chamber of Commerce, Veterans, etc.," (6) "organized volunteer work—such as in a hospital," (7) "youth organizations—such as Little League coach, scouting, etc." In the expressive category were included four Likert-type items: (1) "church or church-related activities (not counting worship services)," (2) "a social, hobby, garden, or card playing group," (3) "sport teams or sport clubs," (4) "a literary, art, discussion, or study group." Internal consistencies were calculated for each of the four derived organization involvement scales. The alpha coefficients were .51, .50, .44, and .32 for the adolescent instrumental, adult instrumental, adult expressive, and adolescent expressive scales, respectively. The internal consistency values for the adult and adolescent expressive participation scales were relatively low and may complicate interpretation of the measures of relationship. Hence, the findings generated from this phase of the analysis that pertain to differences among *types* of organizations must be regarded as tentative.

Data on *voting* were obtained from two items which were used to develop a summated index. These included: "Are you registered to vote?," and "Did you ever vote in a local, state, or national election?" *Discussion of the issues* was determined by summing responses to a list of five Likert-type items which asked the extent to which the respondent "ever talks about public problems" with any of the following people: friends, fellow workers, family, community leaders, and elected government officials.

Four items were summed to form the *campaign participation* index: (1) "Have you ever talked to people to try to get them to vote for or against any candidate?," (2) "Have you ever given any money or bought tickets to help someone who was trying to win an election?," (3) "Have you ever gone to any political meetings, rallies, barbecues, fish fries, or things like that in connection with an election?," (4) "Have you ever done any work to help a candidate in his campaign?"

The basic analytic techniques used in this study were multiple regression and path analysis. The regression coefficients to be reported may be interpreted as net, standardized direct effects. In addition, the distinction is drawn between total effects, indirect effects, and direct effects, based on the standard "tracing" rules of path analysis. Indirect effects are defined after Finney and Lewis-Beck as the sum of all effects that are transmitted through an intervening variable(s).

A preliminary test for race and sex interaction effects which is not presented, revealed no important interaction effects. Therefore, only one model for the entire sample will be examined. Means, standard deviations, and inter-item zero-order correlation coefficients for all variables are presented in Table 1.

The univariate distributions of all variables in the model are not presented, but the percentage distributions of adolescent and adult organi-

**Table 1.** ZERO-ORDER CORRELATION COEFFICIENTS, MEANS AND STANDARD DEVIATIONS FOR VARIABLES EMPLOYED IN THE MODEL ( $N = 10,245$ )

	1	2	3	4	5	6	7	8	9	10	11	12	Mean	S.D.
1. SES	.	.427	.189	.066	.059	.134	.062	.081	.101	.081	.128	.192	-0.01	0.71
2. Academic aptitude		.	.576	.074	.154	.105	.062	.082	.074	.105	.124	.189	205.50	32.02
3. Class rank			.	.116	.318	.145	.034	.070	.083	.080	.086	.147	55.05	27.58
4. Self-esteem 1				.	.130	.127	.372	.083	.105	.120	.097	.076	15.70	2.58
5. Instrumental part. (adolesc.)					.	.376	.057	.207	.196	.182	.212	.133	6.31	1.52
6. Expressive part. (adolesc.)						.	.075	.193	.270	.158	.148	.130	5.35	1.27
7. Self-esteem 2							.	.084	.102	.162	.096	.060	16.79	2.34
8. Instrumental part. (adult)								.	.368	.281	.318	.128	7.82	1.49
9. Expressive part. (adult)									.	.249	.216	.118	5.60	1.77
10. Discussion of issues										.	.440	.195	9.44	2.09
11. Campaign participation											.	.277	5.29	1.66
12. Vote												.	1.33	0.86

zational memberships are 83 percent belonging to at least one adolescent group, 69 percent belonging to at least one adult group, 58 percent belonging to two or more adolescent groups, and 40 percent belonging to two or more adult groups.

## Results

All the standardized parameters for the model are presented in Table 2. However, those for the direct paths from youth organizational involvement to early adult political participation are of primary interest here. Table 3 disaggregates the bivariate relationships of youth organizational involvement with early adult political participation into the total, direct, and indirect components.

In Tables 2 and 3, it can be seen that adolescent participation in voluntary organizations is positively related to early adult political activity. At the same time, however, there are notable differences between memberships in instrumental and expressive groups for the relationship between organization involvement and political participation. Considering direct effects only, the measure of adolescent *instrumental* participation is significantly and positively associated with all three measures of early adult political participation and the relationship is, as expected, much stronger for discussion of the issues and campaign participation than it is for voting. The other mode of youth involvement in organizations, *expressive* participation, appears to have a much smaller effect on political activity. This measure has quite weak direct effects on discussion of the issues and voting, and a positive but insignificant effect on campaign participation.

An examination of the indirect effect coefficients for youth participation in voluntary associations, displayed in Table 3, indicates that self-esteem ( $T_2$ ), operating as an intervening variable, has almost no effect in mediating the influence of adolescent participation in either instrumental or expressive voluntary associations on any of the three measures of political participation. This reflects the fact that the overall relationship between youth involvement in voluntary organizations and later self-esteem is miniscule. On the other hand, the conclusion that self-esteem is positively related to indicators of political activity appears to be warranted even though the relationships are quite modest. In comparison, the measures of instrumental and expressive adult activity in organizations both enhance the explanatory power of the model. This can be seen by examining the indirect effect coefficients in Table 3. For example, summing the indirect paths for adolescent participation in instrumental groups increases its total effect on discussion of the issues from .090 to .135, of which one-third is transmitted through adult participation in voluntary associations.

Concerning differences between instrumental and expressive volun-



Table 2. STANDARDIZED REGRESSION COEFFICIENTS FOR MODEL

Predictor Variables	Self-Esteem 2	Instrumental Part. (Adult)	Expressive Part. (Adult)	Discussion of Issues	Campaign Participation	Vote
SES	.022	.043	.058	.010*	.064	.125
Academic aptitude	.049	.044	.008*	.063	.065	.091
Class rank	-.046	-.038	-.001*	-.024	-.038	.033
Self-esteem 1	.370	.045	.059	.030	.029	.026
Instrumental part. (adolesc.)	.006*	.160	.097	.091	.133	.056
Expressive part. (adolesc.)	.023	.122	.216	.035	.011	.049
Self-esteem 2				.110	.041	.018
Instrumental part. (adult)				.189	.244	.067
Expressive part. (adult)				.135	.083	.043
R <sup>2</sup>	.142	.065	.090	.138	.147	.077

\*Coefficients are less than twice their standard error.

**Table 3.** DISAGGREGATION OF TOTAL EFFECTS INTO DIRECT AND INDIRECT EFFECT COMPONENTS OF ADOLESCENT SOCIAL PARTICIPATION ON EARLY ADULT POLITICAL BEHAVIOR

Dependent and Independent Variables	Total Effect	Self-Esteem 2	Indirect Effects Via		Expressive Part. (Adult)	Effect
			Instrumental Part. (Adult)			
Instrumental part. (adolesc.) on political discussion	.135	.001	.030		.013	.091
Expressive part. (adolesc.) on political discussion	.089	.002	.023		.027	.035
Instrumental part. (adolesc.) on campaign activity	.180	.000	.039		.009	.133
Expressive part. (adolesc.) on campaign activity	.060	.001	.030		.018	.011
Instrumental part. (adolesc.) on voting	.071	.000	.011		.004	.056
Expressive part. (adolesc.) on voting	.066	.000	.008		.009	.049

tary association memberships at the *adult* level, the patterns of the two types of involvement are quite similar with two notable exceptions. First, adult involvement in instrumental groups is about twice as likely to occur among those who participated in instrumental groups in adolescence than among those who participated in expressive groups in adolescence; the reverse is true for adult involvement in expressive groups. As stated earlier, these patterns probably reflect the similarities in makeup between youth and adult types of organization. Second, participation in political campaigns is about twice as likely to occur among those who are involved in instrumental groups in adulthood as among those who are involved in expressive groups. This finding is as expected and corroborates previous research reporting substantive differences between types of voluntary organizations.

## Discussion

In sum, this research indicates that, independent of social class background, ability, academic performance, and self-esteem, participation in voluntary organizations in adolescence is related to the form and extent of people's participation in political activities in adulthood. More specifically, adolescent participation in voluntary associations has direct positive effects on discussion of the issues, campaign participation, and voting rates some two years later, plus indirect positive effects through the mediating factor of early adult involvement in organizations. Even when controlling for the personality factor, self-esteem, members of voluntary associations exhibit relatively high rates of involvement in the several forms of political activity examined in this paper. Additional refinements of the analysis reveal that (1) when broken down by type of organization, participation in instrumental voluntary groups is more efficacious in predicting respondents' scores on the political activity variables than is participation in expressive voluntary groups, and (2) extravoting political activities—campaign participation and to a lesser extent discussion of the issues—are more strongly associated with the measures of voluntary association activity than is voting rates.

There are, of course, several underlying processes which could account for the observed association between adolescent social participation and early adult political activity. However, the interpretation stressed in this paper places emphasis on the educative aspects of organized voluntary action. In this view, often termed the mobilization perspective, involvement in voluntary associations provides members with attitudes, incentives, information and other personal skills and resources necessary for effective political action. On the other hand, the data used in this study do not directly test the mobilization hypothesis. Moreover, there are competing explanations for these data which must be ruled out in order to demon-

strate a socialization effect. The most plausible alternative interpretation is that some as yet unidentified factor determines *both* adolescent social participation and later political participation. Although controls were used for some of these potential sources of spuriousness, there is the possibility that the positive relationship between adolescent social participation and early adult political participation reported in this study is not the true one, but rather is simply an artifact of some antecedent factor. In particular, it seems likely that young persons who are interested and relatively proficient in citizenship activities are *attracted* to voluntary associations in adolescence, but there is no measure of political interest, predisposition toward citizenship activities, or some other such factor, during adolescence to act as a control. Hence, while a strong impression is that some degree of anticipatory socialization occurs in youth voluntary associations, further research is needed on the causal relationships between organization involvement and various modes of political participation to clarify the origins of the political role and to sort out the relative impact of psychological, social-psychological, and structural effects.

One further implication of these data can be mentioned. The findings presented here add to the growing (but still relatively few) number of empirical studies (Hands and Eckland; Otto; Otto and Featherman) which show that the participation of adolescents in voluntary associations, most of which takes place under the auspices of the school (Payne et al.) and is quite manipulable by school officials and community leaders, may have a number of positive consequences that carry into adult life.

### Note

1. Indices of adult socioeconomic status, which are known to be positively correlated with measures of social and political participation, are absent from the model because the sample is too young, only two years separating the respondents from their senior year in high school. The percentage of respondents in various activities at the time of the follow-up were 36 percent in school full time, 6 percent in school part time, 49 percent employed full time, 19 percent employed part time, and 5 percent in the military.

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# Another Methodological Confounder: Comment on Witt, Lowe, Peek, and Curry\*

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In their recent article, Witt et al. examined whether changes in the reported positive association between age and happiness was an emerging trend or an artifact of changes in the methods used to examine this association. The authors conclude that, from 1948 to 1977, the relationship between age and happiness has shifted from essentially no association to a weakly positive one, and that it is no methodological artifact. Reasons offered for greater happiness of the elderly include: improved economic security from governmental programs (e.g., Medicare), less negative health perceptions as a result of geriatric developments, and increases in their political power.

However, Witt et al. also point out several recent studies that report no such positive relationship (Alston et al.; Edwards and Klemmack; Gaitz and Scott; Spreitzer and Snyder).

My purpose is to identify another methodological variable, not considered in the authors' analysis, that may challenge their conclusions. The findings of the researchers just cited, indicating no relationship between age and happiness, may still be supported.

In their inquiry, Witt et al. were able to effect useful changes in methodology: (1) sampling adjustments enabled the inclusion of previously underrepresented groups (e.g., nonwhites and southerners); (2) increased detail in the measurement of both age and happiness; and, (3) the use of more control variables.

But one important methodological matter, not considered in the authors' analysis, is who actually completed the questionnaire. It is often assumed that the respondents themselves wrote their own answers. However, with today's much larger percentage of aged who are handicapped with chronic disorders (Atchley; Coe, Ch. 1), many respondents are likely to have had a relative, friend, or service provider (e.g., social worker, nurse, et al.) write the answers that the respondent tells them to. Elderly respondents may be more prone to report greater happiness than they actually feel, when a relative, friend, or service provider is writing the answers, to avoid offending this person who has tried to make them happy.

\*David D. Witt, George D. Lowe, Charles W. Peek, and Evans Curry. "The Changing Association Between Age and Happiness: Emerging Trend or Methodological Artifact?" *Social Forces* 58(June 1980):1302-07.

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Thus, there is a greater likelihood of a halo effect bias to indicate an increased percentage of happy elderly today than in the past.

In March 1980, a survey of handicapped elderly was conducted in Sioux City, Iowa and surrounding areas ( $N=129$ ). Two items asked of the respondents were their *age* and *who completed the questionnaire* (oneself, a relative, a guardian, a friend, or a service provider). The bottom row of Table 1 shows that a significant percentage of respondents (43%) had somebody else complete the questionnaire for them. In the 50–59 and 60–69 age groupings, about one third of the respondents had someone other than themselves complete the questionnaire for them. Even more potentially biasing, most respondents in the 70–79 and 80 and over age groupings (57% and 64%, respectively) had someone else complete the questionnaire for them.

This study can only suggest the potential for bias stemming from surrogate respondents. More research is called for to measure the degree of distortion, if there is any, that having someone else complete one's questionnaire has on age and happiness research. Two ways to test this are: (1) to conduct the same kind of study that Witt et al. did with additional control over who completed the questionnaire; and, (2) to administer the same questionnaire to two samples of elderly individuals with one sample being sent a questionnaire and the other sample being interviewed (to assure that they answer the questions themselves and in private) and then to measure for any differences in reported happiness between the samples.

The Bureau of the Census reports that in 1950 8 percent of the U.S. population was over 65 years of age. It is predicted that in the year 2000 15 percent of the population will be over 65. It is important for gerontological research to assess, accurately, the happiness and well-being of this growing sector of the U.S. population. With findings such as Witt et al.

**Table 1.** PERCENT DISTRIBUTION, RESPONDENT OR OTHERS, COMPLETING THE QUESTIONNAIRE, BY AGE OF RESPONDENT

Age of Respondent	Who Completed the Questionnaire?		
	Respondent	Family, Friend, or Service Provider	Total Percent (N)
50-59	68	32	100 (50)
60-69	61	39	100 (36)
70-79	43	57	100 (21)
80 and over	36	64	100 (22)
Total	57	43	100 (129)

that indicate only a very small increase in happiness among the elderly, methodological biases such as the one discussed in this paper may inaccurately portray the elderly as a satisfied group. This may result in the failure to develop social programs that can improve the conditions under which they live.

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# Presence of Significant Others and the Avowal of Happiness Among the Elderly: Reply to Bobys

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Bobys' thesis is that the changing relationship between age and happiness we reported is quite possibly a methodological artifact, despite our not inconsiderable efforts in the study to insure that it was not.

The problem, to state his argument a little more formally, is that we failed to consider the joint effects of two forces. First, older respondents are more likely to have a third party help them complete their questionnaires, one who is probably a significant other ("a relative, friend, or service provider"). Second, older persons responding through a significant other are more likely to report greater happiness than older persons filling out their own questionnaires, because the former wish to avoid offending "this person who has tried to make them happy." Thus, Bobys thinks that the positive relationships we reported for age and happiness in the 1970s could be due to significant others' influence on response to questionnaires and therefore an artifact of the methodology employed by the pollsters whose data we used.

We have to reject Bobys' *specific* argument out of hand. *Interviews*, not questionnaires, were used to gather these data. Respondents could neither fill out their own schedules nor ask some significant other to do it for them, because they had to respond to each question as put to them by a trained interviewer.

However, Bobys' *general* point may be more applicable to interview data. The presence of significant others during the interview might increase the level of happiness avowed by the aged, even though these significant others do not answer any of the questions. The elderly may not only simply wish to avoid offending someone who has made them happy (as Bobys notes), but more importantly, may wish to avoid offending significant others because many elderly are so dependent for survival on these significant others. Further, it seems not unreasonable to expect that

the presence of third parties during the interviewing process might be more likely with very old respondents than with other adults.

Survey organizations are, of course, aware of the potential for response bias generated by the presence of third parties. A quick check confirmed that for years (certainly back into the early 1960s) interviewers have been instructed to interview respondents alone if possible. Explicit instructions on strategies for achieving this goal have even been provided. Nevertheless, an unknown but likely small proportion of interviews may still be conducted in the presence of third parties. To have affected our findings, such interviews would have had to occur more frequently during the NORC surveys in the 1970s, since the significant positive relationships between age and happiness appeared in these polls. Because NORCS General Social Surveys were more nearly under the control of the social science community than were the earlier surveys, the greater presence of third parties during NORCS interviews seems rather unlikely. However, the positive relationships in the NORC polls of the 1970s were not all that strong, and the shift of only a few old respondents toward unhappiness might well have altered our conclusion.

Fortunately, we can investigate this possibility. There is a subsample of General Social Survey respondents for whom the likelihood is low that significant others would have been present during the interview: those living in single person households. If the positive associations between happiness and age are artifacts of the presence of third parties during interviews, the associations should not appear in this subsample. To see if this were the case, we regressed happiness on age within this subsample of respondents while controlling for two other important variables: perceived health and social isolation. Perceived health was demonstrated in our original analysis to have more impact on the age-happiness relationship than any of the other ten control variables. Since respondents in this particular subsample live alone, it is also necessary to disentangle the effects of age from those of social isolation. In the 1974, 1975, and 1977 surveys, a rough control for social isolation (the negative of average reported frequency of visits with relatives, neighbors and friends) is available.

Results of these regressions in Table 1 do not support Bobys general position at the presence of significant others artificially inflated the level of happiness reported by the aged. The associations that appeared in our original analysis also occur among persons living alone. All of the zero-order correlation coefficients representing the age-happiness association are positive and, except for 1975, are actually slightly larger than those reported for the total samples (Witt et al.). After perceived health is entered into the regression equation, all of the standardized partial regression coefficients are positive and four are statistically significant. After social isolation is entered in the three years for which this control is available, all age-happiness relationships are significant.

**Table 1.** ASSOCIATIONS BETWEEN AGE AND HAPPINESS AMONG INDIVIDUALS IN SINGLE PERSON HOUSEHOLDS

Year of General Social Survey	Number	Zero-Order <i>r</i>	Standardized Regression Coefficients		<i>R</i> <sup>2</sup>
			With Control for Perceived Health	With Control for Perceived Health and Social Isolation	
1972	150	.046	.164	. . .	.076
1973	150	.057	.223*	. . .	.115
1974	165	.262	.317**	.346**	.170
1975	199	.056	.135	.174*	.135
1976	234	.015	.132*	. . .	.104
1977	259	.053	.132*	.158*	.080

\*Regression coefficient significant at the .05 level.

\*\*Regression coefficient significant at the .001 level.

Therefore, the positive associations we initially reported cannot be due to bias toward avowal of happiness among the elderly generated by the presence of significant others. We repeat with confidence: the changing association between age and happiness is an emerging trend, not a methodological artifact.

We think it is very unlikely that this finding will be used as an excuse for failing to support social programs to improve conditions under which the elderly live, as Bobys fears it might. First, as we originally cautioned (Witt et al.), the statistically significant increments in happiness with increasing age are small. Second, these increments occur most clearly *after* controls for conditions like those that social programs are designed to change (health in the original analysis, health and social isolation in the present one). Finally, the positive regression coefficients between age and happiness indicate only that the values of one variable *increase* when the values of the other do. These coefficients say nothing about whether the *level* of happiness is acceptable, and thus cannot "portray the elderly as a satisfied group."

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- Witt, D. D., G. D. Lowe, C. W. Peek, and E. W. Curry. 1980. "The Changing Association Between Age and Happiness: Emerging Trend or Methodological Artifact?" *Social Forces* 58(June):1302-07.

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## Book Reviews

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### The Two Nisbets: The Ambivalence of Contemporary Conservatism

JEROME L. HIMMELSTEIN, *University of Michigan*

#### **History of the Idea of Progress.**

By Robert Nisbet New York Basic Books, 1980.

Robert Nisbet's *History of the Idea of Progress* is ostensibly a straightforward, thoughtful reevaluation of the place of the idea of progress in Western civilization. It argues that the idea of progress has deep roots in that civilization and underlies most of what is good in it. It then attempts to take a critical view of the contemporary despair over the possibility of further progress in the West.

At a closer look, however, *History of the Idea of Progress* is a very strange book indeed. Previous social thinkers have sometimes attempted to stand their predecessors on their heads. Robert Nisbet does them one better: he stands himself on his head—apparently without the least recognition that he is doing so. The Robert Nisbet of *The Sociological Tradition*, who discovered the conservative roots of sociology and popularized the conservative critique of modern Western society and the idea of progress, is transformed into the Robert Nisbet of *History of the Idea of Progress*, defender of that society and that idea. More correctly, the transformation is attempted, but ultimately fails. In the end, the Old Nisbet reappears and joins the very critics of modern society that the New Nisbet has sought to rout.

Nisbet's attempted self-transformation takes the form of a reassessment of the idea of progress. The conventional wisdom has it that the idea of progress is a late arrival in Western history, the product of the Enlightenment. It is thus conceived as a repudiation of the past, of tradition, and of religion. Not so, counters Nisbet: the idea of progress goes back as far as Western civilization itself and underlies all its achievements. It is tightly interwoven with the development of Christianity.

What precisely is this "idea of progress?" Nisbet defines it as the belief "that mankind has advanced in the past—from some aboriginal condition of primitiveness, barbarism, or even nullity—is now advancing, and will continue to advance through the foreseeable future." This advancement has two components: the improvement of knowledge (and with it the material condition of humanity) and the perfection of humanity's moral or spiritual condition. Progress of both kinds is seen as cumulative, continuous, and unilinear—unfolding in systematic fashion according to divine plan, natural law, or simply the inner logic of things. It implies a basic connectedness between past, present, and future, with each moment containing the seeds of the next.

Nisbet sees elements of this idea of progress throughout Western history. The classical Greeks and Romans, along with their images of history as decline or as cycle, developed a nascent idea of progress in human knowledge. Early Christianity, most notably in the work of Augustine, furnished the idea of the progressive spiritual perfection of humanity, culminating in the realization of the City of God in this world.

The idea of progress toward spiritual perfection was further developed in the Middle Ages, a period, according to Nisbet, far different from its popular image of other-worldliness, stagnation, and gloom. In fact, he argues, the medieval world was intensely involved in secular artistic, intellectual, technological, and economic pursuits; it made great material progress; and, most importantly, it cultivated many of the elements of the idea of progress. Nisbet refers to the prevalence of utopian and millennial images in medieval thought and to the importance of notions of "plenitude" (that everything needed for the perfection of existence was immanent in the world) and "continuity" (that every state of being has within it the seeds of a higher state).

The only break in the development of the idea of progress, according to Nisbet, came with the Renaissance. Given Nisbet's view of the Middle Ages, of course, the term "renaissance" is a misnomer, since there was no need for a rebirth. Whatever one calls the culture of fifteenth- and sixteenth-century France and Italy, however, it lacked an idea of progress, according to Nisbet's criteria. Cyclical images of history (*ricorsi*) predominated. The immediate past was repudiated, and thus there was no basis for the historical continuity essential to the idea of progress. The Renaissance, moreover, was awash in the subjective and the irrational, neither of which fits with an idea of progress. Nisbet finds the legacy of the Renaissance elsewhere, in "the beginning of a line of intellectuals—the Italian humanists of the fifteenth century—that continues to this day, in which the flouting of tradition, the spirit of counterculture, and the exaltation of the wayward or dissident are the high water marks."

The religious ferment of the Reformation, Nisbet argues, gave the idea of progress a new impetus. Puritanism in particular, achieved the synthesis of the classical view of progress as the development of the sciences and practical arts with the Christian idea of progress as the spiritual perfection of mankind. In the process, both were transformed. The advancement of knowledge took on a moral, redemptive value: it came to be seen as essential to the development of religion and the realization of God's kingdom on earth. Conversely, the idea of spiritual perfection became secularized: God was gradually transmuted from a prime mover into a process of unfolding according to natural law. Science and religion, rather than being antagonists, united in the Puritan idea of progress.

The idea of progress came to dominate Western thought fully in the period between 1750 and 1900. It became increasingly secularized (though its religious roots remained important) and more closely tied to a belief in economic growth. Nisbet identifies two tendencies in its development during this period. The first, which began with Turgot and culminated in Spencer, tied progress to a liberal notion of freedom. Freedom, in the sense of the chance to pursue self-interest without external interference, was seen as necessary to progress; and progress was seen as culminating in freedom. The second tendency linked progress to the use of power on a large scale. Power was seen as necessary to transform human beings, unfold the logic of history, and realize the perfect society awaiting at the end of the road of progress. Nisbet identifies three main approaches within this second

tendency—the “utopian” (Saint-Simon, Comte, Marx), the “statist” (Fichte, Hegel), and the “racist” (Gobineau, Chamberlain).

The idea of progress has remained vibrant even into the twentieth century, Nisbet argues, but at the same time, there has been a growing loss of faith in it on the part of both intellectuals and some of the general public. Nisbet roots this loss of faith in progress in the decline of religion, the lack of a compelling belief in the sacred. Religious belief, Nisbet argues, is necessary for the sense of continuity and relatedness between past, present, and future that lies at the heart of the idea of progress. The decline of the idea of progress is also linked to the loss of five of its central premises: a belief in the value of the past, a conviction of the value of Western civilization, acceptance of the worth of economic growth, faith in reason and scientific knowledge, and belief in the intrinsic importance of life on this earth.

Nisbet thus concludes his book on a somber, even apocalyptic note: the idea of progress, which has always underpinned the achievements of Western civilization, seems to be dying. “If the idea of progress does die in the West,” Nisbet warns, “so will a great deal else that we have long cherished in this civilization.” Unless there is a rebirth of religion, Nisbet argues, our civilization is finished.

Nisbet’s account of the history of the idea of progress no doubt will be thoroughly dissected and evaluated by critics for some time to come. His analysis, to say the least, is often slippery. By the time he finishes his historical journey, his notion of progress contains so many elements that it can be applied almost arbitrarily. With a little determination, one can cull from almost any thinker some of these elements and then declare that he indeed has an idea of progress. Conversely, with a similar amount of determination, one can find some of these elements missing in almost any thinker and thus conclude that he lacks an idea of progress. So a belief in the cumulative growth of human knowledge qualifies the Greeks as having had an idea of progress, despite their frequent recourse to lost golden ages or to cyclical images of history. Conversely, Bacon and Descartes, whom Nisbet includes in the Renaissance, lacked an idea of progress, although they believed in the future progress of the human mind, because they regarded its past history as an undifferentiated mass of ignorance.

More importantly, the very complexity and malleability of Nisbet’s idea of progress raise questions about exactly what constitutes a decisive rejection of the idea and what is merely another modification. Nisbet is strikingly inconsistent here. In discussing its development, Nisbet stresses its flexibility by showing how elements have been continually added over the centuries. When he gets to the twentieth century, however, he suddenly transforms the idea of progress into a finished product, each of whose elements is so intertwined with the others that any challenge to one dooms the whole. To be against “economic growth,” therefore, is *ipso facto* to be anti-progress. What justification is there for this sudden rigidifying of the idea of progress? Why is it not susceptible to further modification? Imagine, for example, a possible society oriented not primarily to “economic growth,” but to cultivating personal satisfactions that do not require the consumption of commodities and to minimizing the amount of material resources needed to maintain a given standard of living. Such a society could require and encourage great advancements in both knowledge and the “moral” development of humanity. Would this society nevertheless have abandoned the idea of progress because it has dropped one of its elements—the belief in “economic growth?”

I am less concerned, however, with the validity of Nisbet's history of the idea of progress than with the intellectual and political purpose that seems to inform it. Nisbet attempts nothing less than a total redefinition of the political implications of the idea of progress by ridding it of its liberal, anti-traditional connotations and embedding it comfortably in a conservative worldview. He does this in at least three ways: First, he roots the idea of progress deeply in the heritage of the West. This idea thus appears to have grown organically with Western civilization itself rather than being the offspring of the raffish, anti-traditional Enlightenment. Second, Nisbet argues that religion has nurtured the idea of progress, thus severing it from the more corrosive implications of secularism. Third, he maintains that the idea of progress itself inherently contains an appreciation of the past and its continuity with present and future.

Progress thus appears as an eminently conservative notion, one that an eminently conservative thinker like Robert Nisbet can embrace without qualm or contradiction. The contrast with Nisbet's earlier work could not be greater. In *The Sociological Tradition*, Nisbet forcefully placed the idea of progress at a far remove from the conservative tradition—a tradition with which he closely identified himself. Progress, he argued, was an eighteenth-century Enlightenment idea, antithetical to the conservative worldview of Burke, Coleridge, Maistre, Bonald, and Chateaubriand that emerged in antagonism to the Enlightenment in the early nineteenth century. Conservatism in its very roots appeared as an antagonist not only of the idea of progress itself but of modern Western society and its claim to have bettered the human condition.

So there are two Robert Nisbets—both of whom claim to be conservative. The Old Nisbet of *The Sociological Tradition* (and of many articles before and since) professes a conservative creed that is critical of the idea of progress and of modern society. The New Nisbet of *History of the Idea of Progress* professes a conservative creed that embraces the idea of progress, affirms the basic directions of modern Western society, and hence is critical of the very conservatism propounded by the Old Nisbet.

To be sure, everyone has the right to change his worldview, even very radically. Nisbet's transformation, however, is unsatisfactory and ultimately does not work. To begin with, it is never acknowledged. Despite the clear difference between his previous work and *History of the Idea of Progress*, Nisbet nowhere seeks to reconcile the two or to justify the shift. Indeed, he makes no references whatever to *The Sociological Tradition*, its ideas of progress and of conservatism.

Nisbet's new outlook also requires that he pass over in embarrassing silence the very thinkers who occupied center stage in *The Sociological Tradition*. Burke *et al.*, with their very clear opposition to the idea of progress and to modern Western society, are difficult to assimilate into an argument that effectively stresses the traditional roots and conservative nature of the idea of progress. They thus receive nothing more than a few asides in the current work.

The most damning point against Nisbet's attempted transformation of both himself and conservatism, however, is that it blatantly fails when forced to come to grips with the contemporary Western world. Faced squarely with the issue of the viability of the idea of progress in modern Western society, Nisbet cannot decide whether to side with the idea of progress or with its critics. He seems torn on the issue of whether Western society has truly progressed in the last century and a half.



The last chapter of his current work, "Progress at Bay," is thus painful to read. It is a tortuous combination of New Nisbet and Old (and their respective conservatisms)—one minute condemning the critics of progress, the next minute taking up their argument. The chapter begins with a discussion of those nineteenth-century and early twentieth-century thinkers (the "early prophets") who provided the intellectual basis for the current repudiation of progress, among them Tocqueville, Burckhardt, Schopenhauer, Kierkegaard, Nietzsche, and Weber. If we follow the argument of *History of the Idea of Progress*, these figures are the arch-enemy, since they condemn the very idea that underlies Western civilization. Yet Nisbet seemingly cannot bring himself to disown them. He diligently repeats their case against modern Western society and then fails to offer even the mildest criticism of their arguments. Their points are simply too much his own as well.

Similarly with the issue of economic growth. A belief in the importance of economic growth, Nisbet has told us, is essential to the modern idea of progress, and he scores the critics of growth for undermining that idea and thus threatening Western civilization. When he comes to their specific arguments, however, Nisbet appears to waffle. Here is Nisbet's summary of E. J. Mishan's argument against growth:

What Mishan brings out in a series of arresting chapters is the profound disharmony that exists—must exist, he argues—between continued prosecution of economic progress and these constitutive moral and social values [family, stability, tradition, custom]. Mishan goes so far as to declare that the very legitimacy and thus the strength of our most vital institutions is being undermined by the central, mostly technological elements of economic growth. Unlimited continuation of the kind of industrial expansion we have known for two centuries in the West must result, Mishan concludes, in a slow but inexorable disintegration of the social order, a collapse of the disciplines and authorities which are built into any social order, and a casting of individuals into a social and moral void, one that is most likely to be filled by the totalitarian state. Mishan is convinced of the reality of historical decline. And the cause of this decline, Mishan observes, is the very economic growth that was so widely hailed in the late eighteenth and the nineteenth and early twentieth centuries as the mainspring of progress in general—social, moral, cultural.

Here surely is the enemy of the New Nisbet and his idea of progress, yet the reader waits in vain for a rebuttal. After carefully laying out the anti-progress argument, Nisbet fails to counter it. The reason for his failure seems clear. Are not Mishan's ideas those of the Old Nisbet as well? Is not this the theme that Nisbet has lovingly cultivated for years? Such themes cannot be so easily abandoned.

The same pattern continues throughout Nisbet's crucial last chapter. Nisbet condemns our loss of faith in progress, presents the arguments of the main critics of progress (arguments that he himself has so often made in the past). . . . and then consistently fails to make a rebuttal. At points the loss of faith appears to be justified by the course Western society has taken; at other points it appears to be the work of that intellectual counterculture that Nisbet says has been causing trouble since the Renaissance.

In short, *History of the Idea of Progress* ends in intellectual schizophrenia. The New Nisbet and the Old Nisbet take turns—the one singing the praises of progress, the other condemning it. Neither recognizes the existence of the other or the contradictions that their simultaneous presence creates. Robert Nisbet has confronted his intellectual enemies, but he fails to realize that they include himself.

If Robert Nisbet's contradictory and tortuous intellectual world were

idiosyncratic, it would not merit close scrutiny or extended attention. Nisbet, however, is hardly alone in his confusion: he reflects the dilemmas of conservative thought in general in the late twentieth century. Contemporary conservatism is an unholy and unstable combination of two fundamentally antithetical worldviews. The one is an Edmund Burke conservatism, which pictures society as a slowly developing, intricate, unconscious network of tradition, shared sentiments, and social ties and which is mistrustful of rationality, secularism, the market, capitalism, economic growth, and modern society in general. In its pristine form, it takes feudalism as its model of society. The other is an Adam Smith liberalism, which views society as nothing more than an aggregate of individuals, each pursuing self-interest and each tied to others only by instrumental, contractual ties. It takes the market as its prototypical image of society and favors untrammelled economic growth, rationality, and pursuit of self-interest.

In nineteenth-century Europe, these two worldviews remained quite separate from each other. Conservatives could cultivate rich images of a precapitalist past in which to root their outlook in pure form. Twentieth-century America is another story, however. As Louis Hartz has forcefully pointed out, the United States lacks a feudal past altogether; it is the archetypal bourgeois society. It thus provides poor soil for a pure Burkean conservatism. The would-be conservative thus must try to come to terms with capitalism and its liberal ideology to find some new basis for a conservative outlook. The result is a continual effort to be both Burkean conservative and Smithian liberal—to affirm both community and the market, tradition and scientific rationality, virtue and self-interest.

This synthesis has yet to work. Instead, to use Peter Steinfels' words in *The Neo-Conservatives*, the conservative mind has fallen victim to "a split personality . . . the conflict between its commitment, on the one hand, to a reflective cast of mind and traditional values, and, on the other hand, to the calculating rationality and furious destructive energy of modern capitalism." Robert Nisbet's *History of the Idea of Progress* is simply the latest manifestation of this split personality.

### **Naturalism and Social Science: A Post-Empiricist Philosophy of Science.**

By David Thomas. New York: Cambridge University Press, 1980. 213 pp. Cloth, \$32.50; paper, \$11.50.

Reviewer: ERNST NAGEL, Columbia University

The primary aim of this book is to show that the study of social phenomena can be pursued successfully in conformity with the methodological requirements of the natural sciences. Although Thomas does not explain just what constitutes such a "naturalistic" methodology, he does maintain that it is to be construed so that it satisfies the claims of what he calls a "post-empiricist" philosophy of science. Two fundamental ideas are borrowed from the latter. In the first place and in agreement with traditional empiricism, a necessary condition for accepting a scientific theory is that the theory is supported by empirical evidence; but Thomas also repeatedly stresses the point that every scientific theory is underdetermined by such evidence. Second, though in disagreement with traditional empiricism, the "holistic" structure of scientific theories must be recognized: that is, the meaning of a term

occurring in a theory cannot be specified by simply indicating the empirical data to which it supposedly refers, since what a term means depends in part on the term's relations to the other terms of the theory; and, in consequence, the truth of a statement in a theory cannot be established individually and in isolation from the other statements of the theory, so that a theory stands or falls as a whole.

A good fraction of the book is a critique of views dissenting from its main thesis. Thomas has read widely in the literature maintaining that the natural sciences are not a satisfactory model for the study of human affairs, and he offers many cogent rejoinders to the arguments of Althusser, Durkheim, Gramsci, Mannheim, and Winch, among other thinkers. He believes that social scientists must reckon with the "common sense," though changeable, conceptions that actors involved in social phenomena have of those occurrences; but he also makes clear that the theories eventually proposed by social scientists to explain those phenomena may ignore such conceptions, and may indeed be unintelligible (or "meaningless") to the actors. A long chapter is devoted to the much discussed question of the role of "meaning" in social science. Thomas distinguishes several senses of the word, some of which are distinctive to the social sciences, and he notes that the "understanding" that actors may have of the phenomena in which they participate provides a classification of social traits and happenings, but one which social scientists need not adopt in their own explanations of the phenomena. Moreover, he agrees with recent objections to drawing a sharp distinction between fact and value, and he maintains that social science theories are unavoidably "value-laden," in part because he believes that the congruence of such a theory with the value commitments of social scientists is one condition for their accepting the theory. But despite the concessions Thomas makes to proponents of non-naturalism in social science, he does not think these require the abandonment of naturalistic methodology in social study.

Thomas does not state the considerations that led him to question the fact-value distinction, and it is therefore impossible to assess the reasons for his doubts. On the other hand, he does mention the ways in which, according to post-empiricist philosophy of science, values enter into the social sciences: what social scientists select for study depends on their values; how they categorize and individuate facts is in part also determined by their values; what is counted as good evidence and sound method again depends on scientists' values; and what a scientist states in his explanations and conclusions is once more dependent on his values. In my opinion, all this is doubtless the case in some sense of the ambiguous term "value." But the sense of the word which makes these assertions obviously correct is the sense associated with standards of workmanship in conducting scientific research. I fail to see that these values or standards play a lesser role in the natural sciences than in the study of human affairs. Similarly, the claim that such study is inevitably value-laden because social scientific theory is underdetermined by the evidence for it at any given time—so that considerations other than those related to the weight of the evidence play a role in accepting the evidence—also applies to the natural sciences. Thomas is aware that such rejoinders can be made, but if he has an answer to them, I have been unable to find it in this book.

Thomas makes much of post-empiricist philosophy of science, and he writes as if the validity of his central thesis rests on that philosophy. However, I do not believe his book establishes this dependence, for nowhere in his defense of that

thesis does he make use of a tenet distinctive of that philosophy. To be sure, he does support his view that social science theory is value-laden by invoking the holistic assumption of post-empiricism, according to which value judgments are internally (or logically) related to cognitive elements, and cannot therefore be segregated from other statements of the theory. But his argument for the central thesis of the book makes no use of this assumption, and I must therefore conclude that his discussion of post-empiricist philosophy is an excursion that is irrelevant to his defense of that thesis. But however this may be, the idea that scientific theories have a holistic structure, and that in particular a theory stands or falls as a whole, is in my judgment untenable in the light of some historical facts. For example, the assumption in Darwin's evolutionary theory that new species arise as the outcome of small variations in the progeny of members of existing species is retained in neo-Darwinism, but his pangenetic notion of heredity is regarded as incorrect and is replaced by a quite different one. Similarly, as Herbert Simon has shown, the three major assumptions of George Homans' theoretical system expounded in *The Human Group* are logically independent and each can be replaced by some alternative assumption.

It is rather curious that, although Thomas espouses the holistic view of theories, he himself recognizes that it creates a serious difficulty: if the meanings of statements of a theory are determined (if only in part) by that theory, no rational cross-theoretical debate appears to be possible—although such debates have occurred frequently in the history of science. Thomas believes the difficulty is still unresolved and is a major problem for the philosophy of science. But he suggests that a "combined correspondence and coherence account of truth" might resolve the puzzle. It is, however, a suggestion that seems to me to be incoherent. In my opinion Thomas would be better advised to reconsider his commitment to holism.

**Research in Sociology of Knowledge, Sciences and Art: Volume 2.**

*Edited by Robert Alun Jones and Henrika Kuklick. Greenwich, Ct.: JAI Press, 1979. 247 pp. \$31.00*

*Reviewer. KURT LANG, State University of New York at Stony Brook*

The real *pièce de resistance* of this potpourri of essays is the lead piece by Lewis Feuer. It is part of a continuing debate over Merton's thesis that Protestant asceticism had the same causative influence on the development of modern science that Weber argued it had for industrial capitalism. Feuer launches a head-on attack on both the theory and the evidence to support it. Not asceticism but hedonistic fulfillment, the sheer joy of discovery, was and still is, he insists, the motivational base for scientific activity. He marshalls case after case to show that many of the carriers of scientific take-off in the seventeenth century were anything but highly devout, and that their life styles often clashed with predestinarian ascetic Puritanism.

Feuer's polemical essay is unlikely to end the debate. But its scholarly level raises expectations that, unfortunately, remain unfulfilled by the remainder of the volume. Many of the contributions are at best intellectual history, often with a focus on sociology, and at worst warmed-over discussions of work by some of the big

names and some that are not quite so well known. Several stake out a problem and attempt some analysis in their own terms. Among these the article on the functions of "intellectual rubbish" by Joseph Agassi should be mentioned. What is here called intellectual rubbish is, essentially, what usually goes under the name of "mediocre" culture, and the Agassi essay is a belabored, though not unreasonable, effort to link its persistence to the institutional system that supports the intellectual professions. The article by Vytautas Kavolis is a more ambitious attempt to do the same for the evolution of the artistic enterprise, but covers too much ground to be of much interest to the specialist. A case study of the concept of "race" in anthropology by Larry T. Reynolds, Leonard Lieberman, and Jerry A. Stark uses conventional, but highly simple methodology, to examine the relationship between a number of independent variables, like age and social background, and the predilection to be a "lumper," defining the concept of race inclusively, or to be a "splitter." One wishes the authors had gone more deeply into the subject.

The reader should not be misled. the European and literary influence is very much in evidence, not only in the essay by Feuer but in two discussions of Durkheim and French sociology of knowledge, which are then balanced by two others on modern versions of Marxist theory with its links to phenomenology and hermeneutics. There is also a long piece on Joseph Needham's comparative work on science in Chinese and modern society, a shorter one on the "prescriptive implications of historical self-consciousness in the social sciences," and finally one on the sociology of "Silas Marner," the chief work of Mary Ann Evans, better known as George Eliot. The last probably qualifies for inclusion on two counts: it extracts the sociological insights from a literary work and describes the author's contact with two eminent sociologists of her time, Comte and Spencer.

Probably the volume, for better or for worse, reflects a difficulty intrinsic to the sociology of knowledge, science, and art, where it is easy to confuse writing about theorists with the development of theoretical insights. Even if some of the contributors fell into this trap, they have at least tried.

### **The Sociology of Belief: Fallacy and Foundation.**

*By Keith Dixon. London & Boston: Routledge and Kegan Paul, 1980. 136 pp. Cloth, \$20.00, paper, \$10.00.*

*Reviewer: PATRICK H. MC NAMARA, University of New Mexico*

Keith Dixon's latest contribution to Routledge and Kegan Paul's "Monographs in Social Theory" series criticizes what he believes to be unwarranted reductionism in many applications of the sociology of knowledge. A second, closely related thesis is "that in explaining or understanding beliefs, judgments upon their epistemological basis cannot be evaded." He thus challenges social scientists to take more seriously participants' own accounts of their activities and beliefs and to review critically the rationales which justify participants' discourse in a social milieu "as a distinctive mode of human understanding and experience." I find the author more convincing in developing the first of these two challenges.

Dixon's opening chapter lucidly expounds and criticizes theories of social location proposed by Marx and by Mannheim. Marxian dialectic, while stressing

human agency in the construction of social worlds, attempts to "validate itself" as a theory partially by recourse to assessing the theorist's active life (praxis), group memberships, and class position. Empirical testing in a conventional sense is thus rendered difficult if not impossible. Mannheim's "dual residentialism"—identification of knowledge with consensus of a social group or a generation—makes "the role of compelling evidence, the role of individual intelligence" subject to changes in the social situation of the agents in question. Ideational change as a relatively independent phenomenon is thus disallowed.

Chapter 2 explores various reactions to the reductionism Dixon deplores. Ethnomethodology seems, at first blush, to be especially sensitive to participant accounts and understandings and to eschew interpretation by social location, but we are asked to become "'judgmental dopes' on the crucial issues of what is to count as genuine knowledge or what is of human worth." Cultural relativism comes under attack as an explanatory mode because it denies principles of rationality which transcend specific cultural categories; the scientist's framework yields under cultural relativism to the "private language" of the inhabitants under study.

In "The Sociology of Science" (Chapter 3), Dixon opens with a detailed review of the critical reception accorded Thomas Kuhn's work on scientific paradigms. He takes to task those sociologists of science who view scientific disputes and paradigm shifts only or mainly in terms of the scientists' social locations. The polemical exchanges of Karl Popper and A. J. Ayer serve to highlight the wisdom of ascribing validity to knowledge even when unaccompanied by absolute certainty; furthermore, Dixon opts for a "modified empiricism" which "makes the strong claim that there are no *prima facie* reasons for doubting, as a general rule, the evidence of our senses."

Chapter 4, "Ethics and Religion: Claims to Autonomy," defends the validity of moral discourse, asking social scientists to take seriously a language which allows "the permanent possibility of critical assessment of social rules." Moral discourse has its own autonomy, then, and need not be reduced to conventional acceptance of social norms nor, worse yet, to reflection of social and economic interests. Religion comes under special consideration, particularly through the views of Ninian Smart and D. Z. Phillips who approach religion respectfully and reject reductionist interpretations of religious meaning in both individual and collective contexts. Smart, in particular (and to a certain extent, Peter Berger) advocates "bracketing" epistemological and ontological assumptions concerning the validity of religious belief systems. (Dixon, in common with many social scientists, insists that what is *non*-bracketable is an investigator's assumption that given religious worldviews are worth studying.) Epistemological bracketing allegedly enables the investigator to examine belief systems phenomenologically and in their relationships with other sociocultural factors without making value judgments about them.

Dixon believes this procedure wrongheaded and reiterates his central contention: to bracket epistemological questions concerning the so-called "objective" grounds for belief is to impale oneself on the horns of a dilemma: either (a) one simply sets forth participants' accounts of their beliefs, etc., thus forfeiting "any explanation or justification of beliefs"; or (b) all belief systems are seen as "determined" by social and psychological factors. No social scientist should find either of these acceptable.

Surely the dilemma is escapable. Does it reduce the social scientist's explanatory power to forego making philosophical judgments concerning the "rationality" of a belief/worship system or to refrain from speculating about its "justifiability?" I think not. In fact, to do so would be to cross disciplinary boundaries into philosophical territory. I believe that most contemporary sociologists of religion see no reason to question or to speculate about the reasonableness or "sense" of a given belief system under study, contenting themselves with attempting to clarify, in the traditions of Durkheim and Weber, the relations of religious beliefs and practices to social processes. Guy Swanson's work (with which Dixon is familiar) illustrates the possibility of making such correlations on a macro-societal and cross-cultural level without the necessity of passing judgments epistemologically on the belief systems one studies. Sociologists of religion generally seek to uncover the relationship of religiosity in any one or more of its several dimensions (e.g., the five postulated in the work of Charles Glock) to familiar sociological variables such as sex, age, education, social class, etc. To proceed in this fashion is not yielding to cultural relativism; it is to explain what sociology can best explain, taking religion as it is expressed and lived by a given social group. Dixon is certainly correct in warning against explaining belief systems in terms of social location and in urging all of us to take seriously participants' moral discourse, but I think he asks more of social science than it can properly deliver in requiring it "to make an assessment of [a belief system's] rationality in an intellectual sense."

With the above caveat, I believe Dixon has written an excellent book. It should be most useful in theory courses, providing what is surely one of the best and most succinct critical accounts of varying applications of the sociology of knowledge. I must add my regret that the price for this slim volume is so outrageously high.

**The Religious Dimension: New Directions in Quantitative Research.**

*Edited by Robert Wuthnow. New York: Academic Press, 1979. 376 pp. \$23.00.*

*Reviewer. HART M. NELSEN, Catholic University*

This edited volume does indeed reflect advances in quantitative research in the sociology of religion. The volume is not intended to serve as a text or to give a holistic view of research in religion. Even so, I found some of the articles to be sparse in reviewing appropriate literature, including the studies that no doubt generated the present work. In spite of this one criticism, I believe this to be a very important collection of research articles.

The single best article—certainly an exception to the criticism just made—is Clark Roof's systematic review of the indicators of religious commitment. The measures reviewed concern church-based religiosity, civil religious commitment, ultimate concern, and alternate meaning systems addressing how people define and order their reality. Roof then assesses religious measurement, noting, for example, that "religiosity may be defined and/or measured as either a highly generalized or as a very precise and diversely structured phenomenon, depending on the uses to which it is put." This is an important conclusion because, not that

long ago, arguments about unidimensionality versus multidimensionality were of paramount importance. Roof does note that there still exists a need "for exploring subdimensional components of the core dimensions (i.e., belief, participation, and experience)" and that researchers should move beyond description to specifying the "logical and causal connections among [the] dimensions."

A volume such as this should tease readers with possible new directions for research as well as familiarizing them with what has been accomplished, which the chapter by Roof so admirably does. Glock and Wuthnow who report the nominally religious (43%), the nonreligious (11%), the alternatively religious (8%), and the religiously loyal (34%) interviewed in the San Francisco Bay Area, also do this. An article by Wuthnow in the *American Sociological Review* (1976) reported evidence of a generational shift in subscribing to traditional religiosity, with those under thirty displaying low levels of attendance, for example. Given that article and the "unchurched American" study (the descriptive data are reported by Gallup and more scholarly analyses are reported by Hoge, Roozen, Carroll, and others), which indicate that those in the West are likely to be unchurched, California becomes the bellwether for change.

Now what do the data suggest? The differences between those under thirty and those thirty and older do not suggest such generational differences; only 3 percent of each group identified themselves with a non-Western religion. Life-after-death remains "a matter of concern for most people." The authors' conclusion is a balanced one "given the tenacity of commitment to conventional religion on the part of a still large proportion of the Bay Area population . . . it seems wise to be cautious about predicting the early or even the eventual demise of conventional religion." Glock and Wuthnow would have us focus not on those who have turned to alternative religions, but on the nominally religious: "They are now the largest group in the Bay Area and, we suspect, in the nation as a whole. The direction they take, consequently, is likely to determine which of various possible scenarios for the future of religion works out."

A chapter by Thomas Piazza and Charles Glock deals with people's images of God and their social meanings. Though the literature is not reviewed, the article is stimulating. Piazza and Glock use two items—whether God influences one's life and whether God ordains abilities "so that the work of the world will get done." Using these gives four combinations, including those who endorse God as influencing both the social order and their own lives—"this image of God . . . is one that is often associated with fundamentalistic Christianity." The typology is then related to political orientations and issues. The results suggest how conservatism in religious matters (God as influencing personal lives, but not ordaining the social order) can be linked with a liberal outlook on social issues.

There are other chapters—some excellent, some adequate—which are of less general interest: Welch's superb quantitative approach to classifying sects (with a thorough literature review); Greeley's "Ethnic Variations in Religious Commitment," which is both interesting and valuable for its description; Sasaki's relating of status inconsistency and religious commitment (but less than five percent of the variation in religious commitment is explained by status inconsistency).

The piece by Gary Jensen and Maynard Erickson on religiosity and delinquency shows that "religious beliefs and participation explain as much or more variance in drug use than participation in many other types of secular activities."



The effect of religion here varies by denomination. Furthermore, there are special settings where religion makes a difference for delinquency involvement. In his introduction Wuthnow observes the need for "more careful contextual specification of the relationship between religion and other variables." This is the special strength of the Jensen and Erickson article.

Two articles describe social trends. James McCarthy addresses the relation between religious identification and marriage dissolution, noting the convergence of Catholic-Protestant differences. Charles Westoff examines reproductive behavior: "Catholic contraceptive behavior has lost almost all of its distinctiveness; by 1980, there will be nothing to differentiate Catholics from non-Catholics in this area." Attitudes toward abortion have become more approving from 1965 to 1975, with "the average Catholic attitude" in 1975 resembling the Protestant attitude five or ten years earlier.

There are other articles. Riccio's examination of religious affiliation and SES seems dated—he does not review Greeley's more recent work. Blackwood examines commitment to the work ethic and finds "little religious basis for the work ethic and a corresponding insignificant role for religion in effecting change in commitment to the work ethic over time." Other articles consider the impact of migration on church attendance (Wuthnow and Christiano); Watergate and confidence in American institutions (Bergesen and Warr); black American music and social solidarity (Bergesen), and revival religion and individualism in late nineteenth-century America (Thomas). There are also two cross-cultural, empirical studies by Simpson and by Hammond and Williams.

This is a heterogeneous volume, but its quality is of a high level. The Roof piece has the most value generally. Other pieces are suggestive for future research. The book has a lot to offer—it reviews material (some articles are better than others here), it presents original analyses of data new and old, and it points to new directions for research. It is a must for students in the sociology of religion.

### **The Cultural Critics: From Matthew Arnold to Raymond Williams.**

By Lesley Johnson. London & Boston: Routledge & Kegan Paul, 1979. 235 pp. \$21.95

Reviewer: JEFFREY C. GOLDFARB, *New School for Social Research*

This book reminds us of the degree to which our ideas about culture are based on romantic, rather conservative, reactions against industrial capitalism and mass society. In recent years, it is Raymond Williams who has made most explicit the relationship between culture and society. In doing this in *Culture and Society, 1780–1950* (1958), he wrote a modern classic. Lesley Johnson reworks Williams' material and consciously has chosen to organize her exposition in his manner. Her stated goals for doing this are to make clear her indebtedness to Williams, and to elucidate how she differs in her approach. Both goals are achieved, but significant advancement of scholarship is not.

The bulk of the book traces the development of a distinctive English tradition of cultural literary criticism. Matthew Arnold, F. R. Leavis, and Raymond Williams are presented as the stellar figures in the tradition, chiefly responsible for the development of moral criticism and a projective vision of social change based on the

concept of culture. An explanation of the major ideas of each is wrapped around their theories of state, culture, and education. This explication is given a sociological and psychological foundation through references to the authors' biographies and social and political milieux. A substantial and impressive supporting cast is presented. J. S. Mill, Herbert Spencer, T. H. Huxley, John Ruskin, William Morris, George Bernard Shaw, H. G. Wells, C. F. G. Masterman, Clive Bell and the "Bloomsbury Group," D. H. Lawrence, T. S. Eliot, Bertrand Russell, Christopher Caudwell, George Orwell, R. H. Tawney, Richard Hoggart, Richard Wollheim, G. H. Bantock, and R. S. Peters, along with a brief report on English cultural studies in the 1970s, particularly work associated with the Center for Contemporary Cultural Studies at the University of Birmingham.

The biographies, historical sketches, and explications are competent and informative, but the critical evaluations of the work of those who make up the tradition of English cultural critics are significantly flawed. They are flawed not out of wrong-headedness, but from weakness of argumentation.

Johnson adopts the vantage point of the studies conducted at the Center for Contemporary Studies, self-consciously "anti-humanist" in approach, based on the structural Marxism of Louis Althusser. I personally find this approach overly theoretical, often obtuse, and—though elegant in the hands of its most accomplished practitioners—potentially laying the groundwork for a highly sophisticated Stalinism. Be that as it may, in Johnson's hands the anti-humanist structural approach leads to confusion. In order to support her evaluations, she cites the existence of underlying socioeconomic structures (i.e., basic structures), but fails to elucidate how such structures operate in cultural, social, or political life.

In evaluating F. R. Leavis' mode of literary criticism, for example, Johnson asserts:

The threat to the social and cultural preserve of the literary intellectual was represented by Leavis and his followers as existing only at this cultural level. But these developments were only part of more fundamental transitions in English society. . . . The pressures of the mass market and the expectations of professionalism and specialization articulated at the cultural level the fundamental changes in the material base of society.

This is a cogent critique of Lewis' criticism. It calls for an analysis of the changing nature of the "material base," and the dynamics of its "articulation" to the "cultural level." Unfortunately this is not forthcoming; instead, we get argument by assertion and citations.

In properly criticizing Q. D. Lewis' conception of past and present, she maintains that his "claim for the integration of all people and the existence of a shared cultural life in Elizabethan times can be effectively demolished by reference to detailed studies of the activities and pastimes of different groups in society," but she does not proceed to demolish the conception or cite extensive literature which would provide the basis for such a task. Only a single journal article is cited. Not much better than the scant evidence Lewis uses to make the assertion in the first place.

While such argumentation may not be of the highest quality, in that the historical study probably would support Johnson's judgment, the problem here is not nearly as serious as when she negatively appraises the value of T. S. Eliot's belief in proper cultural hierarchy and its fundamental basis in family background by asserting that T. B. Bottomore "demolishes" Eliot's ideas. In fact, Bottomore only presents, in his primer *Elites and Society* (1964), an alternative scheme for cultural achievement based on equalitarian values and notes that cultural privilege is passed

on by privileged families through extra-family institutions. This is far from demolishing Eliot's argument, however reprehensible from a moral viewpoint that argument may be.

Not only are arguments not as easily demolished as Johnson asserts, sociological investigation is more complex than she seems to be aware. She only suggests an overall approach to culture and the cultural critics, never making a systematic approach explicit, this despite the reference to systematic works.

In pointing to the "mistakes" of Williams' cultural analysis, she maintains that he obscures the reality of culture as a concept as "part of an ideological language," and fails to show, at least in his early works, how such language is embedded in political struggle. Here one would expect a systematic analysis of her frequent identification of culture and ideology. Yet the most significant flaw in this work is that Johnson never explains the degree and nature of culture's embeddedness in political struggle. She proceeds as if the cultural critics are simply different sorts of ideologues and cultural products different ideologies, but then ends the work with an unpredictable praise of cultural critique and its continuity. If her view of the relationship between ideology and culture were adequately explained, her conclusion might have followed from her analysis.

### **Sociological Dilemmas: Toward a Dialectic Paradigm.**

By Piotr Sztompka. New York. Academic Press, 1979. 362 pp. \$19.50.

Reviewer: ROBERT J. ANTONIO, *University of Kansas*

Piotr Sztompka asserts that sociology is in a state of crisis deriving from antinomies in its fundamental ontological, epistemological, and methodological assumptions. Reflecting this situation are six dilemmas (naturalism/antinaturalism, reductionism/antireductionism, cognitivism/activism, neutralism/axiologism, passivism/autonomism, collectivism/individualism) of mutually exclusive, contradictory responses to sociology's central metatheoretical questions. The discipline is polarized because sociologists adopt divergent patterns of these responses, undermining efforts to establish the common metatheoretical foundation needed to direct theory and research consistently.

Sztompka eschews nihilism and relativism, arguing that a "unified paradigm" lies beneath the dissensus. His "dialectical critique" negates invalid meta-assumptions, retains valid ones, and culminates in a new paradigm. The six dilemmas are analyzed in successive chapters, each of which provides substantive examples of the dilemmas in classical and contemporary sociology, describes a common set of meta-assumptions underlying each dilemma's polar responses, demonstrates that these meta-assumptions are fallacious, poses a "dialectical solution" for each dilemma, and explains how each solution was foreshadowed in Marx's work. Sztompka stresses that Marxism is the only existing approach in which the unified paradigm appears in coherent (although incomplete) form.

Sztompka's major contribution is his systematic analysis of the implicit metatheoretical dimensions of sociology. He clarifies critically the invisible hand behind many sociological controversies. Furthermore, Sztompka is a Polish Marxist who has American training and an appreciation of American sociology. His portrayal of Marxism as a superior metatheoretical foundation does not imply a rejection of conventional sociological theory and methods. Instead, he emphasizes

the continuities between the traditions and implies that they could be fused together fruitfully. (This theme is consistent with Sztompka's earlier *System and Function* [1974], which elaborates continuities between functionalism and Marxism.) This is an innovative approach, since American Marxists often dismiss conventional sociology as ideology.

The complexity of Sztompka's problematic is too great to be unraveled fully in one book. As a result, his generally well-conceptualized abstractions are not tied determinately enough to sociological practice. The discussions of the manifestations of the dilemmas in specific sociological theories and traditions are somewhat oversimplified. Also, he relies too heavily upon secondary sources, occasionally uses controversial texts uncritically (e.g., Blumer on Mead, Ollman on Marx) and sometimes cites textbooks as authoritative sources (e.g., Martindale, Timasheff and Theodorson, Cuzzort). The brief descriptions of the exemplars are often based on a few passages that support the author's classification (as reductionist, activist, neutralist, etc.) of the theories. Subtleties contradicting the portrayal are not entertained. Also, Sztompka does not explain in enough detail how his "dialectical solutions" will provide a more rational basis for concrete sociological practice. An unsympathetic view of the work could argue that the author presents straw men and abstract solutions to sociology's problems.

Sztompka's presentation of Marx is generally quite sound, although it ignores some problematic elements. For example, Sztompka explains that Marx overcomes the fact/value conundrum by demonstrating that the proletariat is the universal social class. By "adopting the position of the proletariat," sociologists commit themselves to rational change without biasing their findings. However, proletarian interests are open to debate empirically, theoretically, and valuatively. In fact, some Marxists argue that this position has often been abused ideologically. Bureaucratic elites use claims to gnostic understanding of "real" proletarian interests to justify action against workers. (Ironically, this problem has been manifested in the recent struggles between Polish workers and the Communist party.) In his discussion of the solution to the individualism/collectivism dilemma, Sztompka cites favorably Marx's treatment of individuals as "personifications of economic categories, embodiments of particular class-relations and class-interests." Some scholars argue that this theme contradicts Marx's emphasis on *concrete* individuals and provides justification for elites who sacrifice "real men" on the basis of the alleged "interests of the proletariat." Sztompka's presentation of Marx would have been stronger if he had at least hinted at such problematic implications instead of weaving the issues uncritically into his otherwise competent analysis.

A brief comment should be made about style. Sztompka's writing is systematic and clear, but quite monotonous. The architectonic mode of argument repeated in chapters on each of the six dilemmas is quite exhausting to the reader. The book also exhibits numerous and disturbing typographical errors.

Despite some weaknesses, this is a useful and challenging work. Sztompka defines an extremely important intellectual problem in a sophisticated fashion. His description and critique of the assumptive foundation of sociology is, for the most part, an excellent hermeneutic effort. By sensitizing the reader to usually neglected, yet pervasive metatheoretical influences, he makes the process of doing sociology more self-conscious and critical.

This work should sensitize American sociologists to the fact that Eastern

Europeans are doing interesting and significant sociology. The Poles, who are often well-versed about American intellectual developments, are particularly concerned with establishing bridges between Marxism and conventional sociology. Sztompka's work opens a rational dialogue between Marxism and sociology, emphasizing that the two are not mutually exclusive enterprises. Conventional, as well as radical, American sociologists might find their approaches enriched if they heeded this lesson.

### **An Alternative Sociology.**

By Franco Ferrarotti. New York. Wiley, 1979. 200 pp. \$14.50

Reviewer. ANDREA FONTANA, University of Nevada, Las Vegas

*An Alternative Sociology* is not really an alternative. Instead, it presents one more Marxist critique of American sociology. Ferrarotti's book is an attack on what he perceives to be the whole of American sociology, that is, Parsonsian structural-functionalism. The book begins with a discussion of Gramsci and moves on to analyze class relations and work conditions. It then attacks Taylorism and the general field of the sociology of work for their alleged services to the "ruling" class. Ferrarotti also turns his critical analysis on family life and consumption patterns among various classes. He ends with a brief discussion of power.

While attempting to provide a global picture of the elements that influence the worker, Ferrarotti succeeds only in rambling on in a confused and confusing manner. Ferrarotti is biased and often poorly informed about American sociology. No doubt he would consider all American sociologists as mercenary vassals to the establishment, motivated by profit to perpetuate a "false ideology." However, there is more to American sociology than Parsons, Gouldner, and Bell, important as they may be. And Max Weber cannot be simply dismissed, as *An Alternative Sociology* does, by superficially referencing him to his Parsonsian interpretation. What about, for instance, Alfred Schutz's seminal discussion of Weber in *The Phenomenology of the Social World* and the whole gamut of Weberian usage in various sociologies of everyday life?

Ferrarotti advocates an alliance of sociology with the working classes. Society, for Ferrarotti, ought to be the focus of *empirical* inquiry, not sociology itself. *An Alternative Sociology* also encourages a critique of critical sociology itself, lest it become a reified discipline. Ironically, while advocating empiricism and reflexivity, Ferrarotti himself is remarkably unempirical in his critical analysis and clearly dogmatic in his theoretical stance.

*An Alternative Sociology* may have been seen as a valuable book in Italy, where it was published in 1975, given the general emphasis on critical sociology in that country. But in the United States, given the general emphasis on the very professional sociology that Ferrarotti so vehemently attacks, this book will generate little interest. Even among critical sociologists the book is unlikely to cause a stir, for it adds little to the field.

Very likely this review will be dismissed as motivated by "false ideology," for it is written by an American professional sociologist, self-admittedly steeped in Weberian notions of value-neutrality, objectivity, and understanding. Yet it seems

to me that, viewed from any perspective, *An Alternative Sociology* is an old story that has been told before.

**Life Chances: Approaches to Social and Political Theory.**

By Ralf Dahrendorf. Chicago: University of Chicago Press, 1980. 181 pp. \$15.00.

Reviewer: GUENTHER ROTH, *University of Washington*

Sociology has fallen on hard days. Gone are the dreams and realities of the growth economy and of academic expansion. There are two ways for sociologists to maintain or expand their "life chances": to turn themselves resolutely into useful technicians measuring the decline or scarcity of resources and services, or to think harder about "modernity in eclipse"—the probable title of Dahrendorf's forthcoming analysis of what is going wrong in Western societies. For the present we are offered a kind of rehearsal on the theoretical level.

Dahrendorf established his reputation as one of the leading social theorists in 1959 with *Class and Class Conflict in Industrial Society*. He was the first to offer "conflict sociology" as a theoretical alternative to Parsons' harmonizing social system approach, although sympathizing at the same time with Merton's formalist sociology. He also made a quick international career as an "idea broker" between the United States and Europe, interpreting one to the other in *Applied Enlightenment: Society and Sociology in America* (published in German in 1963) and *Society and Democracy in Germany* (published in English in 1969). In the 1960s he helped found the University of Constance as a new kind of university based on American and European models, a project that has become precarious. At the same time, he entered politics and held high elective and appointive office as a member of the liberal FDP in West Germany. His political career was crowned when he became one of the "Seven Wise Men" of Europe, serving as EEC commissioner in Brussels from 1970 to 1974. With the exception of Thomas Masaryk, who abandoned a professorship to become president of Czechoslovakia, no Western sociologist has held such high office. Since 1974 Dahrendorf has been director of the London School of Economics, his alma mater.

Dahrendorf's political experience seems to have sharpened his acute sense for the gap between theory and practice. He sees no easy mediation, warning of the *trahison des clercs* who look for shortcuts on the road from ultimate meaning to concrete action. Now, however, he feels compelled to leave behind the "curiously formalistic" (his words) quality of his earlier theorizing, which twenty years ago was the expected way to make a career, and face more clearly and openly the normative dimension of social theory. Therefore he offers a "self-correction" in the context of a discussion of Hegel, Marx, and Weber, and a friendly critique of Merton. In his quest for a mode of substantive theorizing, he chooses the loose essay form of the "eighteenth-century style think piece." For him "the time of comprehensive treatises is over. . . . Large subjects are best approached in small steps."

The seven essays assembled here, three of which originated as lectures and most of the others as abandoned drafts for the forthcoming big study, declare themselves greatly indebted to Karl Popper and his critical rationalism and practice,

a kind of "piecemeal social theorizing." Dahrendorf searches for a substantive concept that can give direction to social theory and to the future and offers a self-conscious theoretical solution: we can make sense of history by pursuing life chances that are a "specific combination of options and ligatures, of choices and linkages." Life chances are "the sum total of opportunities offered to the individual by his society"; they are not attributes of individuals, but qualities of societies. Dahrendorf wants to go beyond the current literature on empirical indicators of welfare and the quality of life and advance a substantive concept at "the busy crossroads of theoretical social science, philosophy of history, and political theory."

Dahrendorf opens his round of essays with a chapter on the meaning of history and the possibility of progress. Following Kant and Popper he rejects all historicist interpretations from Hegel and Marx to Habermas and pleads for the openness of history. He adopts a "regulatory" instead of a "constituent" concept of progress and insists on liberty as a maxim of action rather than a state of affairs. Crucial for Dahrendorf's perspective is the suspicion that "an optimal balance of options and ligatures . . . may have been upset in the contemporary world." We must fight for "a liberal society that is not libertine, a structure of authority that is not authoritarian, a social order that is not informed by law-and-order hysteria, thus a world in which choices are more than an invitation for endless *actes gratuites*." Dahrendorf insists that championing such a goal does not make him either a neo-conservative or a romantic, and sets himself off from the trend reversers, system haters, and ecologists.

As a troubled liberal and confirmed critical rationalist, Dahrendorf is as dissatisfied with the ahistorical aspect of older liberalism as he is with the "social-democratic consensus," the ideology of the growth economy and the welfare state, that today prevails in the OECD countries irrespective of party labels (although Great Britain and the U.S. have been veering away from it lately). Some goals and means of the growth economy must be replaced if Western societies are to have a future.

Dahrendorf opposes, in particular, the trend toward "equal lives" and advances a case for inequality as a necessary condition of liberty and expanding life chances. Social and political democratization threaten to overreach themselves. Although he was in the forefront of "education as a civil right" in the 1960s, he now fears the resulting diminution of the quality of education. Instead of "democracy in America" as an ideal, he suggests something like "opportunity and initiative in America," since "today hope springs from difference rather than sameness, and liberty from inequality rather than equality."

Who can best promote life chances? Politicians and intellectuals have a special historical responsibility. In his concluding chapter Dahrendorf therefore contrasts the "representative" activities of the producers of ideas and of art, who create a reservoir of possible futures, with the "legitimative" activities of the politicians, who must choose among the possibilities provided by this reservoir.

Dahrendorf takes pains to stress the tentative nature of many of his formulations and tries to anticipate and deflect many possible objections. We must now wait for his big study to see more clearly the path he wants to pursue and the possibilities he wants to offer us.

**The Sociology of Welfare: Social Policy, Stratification, and Political Order.**

By Graham Room. New York: St. Martin's Press, 1980. 275 pp. \$27.50

Reviewer: KIRSTEN A. GRØNBJERG, Loyola University of Chicago

In spite of several shortcomings, this book is a valuable contribution to the sociology of welfare. Although most of the empirical references concern a limited number of British social policies, the theoretical discussion is macrosociological in scope. Room provides a cogent review of the numerous arguments about the character of the welfare state, its underlying principles, and the outcomes of social policy.

He argues that there are basically three major perspectives on social policies in the modern welfare state: the liberal, the neo-Marxist, and the social democratic. The three perspectives are closely related in a historical sense and the first two in particular have been shaped by mutual criticism and share some basic conceptualizations about the nature of society: market relations form the basis for the division of labor and access to life chances, the basic mechanism of social change is capital accumulation, the role of the state is negative and peripheral, and the nature of social policies have not changed significantly since the early part of the nineteenth century. Within this framework, there are of course sharp differences between the two perspectives. To neo-Marxists, the cash-nexus is still of overriding importance, and social policies are seen as ineffectual or oppressive of the working class, while creating false consciousness and legitimacy for the capitalist system. The liberal perspective presents an optimistic assessment of the market system and its ability to create increasing equality, making social policies unnecessary except to provide a minimum guarantee of income as the agricultural society is transformed into an industrial one.

Room argues that the social democratic perspective views social policies as now fundamentally different from the early nineteenth century, when the 1834 poor law served the laissez-faire economy. The difference lies in the recognition that civil rights, the rights to opportunity and non-interference, are fundamentally different from social rights, the guaranteed claim to certain life chances or outcomes. This recognition places central responsibility on the state to insure that social policies reflect such social rights as are expressed through the political democratic system.

The second half of the book examines how these three perspectives hold up against a review of recent social policies. Chapter 4 examines the development of social policies: social security, reorganization of the National Health Service, unification of personal services, housing policy, and educational policies. Room concludes that these social policies do not support the neo-Marxist perspective that social policies are oriented to the interests of the powerful, nor do they reflect societal demands, as liberals would argue. Instead, they reflect a collective learning process somewhat along the lines of the social democratic perspective.

In Chapter 5, Room examines the social distribution of morbidity, medical care, and deprivation, and concludes that social class no longer determines access to services, but is still related to outcomes of services, although not in the narrow sense of the neo-Marxist or liberal views. Chapter 6 reviews three bases of social services—selective (means-tested), universal, and positive discriminatory



(compensatory)—in terms of their roles in developing social integration. Again, the liberal and neo-Marxist perspectives are found most wanting.

The major strength of the volume is not to be found in these empirical examples. The reviews of these social policies raise more questions than they answer, and the discussion of the three perspectives seems at times superficial and forced, especially as regards the position of the neo-Marxists. Room is clearly sympathetic to the social democratic perspective, especially as developed by Titmus and T. H. Marshall. I am not fully familiar with the British literature reviewed in this volume, but in a couple of cases Room seems curiously off the mark in his characterization of some of the American literature. He does not use Piven and Cloward's *Regulating the Poor* nearly as effectively as he could, and he misclassifies Janowitz's *Social Control and the Welfare State* as a liberal treatise, not as a social democratic one.

The book does, nevertheless, provide a close examination of social policies in the welfare state. Although one may disagree with the specifics of Room's characterization of his three perspectives, his discussion is useful and clarifying in a field of writing that can only be characterized as extensive and complex, if not actually muddy. Unfortunately, at \$27.50 (with no tables, graphs, figures, or even footnotes at the bottom of the page), *The Sociology of Welfare* is quite unlikely to be able to deliver on its promise to be a contribution to the field.

### **Inequality in an Age of Decline.**

By Paul Blumberg. New York: Oxford University Press, 1980. 290 pp. \$15.95.

Reviewer: NANCY ESTEB, University of Notre Dame

This is a book which challenges the reader to take a serious look at the social and political implications of continued inflation and the end of rising affluence in the United States. Blumberg's thesis is that American domestic and international economic and political power have declined since the late 1960s due to the growing noncompetitiveness of much of American industry, the aging of our infrastructure, a permanent war economy, selling advanced technology abroad, migration of United States multinationals to more profitable locations abroad, energy chaos, balance of payments problems, high unemployment, and especially inflation. Inflation has had its effect on living standards for everyone, but especially for the poor, because necessities, which make up a greater share of their household budgets than they do for those with higher incomes, have been hit harder by inflation than have other goods and services. As a result, instead of seeing support for two theories of class structure dominant in postwar America—class convergence and class stability—he sees support for theories of class stagnation and class divergence. In addition, stagnating living standards and growing inequality are showing signs of contributing to increased domestic social conflict and foreign policy belligerence.

There are two issues here: Has there been continued affluence since World War II, or is there now stagnation? And have the classes converged, stabilized, or diverged? Blumberg's argument is much stronger on the first issue than on the second. To substantiate his claim that since the late 1960s incomes in the United

States have stopped growing as they did in the years 1947–65, Blumberg has ample statistics from the federal government. His focus is more on the rate of decline than on the absolute position, and few could argue with that rate of decline.

However, the argument that the class structure is no longer converging or stabilizing, as earlier theorists argued, but rather that it is now diverging, rests on shakier ground. It rests on standards of living rather than on income inequality or other measures of opportunity or achievement. He admits that "the distribution of income has changed little if at all in the postwar period. . . ." In addition, he agrees that the proportion of the population below a poverty line of one-half the median income has changed little. Since neither income distribution nor poverty percentages can support his argument of divergence, he is left with standards of living. His task becomes that of showing that inflation has hurt the bottom of the income distribution more than the top. To do that he argues that low income households spend a greater proportion of their incomes on necessities than do higher income households. Then he must show that the goods and services which make up these necessities (food, medical care, housing) have been hit harder by inflation than have the prices for other goods and services. The conclusion is then that since low income families must spend more of their income on the rising costs of necessities, they have less discretionary income, and so their standard of living is reduced, whereas that of higher income groups is not, because inflationary necessities make up a smaller percentage of their household budgets.

This is a limited view of what we mean by inequality, and not in line with standard definitions of the concept, which typically rest on income distribution or access to opportunities. To claim that he is examining the impact of America's economic decline on class structure requires a broader view of that structure. Although his argument that there is stagnation is well supported, his argument that there is class divergence is not convincing.

The unit of analysis here is the household, a choice which, feminists argue, tends to make women invisible. In this book, they are not invisible, but their inclusion and the effects of their increased participation in the economic system are less than a feminist would hope for. Blumberg offers the following observations about women: the female labor force is predominantly in white collar occupations while the male labor force is in blue collar; both sexes have contributed to the growth of professional occupations; women's influx into the ranks of middle-class salaried workers has contributed to the loss of economic advantage for these workers; female earnings are lower than male earnings; BLS data showing income stagnation are not due to increasing numbers of low-paid women workers; female workers are most common in services; increasing labor force participation by wives is due primarily to insufficiency of male incomes for household needs and such female participation has raised the median income of these families; divorced males' and females' low incomes are related to the disintegration of American families; the percentage of poor families headed by females has increased; multinationals in Third World nations exploit female laborers; home buying now requires a permanently working wife, with implications for labor force participation and family size; and America's decline will increase sexual economic conflicts.

A fuller discussion of women's increasing labor force participation in the United States in recent years would have provided a fuller picture of the impact of a shrinking economic pie. For example, from which social class groups do most

working women come? What occupations are open to them and which ones do they enter? What are the impacts on the social class position of their families?

The classroom usefulness of this book lies in its potential to spark discussion in upper-division undergraduate classes on American stratification, culture and society, or social change. It is full of facts and statistics which can be used to support a variety of conclusions, one of which is Blumberg's.

### **Workplace Democratization—Its Internal Dynamics.**

By Paul Bernstein. New Brunswick: Transaction Books, 1980. 132 pp. Cloth \$12.50; paper \$4.95.

*Reviewer: CURT TAUSKY, University of Massachusetts at Amherst*

Bernstein's book presents a theory of workplace democratization and case materials. He links them by developing a model of democratization based on the cases.

Democratization is viewed as any system which increases employee influence. In selecting empirical cases, Bernstein gave preference to firms that initiated democratization on their own, are organized around profit-making, and are "pure" enterprises rather than an "industrial community" such as the kibbutz. A useful chart lists research on these sorts of enterprise.

In the case materials, comprising Part One of the book, major attention is given to 18 plywood manufacturing firms in Oregon and Washington. These firms are fully owned by their employees and more or less managed by them. Bernstein studied seven of these firms on-site in 1973. The worker-owned firms account for about one-eighth of the American plywood industry; they have been in operation from 19 to 33 years, with each employing between 80 to 450 worker-owners.

The first plant began in 1922 and the latest in 1955. A worker entered a plant by buying a share (initially \$1,000 but currently around \$40,000). The employee-shareholders meet annually and elect from among them a board of directors. They in turn appoint an outsider to serve as general manager; other administrative posts usually go to shareholders. The manager's salary, company expansion, modernization, diversification, are voted on by the worker-shareholders; they have the last word on all major business decisions.

Productivity per man-hour in these firms is high, running 20 to 25 percent above that of conventional plants. Bernstein comments that "pride of ownership motivates the majority to produce more than hired workers. . . ." Perhaps—but significant economic incentives are also present.

The worker-owned plywood mills pay all members an identical wage, from floor-sweeper to skilled panel-finisher. In all but the poorest market times, however, the average wage is 25 percent higher than among conventional firms in this industry, and the year-end division of profits ordinarily comes to several thousand dollars per person. Additionally, the companies provide free lunches, full medical care, life insurance, and gasoline at wholesale prices. When the owner-worker leaves or retires, he sells his share to an incoming worker or to the company for \$20,000 to \$40,000.

Most of these mills also employ hired labor, accounting on average for 10 to 15 percent of their work forces. Shareholders do not bring these hired workers in as

partners because they fear that doing so would dilute the value of their shares; only when a current member leaves is another person sold a share in the plant.

Bernstein uses these plywood firms and several other cases to develop a model of democratization in Part Two of the book. The model has six components: (1) participation in decision-making, whether direct or by elected representation; (2) frequent feedback of economic results to all employees—in the form of money, not just information; (3) full sharing of management-level information with employees; (4) guaranteed individual rights of freedom of speech, assembly, petition; (5) an independent board of appeal, composed of peers whenever possible, to handle disputes; and (6) a set of attitudes and values favorable to democratization. Each component is discussed in a chapter and linked to the case materials.

The model Bernstein proposes is grounded in empirical cases of viable enterprises and is indeed plausible. Still, the case materials also offer a message to the reader who may not be especially interested in workplace democratization but is concerned with workers' productivity. It seems that economic incentives play a major role in motivating workers toward higher productivity—with or without participation in management—but the economic incentives must be substantial.

Bernstein's book is a solid contribution to the current literature on democratizing the workplace. It is particularly useful and insightful in linking the organization and motivational elements which foster democratization. A readable style and availability in paperback further recommend the book to teachers and students dealing with this topic.

### **The Social Origins of the Irish Land War.**

*By Samuel Clark. Princeton: Princeton University Press, 1980. 418 pp. Cloth, \$25.00, paper, \$8.95.*

*Reviewer: KARL BOTTIGHEIMER, State University of New York at Stony Brook*

Historians—a historian would argue—make a lot of trouble for sociologists. They use phrases like "the Land War" as names of convenience for events like those which occurred in rural Ireland between 1879–82. Then they fold their easels and move elsewhere to paint new landscapes, coining new phrases, and caring little for the consternation they have caused among sociologists at the last location.

Samuel Clark is one of those who has been left pondering. With the bifocal vision of both the historian and the sociologist he has surveyed nineteenth-century Ireland in order to put the Land War into a perspective which will render it intelligible. The broader question has attracted many before him. By the late seventeenth century 90 percent of the land of Ireland was owned by an exogenous minority, distinguished from the bulk of the population by its Protestant religion and Anglo-Scottish origins. But by the outbreak of the First World War, even before the establishment of an independent Irish State, this situation had been dramatically reversed. Protestant landlordism was a broken force before the first shots rang out on Easter Monday, 1916. The process by which it was destroyed was a long one, but the events of 1879–82 were arguably the hinge, and it is logical that they, in particular, should attract a sociologist's attention.

The dilemma, as Clark sets it up, is that conditions for tenant farmers in the

late 1870s did not begin to approach the horrors of the famine years from 1846–50; yet the 1840s gave rise to mass emigration whereas the 1870s culminated in widespread, effective action which left Protestant landlordism permanently on the defensive. No charismatic figure like the Daniel O'Connell of the 1820s emerged to ride around the country whipping up the fury of crowds at "monster rallies." For his answer, Clark looks at the changing nature of rural organizations in nineteenth-century Ireland. He is struck by the degree to which the land question in the 1870s brought together diverse economic groups, which might have been expected to oppose one another: small tenants and large tenants, farmers and merchants, even town dwellers and countrymen. Much of the book is a description and explanation of how, and why, this convergence of groups occurred and resulted in a "challenging collectivity."

For those with less interest in Ireland than in sociological theory, *Social Origins of the Irish Land War* tends to support Charles Tilly and his collaborators in their mobilization perspective, as against Neil Smelser and the theoreticians of collective behavior. The Land War is seen by Clark as less a "conversion of people to a new set of beliefs" than as people being "mobilized in the name of principles which they already hold and in the pursuit of objectives to which they have long before committed themselves." Not only does Clark argue against the novelty of the principles and objectives enunciated during the Land War, but in the tradition of Eric Hobsbawm, E. P. Thompson, and George Rudé, he stresses that "qualitative changes in collective action" rather than "changes in the magnitude of unrest" was the key to the Land War's success.

For the student of Ireland, Clark provides a smorgasbord of statistics, samples, case studies, thoughtful digressions, and provocative hypotheses. The work is based on an impressive amount of diverse archival material, and displays thorough conversance with the recent secondary literature on the subject. Indeed, one of its chief attractions is the careful way in which it sets out the state of the subject and then endeavors to expand it by corrections, criticisms, and new explorations. Perhaps the most substantial of Clark's contributions is his painstaking examination of the change in the nature, number, and location of organizations representing farmers' interests through the middle decades of the nineteenth century. It is this which brings him to the assertion of "the challenging collectivity," an infelicitous phrase meant to underline the remarkable unanimity which gave the Land War so much of its force and effect. Although there will be those who challenge aspects of Clark's interpretation, there is likely to be consensus that it is a valuable book which advances our understanding of modern Irish history. If, to this reader, it is overlong, repetitious, and occasionally given to solemn vacuities (e.g., "This observation is, however, simply one corollary of the more general proposition that the coalitions into which people enter have a decisive impact on the character of their collective action"), those are nevertheless to be reckoned small faults in an important and interesting book.

**Power and Influence in a Southern City: Compared with the Classic Community Power Studies of the Lynds, Hunter, Vidich and Bensman, and Dahl.**

By James Bertram Haugh. Washington: University Press of America, 1980. 160 pp. Cloth, \$16.50; paper, \$8.50.

Reviewer: ANTHONY ANDREW HICKEY, *George Mason University*

This research report is not what the title would indicate it to be. Strictly speaking, it is not actually a study of community power and to suggest that it is comparable to the studies listed in the title is misleading. As part of his dissertation work, Haugh interviewed men who were identified as influential in the voluntary sector of Norfolk, Virginia. He has inserted a superficial review of the classic community power studies and published the result with University Press of America.

There are several levels on which this work could be critiqued and unfortunately it is not satisfactory on any of them. Theoretically, methodologically, and technically, it has serious problems. It seems to be designed for classroom use (each chapter concludes with study questions), but it would not make an adequate text.

The most serious problem derives from Haugh's conceptualization of a power structure. Haugh uses one key informant to devise a snowball sample of men who give their time and resources to nonprofit activities in their community. He finds a network of influentials who share some attributes and attitudes. This discussion is interesting particularly since many community studies do not analyze the voluntary sector in terms of influence and power. However, it is not clear how this network can be used to "test the adequacy" of Hunter's or Dahl's theories concerning community power.

A further problem concerns the nature of Norfolk which Haugh thinly disguises as "Colonial City." Norfolk is first and foremost a Navy town. The impact of the Navy on its power structure is not discussed in any depth, but, more importantly, there is no discussion of how Norfolk differs from Muncie or Atlanta. Since the author claims to be assessing and evaluating the classic works through comparison with his findings, this is a serious shortcoming. Is it appropriate to assess theory derived from studies of communities with large commercial sectors through a study of the voluntary sector of a community dominated by the federal government?

There are other problems. The illustrations are sophomoric, the writing style choppy; tables are presented with little or no discussion, typographical errors abound, and the book is poorly referenced. The research methods were inadequate. Haugh declares that he did not have time to interview the last 22 individuals uncovered in his snowball sample. He relies totally on one informant, pretentiously called Doc. Haugh's detailed discussion of data gathering, the type of tape recorder and tapes he used, belies the lack of rigor of the study. In general, this book offers very little to the student of community power.

**Residential Consumption, Economic Opportunity, and Race.**

By Franklin D. Wilson. New York: Academic Press, 1979. 213 pp. \$17.00.

Reviewer: JOHN EDWARDS, *University of London*

Questions of the differential racial consumption of housing and neighborhood services, residential choice and constraint, the relationship between these and workplace location and wage differentials can rarely have been subjected to the wealth and depth of empirical analysis they receive in this monograph. It is not uncharitable to say that this is a book to work through rather than to read, and the non-statistically minded will find it heavy going. It is a tribute to the author, however, that he has compressed so much information into less than two hundred pages and produced a work which, with a little effort, yields many rewards.

What Wilson has done is to weave together data from a variety of sources (but mainly from the One Percent Public Use Sample of Neighbourhood Characteristics) to illuminate, in successive stages, the interaction between a large number of family, housing, neighborhood, racial, workplace, and residence characteristics. The purposes are to identify the determinants of the demand for residential and neighborhood services in urban America for white and black families and to examine the effects of the suburbanization of employment on the economic opportunity structure for blacks. The book is in two parts, largely reflecting these separate but related purposes.

The task that Wilson sets himself in the first part is a formidable one, but it is made manageable by the use of a model of the linkages among the determinants of demand for residential services, estimated in a number of stages using (mainly) the census data. In Chapters 3 to 6 Wilson considers, in sequence, those aspects of the model concerned with housing demand, demand for neighborhood services, central city versus suburban location, and the relationship between location of residence and workplace for white primary families alone. Only in Chapter 7 does he add the race component as a means of estimating the extent to which this factor disturbs the pattern of relationships previously modelled. The objective here is to determine the *extent* to which differences in white/black residential consumption patterns reflect racial discrimination in housing and job markets and how much can be accounted for by differences in income and preferences.

The second part of the book is concerned with the effects of intrametropolitan residence and workplace location on the economic opportunity structure for both whites and blacks, thus reversing the common sequence of investigation—the effect of income on residential location. In Chapter 9 Wilson brings his mine of data to bear on the “mismatch hypothesis,” that is, that the gradual suburbanization of skilled, semi-skilled, and unskilled job opportunities in metropolitan areas has produced a residence–workplace mismatch for blue-collar workers (and especially blacks) who remain residentially concentrated in the inner city. Chapter 10 tackles the specific issue of the influence on wage differentials of suburban or central city residence and workplace location and consequent travel-to-work patterns for white and black workers.

To pack so much into so relatively short a space has required considerable concentration and presents the reader with a dense and closely written account from which it is not always easy to extract tangible results of moment. Indeed, the

author himself appears at times to be a captive of his welter of figures, as when, in interpreting one set of regression coefficients in the section on journey-to-work patterns, he tells us that "Distance travelled to work is positively related . . . to the distance between worker residence and city center," a hardly surprising finding, notwithstanding the trend toward suburbanization of employment. A more serious reservation, however, is how to bridge the gap between the results obtained from the sophisticated manipulation of large quantities of data and the behavior patterns of individuals and groups in operationalizing choice and managing constraints in housing and employment markets. The discussion of demand for neighborhood services acknowledges the influence of constraints on choice and the role played by urban gatekeepers, but can the realization of preferences and reaction to constraints really be quantified (as when it is argued that 50 percent of the total consumption level of blacks, compared with 27 percent for whites, cannot be explained by income and preference factors)?

All residence location decisions are made under some constraints and the outcome is the result of a process of optimizing. The constraints are greater for most blacks, but they are considerable for poor urban whites, too. The difficulty with empirical modeling of this kind is that the space it leaves for the interpretation of results in terms of market behavior (or even nonmarket behavior, as analysis of the allocation procedures of public housing authorities in Britain have shown) allows so much latitude that it can "mean" very different things depending on whether the interpretation is in terms of free-choice decisions or optimalization within constraints. It is partly in consequence of this that the policy implications Wilson is able to wring from his results appear less than signal.

The value of the book, therefore, and the rewards it produces lie in the wealth of information it contains rather than in any prognosis it can make about the directions urban and employment policies should take.

**Neighborhoods that Work: Sources for Viability in the Inner City.**

*By Sandra Perlman Schoenberg and Patricia L. Rosenbaum. New Brunswick: Rutgers University Press, 1980. 179 pp. \$14.75.*

**Back to the City: Issues in Neighborhood Renovation.**

*By Shirley Bradway Laska and Daphne Spain. New York: Pergamon, 1980. 352 pp. Cloth, \$37.50; paper, \$9.95.*

*Reviewer: MICHAEL D. GRIMES, Louisiana State University*

Neighborhood renovation or revitalization has drawn increased attention from both the popular press and the social science community during the past several years. These two books present and discuss some of the major issues.

Schoenberg and Rosenbaum's research monograph presents a comparative analysis of five case studies of lower- and working-class neighborhoods in St. Louis. A major goal is to develop a typology of propositions about neighborhood viability both for use in comparative research, and as a strategy for neighborhood organization. Drawing on previous ecological, ethnographic, and institutional research, the authors identify four characteristics of viable neighborhoods. Such



neighborhoods have (1) "established mechanisms to define and enforce shared agreements about public behavior," (2) "a formal organizational structure," (3) "linkages to public and private resource givers," and (4) "established context or interest aggregation . . . to create conditions for exchange."

The authors contend that each of these characteristics is necessary for neighborhood viability and that collectively they represent a sufficient condition for it. They also appear to treat them as hierarchical and sequential, such that a neighborhood may be placed along the continuum of viability according to the number of conditions satisfied.

Indicators for these attributes are specified and the scheme of analysis is applied to each of the five neighborhoods. The authors show both how the neighborhoods used different approaches to satisfy the criteria and how these resulted in different levels of viability. They conclude with a discussion of how the propositions can serve as guidelines for organization.

The major strength of the book is the derived typology of neighborhood viability. The authors' use of previous work on urban social structure and on social networks contributes to its value. There is also a good review of federal policy and programs dealing with neighborhood renovation.

The book also has weaknesses. Obviously, the "test" of the typology based on the five case studies is not a definitive evaluation of its usefulness. These neighborhoods are all from only one city and no detailed rationale for their selection is presented. The efforts to operationalize some of the variables are also troublesome.

*Back to the City* is an edited work which contains nineteen chapters, the majority prepared specifically for this volume. The book is divided into three sections. The first is labeled "National Evidence and Theoretical Perspectives." The first four articles in this section document the prevalence of neighborhood renovation in large American cities and offer explanations for it. The data in these articles come largely from case studies, limited private surveys, or federal data sources such as the Current Population Series and Annual Housing Surveys. None of these provides the precision of measurement or duration that the problem really requires. The fifth article, by Bruce London, presents an informative discussion of four alternative theoretical explanations for urban "reinvansion" (as he prefers to call the process)—demographic-ecological, sociocultural, political-economic, and as a social movement.

The second section is devoted to case studies of neighborhood renovations in several cities. While there are similarities in the experiences of these neighborhoods, there are also many differences among them. This suggests that neighborhood renovation involves a variety of different ingredients and outcomes, many of which are unique to the particular neighborhood itself (Schoenberg and Rosenbaum make the same point).

The third section, "Discourses Toward Responsible Policy and Action," contains five articles dealing with federal, metropolitan, and neighborhood responses to neighborhood renovation. It yields several interpretations worth noting. Perhaps the most important is that federal government reaction has not effectively ameliorated many of the negative consequences of neighborhood renovation. A second point is that, despite widespread coverage in the media and scholarly literature, the "back-to-the-city" movement remains small, compared to residen-

tial, business, and industrial movements *away* from the central city. A third point is that reliance on market forces alone tends to distribute the benefits derived from neighborhood renovation primarily to the upper and middle classes (and to other affluent interest groups such as real estate speculators), while the costs are extracted from minorities, the working class, and the poor.

This book's contribution is enhanced both by the multidisciplinary mix of authors and by the fact that most of the selections have not been published before. While the book may be faulted for its redundancy, particularly in its first and third sections, this is partially due to the large number of selections. A concluding chapter summarizing the other articles would have been welcome.

In conclusion, the evidence provided by these books could be seen as offering additional support for viewing the "urban crisis" in Marxian terms, as in Castells' *The Urban Question* (1979) or Tabb and Sawers' *Marxism and the Metropolis* (1978). Just as in the past, when the poor and black residents of the central city suffered the negative consequences of their upper- and middle-class neighbors' move to the suburbs, now their relative lack of fiscal and political resources will force them to absorb the bulk of the costs as the higher classes return to the central city.

#### **The Sunshine Widows.**

By Carol D. H. Harvey and Howard M. Bahr. Lexington, Ma.: Lexington Books, 1980. 151 pp. \$18.95.

Reviewer: PATRICIA MAC CORQUODALE, *University of Arizona*

Social scientists are often remiss in studying situations which are essentially naturally occurring experiments. This study is a notable exception; the authors examine a mining disaster in which chance produced nearly random assignment to the condition of widowhood. The authors conducted a carefully designed and sensitive study to examine reactions to sudden bereavement and the process of adjusting to widowhood.

In addition to its general contribution as a methodological example, the research design used in this study overcomes many of the limitations of previous work on widowhood. First, most of the existing studies do not involve comparison groups. Hence, it is difficult to determine to what extent the experiences described by widows are similar to those of other women of the same age and socioeconomic situation. Second, lack of comparison on background characteristics in the literature hinders examination of differences throughout the lifecycle between those who become widows and other women. This study involves a systematic comparison of three groups of miners' wives who were essentially similar before a fire at the Sunshine Mine: those widowed by the fire, those whose husbands survived the disaster, and those whose husbands worked in other mines. These comparisons reveal which reactions and adjustments were unique to widows and which were common to the experience of all the women.

Interviews were conducted at two times. The primary interviews, six months after the fire, focused on initial reactions and early coping. A five-year followup of the widows assessed long-term adjustment. Recalling their initial reaction to news

of the fire, the most common reaction among women whose husbands worked at the mine was not to consider the situation serious. Fires and fatalities were quite uncommon. Optimism waned as the rescue operation stretched over a week. During this week, emergent norms developed as to where to wait, whom to talk with, and how to behave. As might be expected, most women waited at the mine and provided mutual support and assistance.

The study of the adjustment process focused on changes in the women's social networks and psychological states. The use of existing measures enabled the authors to compare their results to those of other studies whenever possible. Differences between the groups in contact with friends and relatives, membership in voluntary associations, religiosity, positive and negative feelings, and satisfaction with the quality of life are reported. Correlational and regression analyses are used to explore the relations among these variables and to determine which factors accounted for happiness and satisfaction with the quality of life. Generally the factors that account for the most variance among widows are not equally important in the comparison groups. Happiness among widows is related to age, education, and religiosity; satisfaction with the quality of life depends on education, income, attendance at meetings, religiosity, and change in one's group of friends. These findings indicate that the availability of personal and economic resources influences adjustment to widowhood more than contacts in the social network.

These findings, however, draw attention to the major weakness of the study—the response rate. Initially the authors were able to interview fifty-seven percent of the miners' widows. In the follow-up, thirty-eight percent of the widows were included. The primary obstacle was that many widows moved away from the area. One wonders whether the women who moved differed substantially from those who remained. In particular, the ability to move may indicate access to personal and economic resources and different coping strategies.

Subsequent researchers using retrospective methods should note that the widows tended to idealize their husbands and marital relationships. Later the widows remembered the period of loss and bereavement as unhappier than they had rated it at the time. The major problems widows report—loneliness, raising children alone, income—are similar to those of other single women. The findings of this research underscore the importance of comparing the experiences of widows to those of other women and of studying adjustment processes over time.

### **Museums of Madness: The Social Organization of Insanity in Nineteenth Century England.**

By Andrew T. Scull. New York: St. Martin's Press, 1979. 266 pp. \$17.95.

Reviewer: VIRGINIA ALDIGÉ HIDAY, North Carolina State University

Scull's *Museums of Madness* takes its place with Rothman's *Discovery of Asylum* and Foucault's *Madness and Civilization* as a work of social history which views "madness" and its treatment in a particular period as manifestations of larger changes in the structure of society. Scull agrees with Rothman that the rise of asylums in the first half of the nineteenth century was not merely a response to the problems of urbanization and industrialization, but rather an attempt to promote

the stability of society. However, where Rothman contends that the state institutionalized deviants to build order in the face of changes threatening the safety of the country, Scull argues that the main underlying cause was economic, lying in "the advent of a mature capitalist market economy and the associated ever more thorough-going commercialization of existence." According to Scull, when capitalism broke the social bonds of paternalism and replaced them with naked self-interest, the bourgeoisie called for an institutional response to the indigent.

Scull contends that the capitalist state initially established workhouses "to remove the able-bodied poor from the community in order to teach them the wholesome discipline of labor." When workhouses became dumping grounds for all unemployables, capitalism demanded segregation of the able-bodied poor from those who were incapable of the order and discipline of work. Whether one accepts the driving force as capitalism, a utilitarian ethic, or a desire for order, one is unlikely to question Scull's arguments on the purpose of the workhouse, its transformation into a dumping ground, and the development of new institutions to remove unemployable deviants so as to return the workhouse to its original purpose. However, Scull's logic breaks down in attributing to capitalism the efforts to separate the mad from other unemployable deviants: he does not satisfactorily explain what interest capitalists had in those incapable of work, once the able-bodied had been separated and disciplined.

Scull's logic also falters when he argues that "medical entrepreneurs" captured the "market of lunacy." While he documents well how physicians came to dominate asylums in nineteenth-century England through defining madness as illness and claiming special skills, he fails to provide an economic motivation. Although a few prereform homes for the mad were profitable, pay and funding were low in public asylums. Furthermore, given the mammoth, overcrowded institutions, with heavy administrative burdens and an unpleasant, disturbing clientele, many chronically insane or physically incurable, one can see little motivation to obtain control of asylums other than that which Scull finds suspect, humanitarianism. Scull describes the scandalous treatment of the mad and the moral outrage which guided lunacy reform, but he does not seriously entertain the idea that asylum physicians were essentially decent men who believed in their ability to do a better job than others because of the scientific basis of medicine. It is true, as Scull argues, that psychiatric physicians had no special knowledge or skills as scientific practitioners, but he forgets that prior to the germ theory nonpsychiatric physicians had little to offer those who were physically ill—yet they held more authority and prestige than other health practitioners. Scull's arguments that physicians were impelled to seek more cases and that they stretched the definition of mental illness to enlarge their domain are not consistent with the facts he presents, showing that asylum physicians knew many of their patients were not mentally ill, but rather undesirables of all sorts, and that patients were brought to the asylums by family members who discovered a dumping ground for "their sick, aged, or otherwise incapacitated relatives."

Despite the logical weaknesses in his polemic against psychiatry and capitalism, Scull's *Museums of Madness* provides a well-documented, detailed social history of the battle to reform pre-asylum conditions and of the failure of reform institutions. The details of the demise of moral treatment, of worsening physical conditions, insufficient funds and increased dumping, of growth in the number,

size, and population of institutions, of official explanations of "increasing" mental illness, and of psychiatric maneuvers to gain control of asylums—all comprise an important story of state intervention in the lives of the helpless. Whether the reader accepts Scull's account of the causes of that intervention, he will be confronted with major sociological issues of social change and social control. *Museums of Madness* should have wide appeal to sociologists, but it is particularly well suited for graduate seminars in medical sociology and deviance.

**Alcohol Problems: Review, Research, and Recommendations.**

*Edited by David Robinson* New York: Holmes & Meier, 1980. 246 pp. \$24.75.

*Reviewer:* SAMUEL E. WALLACE, *University of Tennessee*

Because alcohol problems are so diverse and numerous, many professionals outside medicine "are called on to deal quite regularly with people for whom alcohol has caused some kind of problem." To help these non-medical professionals—social workers, probation officers, health educators, the police, employers, policymakers and others—David Robinson has prepared this volume. "This is a book about alcohol, its use, associated problems, and certain recommendations. . . ."

In lucid prose Robinson introduces the seven major sections of *Alcohol Problems*—Drinking and Social Life, Alcohol Problems and Alcoholism, Teenage Drinking, Women and Alcohol, Alcohol and Work, Drinking and Driving, and Prevention and Public Health. With merciless editing, Robinson reduces the 28 articles he includes to five to eleven pages each, and can therefore add several pages of introduction to each section, six cartoons, and an introduction to the entire volume, yet keep the length to 246 pages.

The result is a broad and surprisingly thorough examination of alcohol problems from their onset to their prevention. Among the many themes explored, albeit briefly, are: variations in drinking, the functions and dysfunctions of treating alcoholism as a disease, alcohol abuse and the use of other drugs among teenagers, sexism in the identification and treatment of women alcoholics, drinking at work by occupation and by industrial alcoholism programs, and the consequences of alcohol control policies.

Given the audience for whom this book is intended, Robinson succeeds rather well in providing "a little more about the background to the alcohol problems they [practitioners] are called on to handle." One wonders, however, if that same audience will pay the high price asked for these 254 pages.

**Cocaine Users: A Representative Case Approach.**

*By James V. Spotts and Franklin C. Shontz.* New York: Free Press, 1980. 517 pp.

*Reviewer:* JAMES T. CAREY, *University of Illinois, Chicago Circle*

The increasing popularity of cocaine among middle-class groups has led to a series of research investigations of cocaine's physical, psychological, and social effects

This is one such investigation, sponsored by a grant from the National Institute on Drug Abuse to study the patterns and life styles of cocaine users in the greater Kansas City area.

The authors selected nine heavy cocaine users, men who presumably could speak with authority about it, and administered a battery of tests to them. They characterize their approach as the "representative case method" though just what their cases represent is not clear. They asked officials of the criminal justice system and several drug treatment programs in Kansas City to supply them with names of those who took cocaine intravenously. The users were categorized in terms of life adjustment patterns identified from the literature. The authors do not claim that their nine subjects are representative of extremely heavy cocaine users, nor were they a random sample from the population of users in Kansas City generally. The subjects served as knowledgeable informants on the effects of the drug. Chapters 3 through 11 give detailed profiles of each of the respondents. They apparently did not know one another and the data collected on them was generated in the researchers' offices, not in the field. The authors had measures of intelligence on each, personality tests, drug preference rankings, Q sorts (to tap attitudes towards drugs, self, and others), a role construct repertory test (to elicit cognitive constructs subjects used to describe important people and roles in their lives), semantic differentials that ranked various substances, a social and drug history, a cocaine experience inventory, an activity and experience audit, and extensive interviews. The information gathered gives no clues to why cocaine is considered the "Cadillac of the drug market," perhaps because the sample included no actors, models, athletes, artists, politicians, or diplomats. Only three of the subjects, all white, could be characterized as middle-class or upper middle-class; one was a blue-collar worker, and five made their living illegally (dealing, pimping, armed robbery, and street hustling). All except one had been arrested for drug-related crimes. The total number of arrests ranged from one to more than 100, averaging 21 per man. It was not clear from the data presented just how those who were not full-time dealers managed to support their very expensive habits. The four heaviest users annually consumed from 26 ounces to almost 1 ½ kilos of cocaine. Two admitted spending between \$35,000 and \$40,000 per year. The current street value of one of the dealer-subject's annual consumption was \$117,000. The investigators, however, did not feel free to explore the economics of dealing and consuming: "For obvious reasons the investigators did not attempt to pursue the matter of how these men maintained regular supplies of cocaine." None of the subjects seemed particularly disturbed by cost factors and the authors conclude that this was because "all were, to some degree, dealing and thereby able to reduce the cost of their personal use."

The investigators were not testing any hypotheses and assumed at the outset that their subjects and cocaine users in general were not pathological. The results of their exploratory inquiry on the group as a whole are rather prosaic. They are surprised at the number of arrests; all but one had been introduced to cocaine by friends or acquaintances; all had experience with other drugs prior to cocaine. The authors conclude by comparing their findings to the literature in a number of areas. Again there are no surprises: the physiological, psychological, and cognitive effects of intravenous injection of cocaine were similar to what has been reported. They state that their work fits in with a three-stage model of dosage increase: the "rush," arousal carried to an extreme, with anxiety developing and inhibition, and finally

apathy, indifference, misery. They make a special effort to distinguish between the effects of cocaine and amphetamines. Both, they note, are powerful central nervous system stimulants, but cocaine is more of an "ideational" drug and it encourages thoughtfulness and introspection. Cocaine does not produce a "wired" effect nor does it lead to rigid, compulsive behavior. The potential for violence with cocaine use is also lower than with amphetamines; while amphetamine users are prone to attack and to try to overwhelm a hostile environment, cocaine users are prone to withdraw, to isolate themselves and to barricade the doors. They also note that, although cocaine suppresses appetite, it does not lead to emaciation, as do amphetamines. The authors do not deal with the question of whether the differences between the two substances might be related to factors other than chemical effects, like the background characteristics of users, the setting within which use occurs, and the impact of the law. The brief discussion of the differences between the two drugs is by far the most spirited in the book. The authors leave no doubt about which they think is better, although they assert that efforts to "decriminalize" cocaine at this point are inappropriate because some of its effects are not benign.

From a theoretical perspective, *Cocaine Users* is a disappointment. Structural or interactional variables are ignored and the inquiry does little more than provide nine case studies to ponder. The rationale for the massive testing of the subjects is not explicated, nor are the results sufficiently discussed so the reader can get some sense of how their respondents differ from "normals." The argument for the representative case method as "a powerful tool for studying drug abuse" strikes this reviewer as overblown. Is this what researchers must do to persuade funding agencies that some of their findings are quantifiable and that they are not proposing to resurrect the discredited case study method?

### **Broadcasting in the United States: Innovative Challenge and Organizational Control.**

By Vincent Mosco Norwood, N.J.: Ablex, 1979. 136 pp. \$12.95.

*Reviewers:* HALUK SAHIN, *Cleveland State University* and JOHN P. ROBINSON, *University of Maryland*

This book from the publisher's series on Communication and Information Science examines four decades of Federal Communications Commission decisions on the technological innovations of FM radio, UHF television, cable television, and subscription television. The author argues that these "innovative challenges" have been controlled by the FCC in order to foster and perpetuate the dominance of AM radio and VHF television.

The report is described by the author as an attempt to discover "significant parallels in approach and resolution" that go beyond the specifics of each case. His evidence that the FCC was very slow in placing the new technologies on its agenda is quite convincing. It is also clear that it acted conservatively in all four cases. Each technological innovation was assigned to an ancillary role rather than being considered as an alternative to the prevailing system.

The author does not provide any clearcut analysis of these decisions because

he believes that there is no "one single key to understanding FCC decision making. He does not subscribe to the simple view that the FCC has been "captured" by the broadcasting industry, although significant amounts of industry pressure are a fact of life at the FCC.

Why then have the major industry interests won just about everything they have wanted from the FCC? Mosco concludes that the answer is more structural than motivational. What he means by structure is not objective relational arrangements, but the cognitive mindsets used by the FCC commissioners. These mindsets determine how organizations will deal with the complexity and uncertainty associated with innovation. Mosco argues that these cognitive explanations offer far more insight into the FCC's ultra-cautious and conservative decisions than models that view organizations as rational "machines," loose coalitions of actors, or units primarily responding to external influences and constraints. A key focus of the author's cognitive approach thus becomes how the FCC agenda gets defined.

Following Steinbruner, this cognitive approach allows the author to explain FCC actions as "responding to complexity by imposing a structure on it rather than by using probabilistic methods," while ignoring "information that does not conform to this cognitive structure." Value conflicts inherent in complex problems are avoided by dividing them into discrete long-term and short-term categories. Thus, in three of the four cases, the FCC did define the technical innovation as perhaps the most appropriate long-range solution, but promoted the established broadcasting system as the norm "for the short run."

The main problem with this cognitive approach lies in the fact that it itself is in need of explanation. Organizational actors do not work in an environmental vacuum. How did these particular organizational coping styles evolve and why were certain cognitive structures preferred over others? Without a sociohistorical analysis of the objective factors that have produced these cognitive patterns, the type of "structural" answer Mosco offers begs the question. Further, the cognitive structures that evolved at FCC were undoubtedly influenced by the role and functions of other regulatory agencies in the U.S. Government

This does not mean the author is oblivious to the ways the FCC can function as an agency of legitimation. While the FCC "provides an institutional target for those dissatisfied with the system's shortcomings," he points out how "those with power and resources" get what they want. He also notes that those who own the broadcasting networks profit from the oligopolistic nature of the system while the FCC serves "to absorb much of the criticism directed at the system." Yet, we are not offered further analysis of this legitimation process.

Despite its openly critical stance and the radical answers that lurk beneath its cautiously formulated questions, the book fails to propose any real alternatives to the present system. The one suggestion that the author does make is a complete review of the 1934 Communications Act—an idea that obviously predates the annihilation of the Van Deerlin rewrite by the broadcasting industry in 1979. A case study of the fate of that legislation (HR 3333) may shed more light on why the FCC has functioned as a "security blanket" for the broadcast industry.



**Privacy and Confidentiality as Factors in Survey Response.**

*By the Panel on Privacy and Confidentiality as Factors in Survey Response, National Research Council. Washington: National Academy of Sciences, 1979. 274 pp. \$12.75.*

*Reviewer: RICHARD C. ROCKWELL, Social Science Research Council*

This report summarizes results from an exploratory investigation of how various conditions of confidentiality and privacy affect the completeness and accuracy of survey data. In this study "privacy" refers to an individual's right to refuse to disclose information, and "confidentiality" to the recipient's safeguarding of information about another individual. The study was undertaken by a panel of the Committee on National Statistics of the National Research Council; William H. Shaw was chairman.

The report offers tentative conclusions and recommendations based on the panel's preliminary analysis of two experiments in survey design, as well as on observations from eight small group discussions and a canvass of the experience of survey research organizations. A broad range of technical issues in survey methods is addressed, but the report does not consider the ethical and legal questions that have been raised concerning rights to privacy or research use of confidential records.

The first of the two experiments was a pilot survey of attitudes to survey research, using interviewers from both the U.S. Census Bureau and the University of Michigan's Survey Research Center (SRC). The sample was randomly divided between the Census Bureau and SRC, and the regular interviewers of both organizations administered questionnaires which differed only in identification of the survey organization. This design permitted an assessment of the effect of interviewer affiliation on refusals to be interviewed and on responses to specific attitudinal questions. One of the few differences between results from the two organizations was a higher refusal rate for SRC interviewers, about 13 percent compared to the Census Bureau's 6 percent. The panel offers little analysis of this difference, other than to note that the Bureau's sample included a higher proportion of persons aged 65 or older.

Respondents' reports of contacts in other surveys did not support the notion that the population is generally afflicted by frequent surveys, but many respondents expressed sour views of the value and validity of survey research and of the trustworthiness of survey organizations. Only 5 percent of the 1,187 respondents believed that the Census Bureau would be able to protect the confidentiality of individually identifiable records against other agencies if those agencies "really tried" to get access, and 18 percent believed these records were open to public examination.

Respondents' concerns about privacy are expressed in refusals to participate in a survey, in nonresponse to particular questions, and in distortion or concealment of information. When asked to recall surveys in which they had recently participated, to indicate whether they had been asked questions the survey taker "had no business" asking, and to list the questions they had particularly disliked, the great majority of respondents recalled no questions as objectionable. But the question most frequently mentioned as disliked was on income. A question on religion, which appears in many private surveys and polls, was never mentioned.

This survey's own question on income had the highest proportion of nonresponses of all the personal or family descriptive items (11.8 percent); it used a flash card with seven broad income categories. Apparently some respondents who did answer this question also indicated their discomfort with it.

On the basis of this evidence, the small group discussions, and the canvass of research experience, the panel recommends "continued efforts . . . to minimize the adverse effect of direct income questions." The use of proxy measures of economic status is suggested, as are subsampling, special confidentiality procedures, and "the formulation of income questions in broad instead of detailed and exact terms." Coming from an Academy panel, this recommendation may be expected to be weighed by the Census Bureau and other statistical agencies during the redesign of surveys. The suggested alternatives might be appropriate if the purpose for the income question were the univariate estimation of economic status. However, research in such areas as use of health services, employment, equality of opportunity, and life course transitions relies upon detailed question about income, wealth, and consumption as direct measures of economic resources and limitations. The panel's recommendation recognizes only the problematic aspects of the income question and ignores its place in research, as well as in public policy.

The second experiment was to ascertain how respondents' behavior was affected by whatever was promised them about the confidentiality of their responses. The assurance of confidentiality was, by random assignment, one of five levels: confidentiality in perpetuity, for 75 years, or for 25 years; unmentioned; or the statement that replies "may be given to other agencies and to the public." Interviewing was undertaken by the Census Bureau, and the questionnaires were the same for each case in an intended sample of 2,440 cases, 488 in each treatment.

There were a few more refusals to be interviewed when the respondent was told his responses were public than there were at the other extreme; the refusal rate rose to 2.8 percent from the 1.8 percent obtained for the promise of perpetual confidentiality. As panel member William O. Aydelotte remarks in a separately signed note, an assurance of confidentiality for 75 years raised the refusal rate by only one-tenth of a percent and a promise of 25 years added four-tenths of a percent to that, and these two levels are in the range of proposals for research access to these records. It is apparently not true that assurance of a high level of confidentiality is essential for a low refusal rate (but the attitude survey warns us that *any* promise may not be believed). In the attitude survey, about half the respondents felt that Census records should be open for research use "after a reasonable time." The evidence suggests public receptivity to release of Census records for research use without long delay.

The panel presents more than 100 tables in a book with only 162 pages of text and over 100 pages of appendices; many of these tables should have been consolidated according to standard principles of tabular presentation. The reader leaves the report with the sense of having taken an unsatisfying flying tour of the data. Both surveys deserve further analysis, particularly if that research were informed by some analytical concepts.

Appendix B is the familiar note on sampling errors, found in most responsible statistical publications. The special treatment given to this particular source of error is a bit ironic in this context, for this study dealt with a multitude of sources of error, including refusal to participate, item nonresponse, misunderstandings,

deliberate distortion, reporting on persons other than oneself, and inability to provide accurate answers. The error introduced by sampling can usually be controlled by increasing sample size, and may be of much less importance in large surveys than other sources of error.

**The Fieldworker and the Field: Problems and Challenges in Sociological Investigation.**

*Edited by M. N. Srinivas, A. M. Shah, and E. A. Ramaswamy. New York: Oxford University Press, 1980. 288 pp. \$13.95.*

*Reviewer: JOHN B. STEPHENSON, University of Kentucky*

"I learned something about such qualities as courage, friendship, kindness, patience and tolerance from my encounters with various people in the field—which in itself is not a mean achievement, I believe." Indeed not; would that all of us who have engaged in field studies such as those reported in this book could have learned as much. And beyond achieving greater virtue as human beings, I wish we could learn more about the sociology (or anthropology) of field studies which rely on participant observation from reflection on those studies.

*The Fieldworker and the Field* carries us only a few steps in developing the principles of a sociology of field studies, but it does provide some interesting case material in which such principles could be rooted. Here are eighteen essays, most of them brief, personalized accounts of observers responding to communities, families, and organizations which are the objects of their study. All the writers, with the exception of one German, are from India; all but one are currently residents of India. Most of the case materials are set in India, although the exceptions (involving Indians studying Japanese, Zambians, and native Americans) are quite interesting. In short, this is a book mostly by and about Indians studying India. Most of the studies on which the writers reflect were conducted in the 1960s. Many of the writers appear to have some connection with the senior editor as former students or colleagues.

What is in this book for readers who share no special interest in area studies of India? An invitation to share subjective reports on the interaction between observer and field. A chance to join an international conversation on what we have learned collectively after seven decades of this "distinctive method" in sociology and social anthropology. Another set of toe-holds to guide us toward some usable generalizations about field observation. And a memorable Navajo anecdote in Pandey's essay, where the question of whether an outside observer can leave the field setting in its pristine state should be settled for all time: ". . . my host asked me to give him ten dollars. When I asked him 'for what?', he immediately retorted, 'You are not supposed to ask that. If I asked Clyde Kluckhohn for ten dollars, he gave me twenty.'" Thus do great anthropologists leave their tracks.

Despite the richness of personalized detail, on the other hand, there are few new generalizations offered in this book. The Introduction is best in this regard, and should be read as an afterword. (Srinivas' artful synthesis, incidentally, contains an apt methodological note from Mao Tse Tung on turning one's eyes downward to understand social conditions.) The raw material is provided here for

discussion of how the sex of the observer, the degree of participation by the observer, and prior familiarity with the study area influence fieldwork, but these issues are not fully developed. The interesting question is raised of what difference it makes to come from an underdeveloped country to study a more "advanced" industrial society, but it is not resolved. Caste considerations present in the case of Indians studying India may or may not be a special case of stratification-related issues confronting social research, but they are not elaborated here. While this book may have been intended to supplement methodology texts, students will not find many rules to follow here.

Despite the minor irritations of poor printing, the need for tighter editing, and the need for a better balance between raw material and synthesis, this book should find its way into the libraries of sociologists and anthropologists who are self-conscious about their involvement in observational field studies. It will advance our conversation and lead us a step further toward a sociology of fieldwork.

# Take Note

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**Alvares, Claude.** HOMO FABER: TECHNOLOGY AND CULTURE IN INDIA, CHINA, AND THE WEST FROM 1500 TO THE PRESENT DAY. *Hingham, Ma · Martinus Nijhoff, 1980. 275 pp. \$46.20.* This is a book with a mission: to lay bare the ethnocentricity of Western scholars who have analyzed non-Western cultures and technologies. Distinctions between "advanced" and "traditional" technologies are seen as ideological rather than analytic, and the author further argues that unless the Third World resists Western technology it will find itself in a colonial situation even more exploitative than earlier colonial systems. A provocative study that deserves attention that its price makes it unlikely to receive.

**Anderson, John A.** INTERNAL MIGRATION DURING MODERNIZATION IN LATE NINETEENTH CENTURY RUSSIA. *Princeton. Princeton University Press, 1980. 222 pp. \$18.00.* Data from the 1897 Russian Census indicate that migration to established areas is associated with coming from a relatively modernized place of origin, while migration to newly populated areas is associated with population pressure in a predominantly agricultural place of origin. A useful addition to the growing literature on modernization in prerevolutionary Russia.

**Armytage, W. H. G., R. Chester, and John Peel (eds.).** CHANGING PATTERNS OF SEXUAL BEHAVIOR. *New York. Academic Press, 1980. 214 pp. \$24.00.* The proceedings of the Fifteenth Annual Symposium of the Eugenics Society consist of 13 papers on a wide variety of topics including sexuality in the post-Kinsey era, sexual attitudes of young people, sexual experience and attitudes of British women, the sociology of the orgasm, lesbianism, transsexualism, incest, and sex and the handicapped. An interesting addition to the fast-growing literature on the oldest pastime.

**Bailey, Joe.** IDEAS AND INTERVENTION: SOCIAL THEORY AND PRACTICE. *London & Boston. Routledge & Kegan Paul, 1980. 164 pp. Cloth, \$24.00; paper, \$12.00.* A criticism of formalism in sociological theory and an argument for the contribution of theory to social intervention. A discussion of the general relation between disciplines and occupations is followed by illustrations of the usefulness of sociological theorizing in law, social work, and urban planning. Intervention to relieve suffering, says Bailey, is the principle that should unite a pluralism of theories.

**Barnes, J. A.** WHO SHOULD KNOW WHAT? SOCIAL SCIENCE, PRIVACY AND ETHICS. *New York: Cambridge University Press, 1980. 232 pp. Cloth, \$17.50, paper, \$4.95.* Looks at changes in the conduct of social research since the early nineteenth century. Of particular interest is the changing power of scientists relative to sponsors, to gatekeepers, and to the citizens they hope to study. Discusses the possibility of a policy of inquiry by negotiation, where citizens have a greater say in deciding what questions are asked and what appears in print.

**Barth, Gunther.** CITY PEOPLE: THE RISE OF MODERN CITY CULTURE IN NINETEENTH-CENTURY AMERICA. *New York: Oxford University Press, 1980. 289 pp. \$19.95.* A historian's account which will enable sociologists to put more flesh on the bones of urban structure and process. Barth describes the emergence of an urban lifestyle, looking at such topics as the development of newspapers, apartment life, department stores, sports, architecture, and entertainment. A delight to read, whatever your interests.

**Bennett, Ruth.** AGING, ISOLATION AND RESOCIALIZATION. *New York: Van Nostrand Reinhold, 1980. 240 pp. \$14.95.* A collection of works on the quantitative measurement of social isolation of the aging. Includes an evaluation of treatment techniques for different patterns of isolation and a concise summary of current research findings, as well as discussion of an experimental Friendly Visitor Program for reducing social isolation.

**Bilski, R., I. Galnoor, D. Inbar, Y. Manor, and G. Sheffer (eds.).** CAN PLANNING REPLACE POLITICS? THE ISRAELI EXPERIENCE. *Hingham, Ma.: Martinus Nijhoff, 1980. 337 pp. \$41.60.* Ten papers are arranged under three headings; theoretical aspects of national planning (including considerations of model building, ideology and values, and environmental influences on the public sector); case studies (with specific focus on water planning, public health services, urban and regional planning, and funding problems); organizational aspects of planning and policy-making. The conclusion is that planning does not replace politics (at least in Israel), and that national planning is not made more attainable by developing a system of partial (subsystem) planning. A book of interest to all concerned with change in the more developed societies.

**Caplan, Gerald (with Ruth B. Caplan).** ARAB AND JEW IN JERUSALEM: EXPLORATION IN COMMUNITY MENTAL HEALTH. *Cambridge: Harvard University Press, 1980. 300 pp. \$15.00.* Patterns of communication and conflict between Arabs and Jews in Jerusalem are analyzed in a number of settings, ranging from routine, everyday contacts (government offices, marketplace) to crisis situations (the burning of a mosque). The author, a community psychiatrist, describes his participation in a vocational education program for the Arabs of Jerusalem, and he argues that the mental health consultant can operate as a successful mediator in community disputes.

**Chapman, Jane Roberts.** ECONOMIC REALITIES AND THE FEMALE OFFENDER. *Lexington, Ma.: Heath, 1980. 234 pp.* Written by the founder of the Center for Women Policy Studies, this volume examines the relation between changes in women's socioeconomic status and their involvement in crime. Topics include perceptions of female crime, women in prison, patterns of female crime, and expected developments in female criminality.

**Chorover, Stephan L.** FROM GENESIS TO GENOCIDE. *Cambridge: MIT Press, 1980. 237 pp. \$5.95.* A neuropsychologist attacks as unfounded all forms of behavior modification and control resting on biological determinism. Topics include the genetics of intelligence, race, XYY males, psychosurgery, drug addiction, and criminality. Nothing much really new here to sociologists, although the historical context of the interplay between science and society makes for interesting reading.

**Dunn, John.** POLITICAL OBLIGATION IN ITS HISTORICAL CONTEXT: ESSAYS IN POLITICAL THEORY. *New York: Cambridge University Press, 1980. 355 pp. \$34.50.* This collection of the author's writings over the last fifteen years or so has more to offer the sociologist than its title would suggest. A sampling of the problems addressed include the nature of commitment to such social units as family, tribe and nation; the nature of state authority; and the sources of revolution in the contemporary world. Worth considering for use in graduate sociological theory courses.

**Ehrlich, Stanislaw, and Graham Wootton (eds.).** THREE FACES OF PLURALISM: POLITICAL, ETHNIC, AND RELIGIOUS. *Westmead, England: Gower, 1980. 219 pp.* The thirteen essays in this collection were originally presented at the Xth and XIth World Congress of the International Political Science Association, and consist of both theoretical pieces and empirical case studies. Countries covered include West Germany, Nigeria, Canada, Belgium and India.

**Fried, Martha Nemes, and Morton H. Fried.** TRANSITIONS: FOUR RITUALS IN EIGHT CULTURES. *New York: W. W. Norton, 1980. 306 pp. \$14.95.* A description of the rituals involved in four critical transitions—birth, puberty, marriage, and death. The data, based on both the authors' fieldwork and secondary works, is drawn from cultures which vary greatly in their scale, technological complexity, and social structure. The analysis emphasizes the functions of the rituals in regulating group membership and social roles.

**Goodin, Robert E.** MANIPULATORY POLITICS. *New Haven: Yale University Press, 1980. 250 pp. \$17.95.* Distinguishing between the acceptable use of power and unacceptable manipulation, the author identifies modes of manipulatory politics including lying, laying linguistic traps, rhetorical trickery, symbolic rewards, political rituals, and the rigging of "obvious" solutions to problems. Numerous examples should be of interest to students of politics, as well as anyone concerned with being manipulated—or, for that matter, with manipulating.

**Graebner, William.** A HISTORY OF RETIREMENT: THE MEANING AND FUNCTION OF AN AMERICAN INSTITUTION, 1885–1978. *New Haven: Yale University Press, 1980. 270 pp. \$22.50.* Presents the changing institution of retirement as influenced by social, political, and economic factors in the U.S. during the twentieth century. The efficiency of retirement systems for civil service employees and teachers is also discussed; as are the relations of retirement to social security, the unemployed, and age discrimination.

**Harre, Rom.** SOCIAL BEING. *Totowa: Rowman & Littlefield, 1980. 429 pp. \$25.00.* A genuinely new social psychology? That, at least, is the promise of this book, the latest by a noted British exponent of "realism" in the philosophy of science. Draws on both theoretical arguments and new empirical reflections to argue the pointlessness of most experimental studies of social action. Attention is needed, Harre declares, to practical reason, the temporality of all action, and the importance of an expressive dimension. He attempts to remedy both the theory of the person and the theory of the social conditions of action to facilitate that attention.

**Houseman, Gerald L.** THE RIGHT OF MOBILITY. *Port Washington, N.Y.: Kennikat, 1979. 111 pp. \$12.50.* For Houseman "the right of mobility" involves the freedom to move about and access to peaceful settlement at a chosen destination. He claims that the

development of the "mobility concept" will shed light on such apparently disparate phenomena as migration, housing, residential segregation, and transportation. Since this volume is part of the National Universities Publications' "Multidisciplinary Studies in the Law," much of the text is devoted to an analysis of the legal status of "the right of mobility" through the review of relevant court cases.

**Jones, M. A.** *THE AUSTRALIAN WELFARE STATE*. Winchester, Ma.: Allen & Unwin, 1980. 244 pp. Cloth, \$25.95; paper, \$13.95. Provides a brief, yet comprehensive, account of the origins and development of welfare in Australia, and a description of such topics as the cash benefit system, poverty and inequality, the taxation system, employment, health, housing, and personal services. In addition, the dozens of tables of basic data provide an opportunity for others to engage in more detailed comparative analysis.

**Kaufman, Edy.** *URUGUAY IN TRANSITION*. New Brunswick: Transaction Books, 1979. 126 pp. \$9.95. Despite a long tradition of constitutional democracy and political stability, the South American nation of Uruguay faced a coup d'état in the early 1970s that established a military government claiming absolute power. Kaufman tries to come to grips with this transformation using a complex hierarchical model of the political decision-making process that emphasizes the importance of a multi-level analysis. Both internal and international variables are conceptualized to have an effect on the "operational and psychological environment" in which political elites act. A variety of hypotheses about the effect of military intervention are discussed in light of the Uruguayan example and the possible routes back to a civilian government in Uruguay are sketched out.

**Kleppner, Paul.** *THE THIRD ELECTORAL SYSTEM, 1853-1892. PARTIES, VOTERS, AND POLITICAL CULTURES*. Chapel Hill: University of North Carolina Press, 1979. 446 pp. \$21.00. Quantitative and qualitative data are used in an analysis of the major political divisions among the mass electorate. Ethno-religious aggregates are shown to be more important bases of parties and political behavior than economic aggregates. A book of interest to political sociologists and sociologists of religion, as well as to political and social historians.

**Krause, Elliott.** *WHY STUDY SOCIOLOGY?* New York: Random House, 1980. 189 pp. \$4.95. Based on the premise that the title poses a legitimate question for the beginning student to ask, Krause tries to indicate how sociology is useful to the everyday citizen. Chapters discuss social identity, relationships, work, communities and neighborhoods, politics, the history of sociological thought, and "thinking sociologically." The author illustrates the sociological approach by drawing on both "classical" and contemporary research and through reference to his own personal experience as an active participant in community politics. This book is primarily intended as a supplement to other texts in introductory courses.

**Lansbury, Russell D. (ed.).** *DEMOCRACY IN THE WORK PLACE*. Melbourne, Australia. Longman Cheshire, 1980. 259 pp. \$9.95. Seven of seventeen articles are devoted to recent developments in industrial democracy in Australia, ranging from participation in a large multinational enterprise to a small engineering company. Democracy in trade union organizations is also examined, together with the question of the extent to which minority and disadvantaged strata have gained access to participat-



ory mechanisms. The other ten papers deal with general theoretical issues, and provide comparative materials on Sweden, Yugoslavia, West Germany, the U.S., and Israel

**Lawson, Kay (ed.).** POLITICAL PARTIES AND LINKAGE: A COMPARATIVE PERSPECTIVE *New Haven. Yale University Press, 1980. 410 pp. Cloth, \$35.00, paper, \$8.95.* Sixteen articles by political scientists examine the ways in which political parties, in a variety of settings, link citizens to policymakers. Some of the parties are competitors for power in democratic states; others are themselves part of the state apparatus in one-party systems. The essays suggest that a common vocabulary, but not yet any substantial scheme of analysis, has been developed for this line of analysis.

**Leonard, Diana.** SEX AND GENERATION. A STUDY OF COURTSHIP AND WEDDINGS *New York. Methuen, 1980. 325 pp. \$29.00.* A study of the process of courtship and marriage in a provincial town in South Wales, based on extensive interviews and four years of participant observation involving fifty young couples and their parents. Provides many insights into love and family life in British society by focusing on the complex rituals associated with going steady, getting engaged, stag and hen parties, weddings, honeymoons, and setting up a home.

**Lerner, Melvin J.,** THE BELIEF IN A JUST WORLD *New York. Plenum, 1980. 209 pp. \$20.95.* A fundamental delusion is the belief that the world is, or could be, just. Lerner presents a theoretical argument and empirical evidence to support the case that believing it is not just foolishness, but a general fact of human life. We need to believe that there is justice in order to orient ourselves for action. Experimental evidence shows that people go to considerable lengths to attempt to make the world conform to their expectations that it be just.

**Leviatan, Uri, and Menachem Rosner (eds.)** WORK AND ORGANIZATION IN KIBBUTZ INDUSTRY *Norwood, Pa. Norwood Editions, 1980. 213 pp. \$17.50.* Previously published articles attempt to acquaint the English-speaking reader with the intensive industrialization process that the Israeli kibbutz society has experienced in the last 15 years. It reports on research concerning problems of industrialization and the adaptations of the kibbutz to these problems within the framework of kibbutz ideals.

**Little, Kenneth.** THE SOCIOLOGY OF URBAN WOMEN'S IMAGE IN AFRICAN LITERATURE. *Totowa: Rowman & Littlefield, 1980. 174 pp. \$27.50.* Examines the portrayal of urban women in various social roles as depicted by some thirty African writers and then asks: How true is this image to real life? The conclusion is that women are inadequately characterized with regard to their actual position in such areas as public life and the professions.

**London, Bruce.** METROPOLIS AND NATION IN THAILAND. THE POLITICAL ECONOMY OF UNEVEN DEVELOPMENT *Boulder. Westview Press, 1980. 145 pp. \$20.00.* How has Bangkok, Thailand's huge primate city, been "parasitic" rather than "generative" in that country's efforts to bring about development? London attempts a comprehensive framework that combines the insights of the "traditional demographic-ecological approach" with a "political-economic approach" to urbanization in the

Third World. Empirical evidence pointing to the parasitic nature of the capital includes a chapter of demographic data and a lengthy discussion of Thai political history.

**Lustick, Ian.** ARABS IN THE JEWISH STATE: ISRAEL'S CONTROL OF A NATIONAL MINORITY. *Austin: University of Texas Press, 1980. 385 pp. Cloth, \$19.95; paper, \$10.95.* Despite their objective deprivation and communal separation, the Israeli Arabs have been politically quiescent. The absence of ethnic strife is explained by three major features of control: segmentation, the internal fragmentation of the Arab minority; dependence, the absence of autonomous centers of economic power and reliance on the Jewish economy; and cooptation, which prevents development of an independent Arab leadership.

**Marsden, Lorna R., and Edward B. Harvey.** FRAGILE FEDERATION: SOCIAL CHANGE IN CANADA. *Toronto: McGraw-Hill Ryerson, 1979. 242 pp. \$7.95.* Using the world-systems approach, social change in Canada is understood in terms of the country's relationship, first as a peripheral country in the world system, and now as a semi-peripheral country to the core capitalist nation, the United States. Developments in economic arrangements and population patterns constitute two crucial, interrelated parameters of social change in Canada, and class interests are shown to be fractured by regional and ethnic identities.

**May, Elaine Tyler.** GREAT EXPECTATIONS: MARRIAGE AND DIVORCE IN POST-VICTORIAN AMERICA. *Chicago: University of Chicago Press, 1980. 200 pp. \$15.00.* Why did the rate of marital dissolution increase dramatically from 1867 to 1929? The author analyzes over 1,000 divorce cases from New Jersey and Los Angeles and supplements her quantitative study with court testimony by divorcing couples to argue that post-Victorian society raised expectations, but was unable to fulfill them.

**Miller, Alan V.** THE GENETIC IMPERATIVE: FACT AND FANTASY IN SOCIOBIOLOGY. *Toronto. Pink Triangle Press, 1979. 107 pp. \$6.00.* A misleading title, for the book is strictly a bibliography on sociobiology published by the Canadian Gay Archives, with special emphasis on sexuality. Could be a useful guide for someone starting out in this area, but the coverage is weak: two of the most relevant journals are missing—i.e., *Social Biology* and *Behavior Genetics*.

**Montagu, Ashley.** SOCIOBIOLOGY EXAMINED. *New York: Oxford University Press, 1980. 355 pp. \$19.95.* An excellent review and critique of sociobiology by fifteen authorities from anthropology, ethology, genetics, neurobiology, psychology, and philosophy. Unlike many of the earlier attackers of Edward O. Wilson, most do not argue that biological determinism can be ignored, but rather assess the scientific validity of Wilson's claims, many of which they believe are unsound.

**Nader, Laura (ed.).** NO ACCESS TO LAW: ALTERNATIVES TO THE AMERICAN JUDICIAL SYSTEM. *New York: Academic Press, 1980. 540 pp. \$27.50.* In this book, a product of the Complaint Research Center founded by Ralph Nader's Center for the Study of Responsive Law, Nader's sister and her associates explore the problems consumers have in obtaining legal remedies for their problems, and consider the ways other institutions handle consumer complaints: the Better Business Bureau, a department store's complaint department, the California Department of Insurance, a Congressional district office, a labor union, and some grassroots consumer organizations.

**National Commission on Youth.** THE TRANSITION OF YOUTH TO ADULthood: A BRIDGE TOO LONG. *Boulder: Westview Press, 1980. 228 pp. Cloth, \$24.95; paper, \$11.00.* An excellent report of the causes and consequences (mostly the latter) of one of the central social problems of contemporary American society, i.e., the persistent and allegedly growing gap between youth and adults, based on testimony before the National Commission on Youth. Of special interest are 27 recommendations for social change, many of which would certainly spark controversy if enacted.

**Nijeholt, G. Thomas-Lyckloma.** ON THE ROAD FOR WORK: MIGRATORY WORKERS ON THE EAST COAST OF THE UNITED STATES. *Hingham, Ma.: Martinus Nijhoff, 1980. 211 pp. \$16.00.* Using a fourteen-state sample on the U.S. East Coast, this study combines statistical data analysis with ethnographic materials gained from observation studies, and provides both an account of the contemporary situation and a useful historical analysis of a group too often left out of discussions of social stratification and political economy. The author confirms what we already know about the horrors of migrant life, but also provides some needed perspective on the size of the problem.

**Norton, Augustus, and Martin Greenberg.** INTERNATIONAL TERRORISM: AN ANNOTATED BIBLIOGRAPHY AND RESEARCH GUIDE. *Boulder: Westview Press, 1980. 218 pp. \$20.00.* The items included here cover a wide range of issues more or less central to the study of terrorism, including philosophical and ideological foundations of terrorism, legal issues, anarchist and nihilist movements, tactics, and nuclear terrorism. In addition to these general categories, items have also been arranged geographically. There is also a section on terrorist fiction. Although entries are not cross-listed, the bibliography should still prove a useful guide to a disparate and fast-growing literature.

**Oldroyd, D. R.** DARWINIAN IMPACTS: AN INTRODUCTION TO THE DARWINIAN REVOLUTION. *Atlantic Highlands: Humanities Press, 1980. 398 pp. \$12.75.* In addition to an account of Darwinian thought and of the general biological theorizing in progress during the eighteenth and nineteenth centuries, sociologists will also enjoy the author's analysis of the impact of biology on social thought. In addition to Spencer and the Social Darwinists, Darwin influenced such areas as politics, philosophy, psychology, anthropology, literature, and music. A useful addition to the literature on the history of ideas.

**Pearson, David.** JOHNSONVILLE: CONTINUITY AND CHANGE IN A NEW ZEALAND TOWNSHIP. *Winchester, Ma.: Allen & Unwin, 1980. 204 pp. Cloth, \$21.00; paper, \$11.50.* Essentially a sociological history, this study of Johnsonville (a suburb of Wellington) traces the growth of the community from 19th-century bush clearing to contemporary suburbia. The author also catalogs the social class and political changes which accompanied this development, and suggests that increasing social mobility and affluence have had less effect on the old class divisions than many New Zealanders claim.

**Peigh, Terry D., Martin J. Maloney, Robert C. Higgins, and Donald J. Bogue.** THE USE OF RADIO IN SOCIAL DEVELOPMENT. *Chicago: University of Chicago Press, 1979. 172 pp. \$3.75.* Nine essays intended to provide practical help for those wishing to use radio to promote social development. In an introductory chapter, Bogue acknow-

ledges radio's limitations, but says the purpose of the volume is to serve as a manual to help broadcasters reach "the least educated and most traditionally minded segments of the audience." Most of the essays deal with the scheduling, writing, and production of educational broadcasts.

**Rejai, Mostafa (with Kay Phillips).** LEADERS OF REVOLUTION. *Beverly Hills: Sage, 1979. 245 pp. Cloth, \$18.00; paper \$8.95.* Examining twelve revolutions, from England of the 1640s to France in 1968, Rejai collectively analyzes the social and psychological characteristics of 64 revolutionary leaders. Results from this quantitative analysis, together with a critical review of the psychoanalytic, psychohistorical, and sociological approaches to this issue, provide the basis for a new situational theory of revolutionary leadership.

**Roberts, Elizabeth J. (ed.).** CHILDHOOD SEXUAL LEARNING. THE UNWRITTEN CURRICULUM. *Cambridge Ballinger, 1980. 304 pp. \$18.50.* Researchers from fields as diverse as social welfare, urban design, telecommunications, and business examine the social, cultural, and institutional influences on childhood sexuality. They evaluate the effects of television, schools, community service providers, peers, legal sanctions, religious norms and practices, and health and work policies on childhood sexual learning. They call for reconsidering the assumptions about sexuality that underlie our institutional policies and practices.

**Sheridan, Alan.** MICHEL FOUCAULT: THE WILL TO TRUTH. *New York: Methuen, 1980. 244 pp. \$8.95.* This first full-length study of Foucault should help greatly in easing students into the work of a major social analyst. Although readers will have to work hard, it will prove well worth the effort. The author (translator of most of Foucault's writings) has also provided a substantial bibliography of and about Foucault, together with a biographical sketch.

**Stockwell, Edward Grant, and Karen Anne Laidlaw.** THIRD WORLD DEVELOPMENT. PROBLEMS AND PROSPECTS. *Chicago: Nelson-Hall, 1981. 352 pp. Cloth, \$21.95; paper, \$10.95.* Provides a background for understanding barriers to economic development in the poorer regions of Asia, Africa, and Latin America. The authors pay some attention to the economic and demographic aspects of these problems: A discussion of sociocultural traits of Third World countries (e.g., religious tradition and social structure) forms the core of the book; the authors contend that these traits inhibit economic development.

**Studer, Kenneth E., and Daryl E. Chubin.** THE CANCER MISSION. SOCIAL CONTEXTS OF BIOMEDICAL RESEARCH. *Beverly Hills: Sage, 1980. 319 pp. Cloth, \$18.00, paper, \$8.95.* Develops a framework for conceptualizing growth and change in biomedical science. After an initial case study of cell transformation research, the authors address one "problem domain" in cancer virology. They seek to restore knowledge to a central place in the study of scientific growth, and explore the possibility that social and intellectual structures of research may not be the same across all science or for all time.

**Sundt, Eilert.** ON MARRIAGE IN NORWAY. *New York: Cambridge University Press, 1980. 168 pp. \$29.50.* This translation of a major work by the nineteenth-century Norwegian pioneer of sociology represents an important contribution to our understanding of the empirical and theoretical development of sociology. Sundt

shows that the sharp increase in the number of marriages in Norway during the 1840s was not due to growing recklessness on the part of the lower classes and earlier marriages, but to a "baby boom" in the decade after 1815.

**Trullinger, James W.** VILLAGE AT WAR: AN ACCOUNT OF REVOLUTION IN VIETNAM. *New York: Longman, 1980. 235 pp. \$7.50.* Based on firsthand observation and research in the village of My Thuy Phuong undertaken in 1974–75, this study provides an excellent picture of both the structural and day-to-day responses of Vietnamese villages to the war. As a sociological document it contributes to our knowledge of societal responses to foreign occupation; as a human document it is harrowing.

**Witte, John F.** DEMOCRACY, AUTHORITY, AND ALIENATION IN WORK: WORKER'S PARTICIPATION IN AN AMERICAN CORPORATION. *Chicago: University of Chicago Press, 1980. 216 pp. \$20.00.* A report of a participant-observation study of the first three years of an attempt to introduce industrial democracy into the subsidiary company of a major corporation. The author is less sanguine than at the beginning, but still hopes meaningful democracy can be developed. At the same time, he provides a hardheaded analysis of the central issues of power and control. A useful contribution to a literature not generally noted for its conceptual clarity.

# Correspondence

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I have not answered a review before, and do not believe it should be a general practice.

Yet, when a reviewer grossly misrepresents a book, as Dean MacCannell has *Opening and Closing* (*Social Forces*, June 1980), it seems to me that the author should be allowed a few words to try to set the picture straight.

1. MacCannell claims the book is not theory, but he ignores the paradigm of opening and closing repeatedly diagrammed (20, 106, 157).
2. Having ignored the paradigm, he misunderstands my argument that, as an information and boundary strategy, closing is not all bad but is *on a par with opening*. He says, rather, that I am "consistently retreatist."
3. How MacCannell has found my book "decrying" Archie Bunker and "loving" *Sesame Street* is beyond me. They are mentioned only incidentally, and I hold no brief for or against either (illustrative statements: "Archie Bunker, the bigot of the television series *All in the Family*," 63; "perhaps Archie represents more than the ignorance end of a scale of which the opposite is knowledge," 17; "the main issue was not quality but 'foreignness' of programs felt as threat to national culture. An example was the excellent American television series, *Sesame Street*," 45).
4. MacCannell says the book "denigrates reading skills and language." This is fancifully gratuitous, for no paragraph treats that topic; and I have the highest respect for reading skills.
5. Finally, he says the book is a "trumpet call for...retreat from the study of modern sociocultural systems." Not so. It offers at least thirty-seven hypotheses for study of those systems.

These statements by the reviewer about *Opening and Closing* are simply not true, and I urge readers who are interested to look at the book and judge for themselves.

ORRIN E. KLAPP  
*University of Western Ontario*

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# **Research in Service to Society**

The First Fifty Years of the Institute for Research  
in Social Science at the University of North Carolina  
by Guy Benton Johnson and Guion Griffis Johnson

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# Notes on the Authors

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## Part I

The psychological addiction to running is one research interest of Susan **Birrell**. She is also studying sport fan markets in Canada and is, additionally, working on an analysis of the work of Erving Goffman.

Lars **Björn** is engaged in an ongoing project on the history of jazz in Detroit under the auspices of the Jazz Research Institute. He is also working (with James E. Gruber) on a study of alienation among women assembly line workers in the auto industry.

Mihaly **Csikszentmihalyi** is working on several major projects: a study of the phenomenology of everyday life based on data collection by electronic pager-induced self-reports from people in all walks of life; a longitudinal followup of a sample of over 200 artists initially studied 20 years ago; and the completion of a report on the symbolic ecology of urban households to be published by Cambridge University Press and entitled *The Meaning of Things*.

Among Dale **Dannefer's** current projects are a longitudinal study of social-structural differences in the relation of work, leisure, and central life interests; and a study of the effects of social context on psychological measures of identity (in collaboration with Susan Krauss Whitbourne). He is also investigating the determinants of legal processing outcomes.

John **Kelly's** book, *Leisure*, will be released by Prentice-Hall in November 1981. He is now at work on a book on *Leisure Identities and Interactions* to be published by Allen and Unwin. His current research is on leisure roles and identities through life course transitions; and on leisure satisfactions in different social environments.

Stanley **Parker** is editor of the new international journal, *Leisure Studies*. He has just completed a book on *Work and Retirement*, and is following this with a second, now under way, on *Leisure and Work*.

The paper by Nancy **Theberge** on radical and feminist critiques of sport reflects her interest in women in sport; and problems arising from the

rationalization of North American sport. Other research interests entail the analysis of voluntarism in Canadian sport and recreational associations.

Robert **Wilson's** introductory essay is the outgrowth of longstanding interests in the sociology of work and the sociology of art. Much of his recent scholarship has been devoted to a series of seminars on social and psychological aspects of modern literature, under the auspices of the National Endowment for the Humanities.

## Part II

His paper in this issue ("Managerial Power and Tenure . . .") reflects Michael Patrick **Allen's** interest in the inheritance of wealth and the evolution of family control in the large corporation. His chief fields of research are political sociology, social stratification, and complex organizations.

David **Altheide** continues to study the nature and effects of new production, the logic of communication through the media and in releases of official information. He is now investigating TV network news coverage of the Iranian hostage situation.

In addition to his ongoing research with Whitney Pope, Nicholas **Danigelis** is studying the effects of sex and age on suicide. A major continuing project is his research on black politics; he is working toward an integration of his findings on conventional voting behavior among black Americans with data on political protest and violence—this as a step toward developing a comprehensive theory of minority group political behavior.

The dangers Walter **Gove** confronts as a mountain climber are as naught compared to those posed by theory and research in a dangerous world. (See his reply, with Michael Hughes, to Handel.) Gove has produced a number of studies of deviance, of the relation of sex and marital roles to mental health, of the effects of household crowding and alone-ness, and of crime and deterrence.

Warren **Handel** has just completed an introduction to ethnomethodology, tentatively entitled *How People Make Sense*, scheduled for 1982 publication. He is working on a study of role theory and behavior, showing how models of behavior can embrace elements of purpose and causal influence; and how the discrepancy between operational and theoretical conceptions of norms have confounded role theory. He also reports an applied project in socialization, trying to protect his son from the doctrines of the '60s until

he is old enough to reject them on his own.

Philip **Hastings**'s principal research work is for Survey Research Consultants International, Inc., of Williamstown, Massachusetts. He compiles the annual *Index to International Public Opinion*, edits the bimonthly *World Opinion Update* newsletter, and manages the New England Poll.

Robert **Hauser**'s central interest in stratification and mobility is reflected in his discussion of the Hope and Macdonald papers in this issue of *Social Forces*. To an earlier issue of *SF* he contributed "A Structural Model of the Mobility Table," 56.3(1978). An example of his contribution to the tradition of Blau, Duncan, Tyree, Sewell et al. is seen in his recently published book (with David Featherman), *Opportunity and Change*.

Dean **Hoge**'s work has also graced our pages before, as our excellent *Index* will reveal. Professor Hoge is doing research on value trends and religious trends in the United States. He recently completed a monograph on persons joining or leaving the Roman Catholic Church. He is co-director of a longitudinal study of adolescents' self-esteem.

Consonant with the interests expressed in this issue (see "The New Mobility Ratio") Keith **Hope** is writing a monograph on comparative mobility which argues for the disaggregated and a priori analysis of mobility tables (the structured model). This is to be published by Academic Press. He is also working on a new way of investigating the effects and concomitants of mobility.

Like his co-author, Walter Gove, Michael **Hughes** does research on problems of mental health and mental hospitalization. He is studying the effects of living arrangements, particularly crowding and living alone, on mental health. On other projects he is investigating connections between sexuality and mental health, between prison experiences and mental health, and the influence of television watching on social and political attitudes and attitudes toward violence.

J. David **Lewis** has been investigating the social thought of G. H. Mead and the intellectual history of early American sociology. He recently published (with R. L. Smith) *American Sociology and Pragmatism: Mead, Chicago Sociology, and Symbolic Interaction*. He is currently doing research on the sociology of Georg Simmel.

K. I. **Macdonald** ("On the Formulation of a Structural Model . . .") continues his work on the interpretation of data analysis techniques, and on the pertinence of time and information to political theory. He is also plan-

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ning work on the notion of social "trapdoor functions."

As his paper in this issue might suggest, John **Marcum** is studying various aspects of fertility by race, ethnicity, and religious identification in the United States. These studies include the effect of SES on black fertility, the relation between degree of assimilation and the fertility of Mexican Americans, and the effect of labor force participation on the fertility of Catholic wives.

In his reply to Altheide, David **Phillips** endorses the use of case studies and other modes of qualitative analysis (but barring participant observation in the instances he is investigating). On the other hand he points out that the magnitude of the quantitative evidence adduced in his studies of suicide, supposed suicide, and homicide lead to plausible inferences hitherto unremarked and altogether startling.

While continuing his work with Nick Danigelis on suicide, Whitney **Pope** is, additionally, working on analyses of major theorists, particularly Weber, Durkheim, and Tocqueville. His major current project is a book tentatively titled *Tocqueville as a Comparative Social and Political Theorist*. Over the longer haul, Pope hopes to do work combining his interests in sociology and United States history, giving special attention to the relationship between state and society.

Ann **Tickamyer** is working on a number of projects touching aspects of power and inequality. These include research on the relationship between financial interests and legislative decision-making; and, with Janet Boke-meier and Jon Shepard a study comparing women and men administrators.

As his presidential address indicates, Irving **Webber's** apprehensions about parochialism in U.S. sociology grew out of his experiences with students and colleagues in other countries. He is coordinator of health policy research at the University of Alabama.

Andrew **Weigert** works at the development of a sociological psychology as exemplified in his book, *Sociology of Everyday Life*, and in a forthcoming text in social psychology. He has a continuing interest in and is writing about the problem of identity in modern society; and the sociology of José Ortega y Gasset.

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PART I

# THE SOCIOLOGY OF LEISURE

Special Editor: **ROBERT N. WILSON**





# Introduction

ROBERT N. WILSON, *University of North Carolina at  
Chapel Hill*

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For many reasons, some of them adduced in the contributions assembled here, leisure has seldom been a focus of serious sociological attention. Leisure activities have been consigned to the sphere of the trivial or the exotic. Hence the field has been conceptually weak and empirically diffuse. In this issue, leisure is approached from several theoretical perspectives and examined in diverse empirical contexts. My aim as editor has been to seek variety of discourse in the hope of stimulating scholarly consideration of this vital arena of social life. Yet within the lively eclecticism one may detect certain unifying themes: a concern with leisure as a central, not peripheral, element in human affairs; an effort to bring the conceptual and investigative resources of sociology to bear on a too-often neglected topic; reciprocally, to ask what the study of leisured processes may offer to our understanding of what it means to be human, to live our lives alone and with others.

# The Courage To Be Leisured

ROBERT N. WILSON, *University of North Carolina at Chapel Hill*

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## ABSTRACT

*Many of man's most significant activities, the processes of creativity, invention, discovery, are ultimately based on a certain detached, playful attitude toward experience. Many of his most vital achievements, the results of these processes, could not occur in the absence of civilized reflection and purposeful daydreaming. Yet the very behavior that is essential to these activities and achievements, the behavior of being leisured, is one of the least understood and seldom analyzed of human experiences. This essay is an attempt to define certain characteristics of leisure, to explore reasons why it is such a neglected topic, and to suggest that the avowed, conscious pursuit of leisure is, especially in the highly organized societies of the modern West, a genuine test of personal courage.*

## Introduction

The late psychiatrist Eric Berne, best-known for his observations on the strategies people employ in playing the games of daily living with one another, once stated that the first problem faced by each individual is to get through twenty-four hours a day. That is, each of us must devise some terms of structure, some minimal order to instruct himself on what to do next. Berne's assertion rings true, surely, for the majority of Americans, marching to clock-time and accustomed from early years to emphasize the planned, scheduled piecing-together of their days. Yet the idea that time is a problem, or a commodity to be rationally "used," is of fairly recent origin in man's history and appears to be largely confined to advanced industrialized countries. Our conception of time, and our exquisite sensitivity to its passage, militates against leisureliness. One might almost assert that leisure with a fixed clock duration is a contradiction in terms, that it cannot be fully free or true to itself.

Closely linked to an exaggerated time-sense as an enemy of cultivated leisure, we encounter the familiar work morality that Max Weber identified as "the Protestant Ethic." Although at least some of its roots lie in Calvinist

dogma, the obligation to busy oneself is today an entirely secularized condition of the mass-productive social order. Freud said that the mark of a healthy individual was a capacity to love and to work; and indeed, work is central to the integrity of contemporary personality, a chief source of one's self-concept. The difficulty for leisure comes not from the importance attached to work as such, but rather from too-rigid definitions of work as a narrowly goal-directed enterprise, as an obligation to be performed instead of an activity to be savored. An individual who is not "working" in this limited sense of performing a specified routine for a stated time with a set product or result, is almost sure to suffer pervasive feelings of guilt. Bearing this guilt, confronting its sources, and at least partly curing oneself of the compulsive habit of busy-working: here is the all too-infrequently met challenge to one's courage.

The pressure of time and the pressure of busy-ness are abetted and underlain by the language in which we speak of ourselves and our doings. Language to a very considerable extent shapes what we can perceive and how we can think. Our language is beautifully adapted to the world of organized work, but ill-adapted to the world of unorganized leisure. We have many words, and many perceptual frames, to describe the production of goods or the rules of bureaucratic behavior. But our tongues twist or fall silent when we try to tell of contemplation or love, of writing a poem or pondering a philosophy. What Suzanne Langer calls "discursive" symbolism is exceedingly familiar and comfortable to us; this is the language of direct rational statement, of the textbook, the directive, the instructional manual. It is a superb vehicle for getting certain things done, or for pointing things out. However, our very mastery of discursive style tends to blind us from real appreciation of a significant alternative mode of discourse, one Langer terms "presentational" symbolism. Presentational discourse is geared to the reporting of whole experiences in all their richness, instead of pointing to items or aspects of experience. It is the language of art and religion. Most of us are less comfortable with this mode of seeing and speaking, and since this posture for dealing with the world is much closer to the spirit of leisure than to the ethic of work it compounds our difficulty in either analyzing or enjoying leisured behavior.

Time pressure, the work ethic, and the dominance of discursive symbolism will be discussed in some depth; they are identified as "three traps" from which we must extricate ourselves if leisure is to be possible. I shall ask questions about the social and psychological conditions or concomitants of leisure, and assert that leisure is crucial not only to artistic and scientific creativity but to the creative interpersonal spheres of friendship and love.

## The Leisure Process

Leisure is not static, is not a fixed posture. Even when, to the superficial outward observation, the person at leisure appears passive or immobile, he is engaged in a process of events. Tumultuous or calm, he is ever in motion. One of our great hazards in considering leisure is that it is so commonly thought of as a residue, an empty category of experience that is "left over" when other life-sustaining activities have been accomplished. We conventionally ask, "what is that person *doing*?" and lacking the language to describe creativity or contemplation we conclude, "nothing." But leisure is precisely the most active way of doing nothing—that is, of doing nothing that is yet apparent, of nourishing an emergence.

The idea of leisure as an alert engagement with the world, rather than a quiescent onlooking, may be exemplified by the activity of responding to art. Aesthetic enjoyment is really at the opposite pole from the kind of sleepy—and sloppy—spectatorship we so often associate with watching television, say. Attending to a work of art requires that the individual mobilize his energies and adopt an attitude of vigilant perception. I remember the psychologist Christiana Morgan's shrewd remark that one cannot read Shakespeare when he is tired, that one must be somehow "up to" the level of intensity at which great art is wrought. To truly look at a painting, to listen to music with both ears (and perhaps with Theodore Reik's "third ear," the ear that catches the motive behind the sound) to read a poem in full following of the poet's voice: all these are in effect a reaching-out for experience, the opposite of withdrawal or passivity. Suzanne Langer argues that attending to presentational symbols, the language of art, is more like having a new experience than it is like entertaining a proposition. Just so, and if it is possible to entertain a proposition with only a part of one's responsive repertory, it is impossible to have a genuinely new experience without spending the whole self.

The reasons why art demands our full involvement are clear. Art is many-layered, complex, often ambiguous, hence anything less than complete attentiveness will not do. Too, it may be asserted that the artistic transaction remains incomplete until an alert perceiver has done his part; the unlooked-at painting or the unread poem do not, in an important sense, actually exist as art. Only when the circuit of communication is closed by the perceiver's grasp, whether sure or hesitant, of the artist's intent may we say that the process of art has in fact occurred. There are three parties to the process: the creating artist, the yield of his work, objectified in the art itself, and the aesthetically responsive individual confronting the art, attuning himself more or less successfully to the artist's voice, slipping with whatever degree of fit into the artist's skin.

Of course the perception of art is only one instance, although a particularly instructive one, of the leisure process. The aesthetic response

is a useful example because it helps us begin to sort out the generic characteristics of leisure, to say what this behavioral mode is and is not. Leisure is identified as a process rather than a state; as vigorous involvement rather than enervated withdrawal; as a willing surrender of the whole self to experience rather than a parcelling-out of some one or few of the individual's faculties. These features happen to be, not at all accidentally, also the distinctive marks of other leisured activities: of play; of creativity; of philosophic or religious or scientific contemplation; of the spirit of craftsmanship; and, crucially, of the most compelling interpersonal relationships, sexual love and committed friendship. What all these prized patterns of conduct have in common is a certain openness to the world, a relaxation of a tight, defensive hold on the boundaries of the self, a bold willingness to experiment and to let things happen without a brittle adherence to specified means and ends. And as they entail what we most value as the exquisitely conscious animals we are, so they entail great risks: the "loss" of the self, the fragmentation of a hard-won personal integrity, the frustrations and depressions that often accompany slow processes, the fear of failure attendant on the courageous quest for great riches. We shy away from this wholehearted giving of ourselves to the deep currents of emotion and intellection; this, surely, is one of the reasons why genuine lovingness and rich creativity are such rare happenings.

A further hallmark of leisure is its gratuitous quality. None of the activities that exemplify the leisured attitude has to be undertaken by a given individual at a given time. One might well argue that love and friendship, innovation and meditation, are required by society at large, certainly by what we are pleased to think of as a highly civilized society. *Someone* must be capable of leisure some of the time for the total health of the community. One might even argue that a talent for entering into leisure processes is an important feature of the best-integrated, most highly cultivated individual personalities. But for the individual the talent is not central to survival, or even to the modest enjoyment of life. Leisured behavior is by definition free, not forced.

True leisure is not, perhaps unfortunately, part of the common run of modern experience; it is rather more like what A. H. Maslow described as "peak experience," the apprehension of the world at full throttle, in utter involvement. The feeling of being at a peak when one is immersed in the most excellent leisure events is uniformly reported by people who try to describe their love, or religious experience, or creative activity. Its rare occurrence in most people's lives is probably one reason we are so inarticulate about the most intense, heightened sequences of leisure. Thus we fall back on words like "mysterious," "ineffable," and maybe most honest of all, "indescribable."

If leisure is gratuitous and, at least in its superior manifestations, quite rare, it is also today a cultivated, learned, willed capacity. This prop-

erty of leisureliness as an attainment is well-expressed in Montaigne's observation that the test of a civilized man is the ability to sit alone in a room without being bored. The inwardness to which Montaigne refers, the idea of being comfortable with oneself, presumably demands a certain richness of personal resources, a cognitive and spiritual stock on which one may draw without fear of imminent depletion. But if the talent for being leisured must be learned and nourished, the learning can probably not take place in any very deliberate or mechanical fashion. Novices can no doubt be "instructed" to some extent in the arts of meditation, of spiritual concentration; but even here the teacher can offer only clues, and the patience and personal depth required of the learner insure that the mystical experience is not for everyone, or even for very many. Similarly, in art or science or love the apprentice is substantially self-taught. The would-be practitioner of creative work can only receive some cues and try to model himself on exemplary figures. And although the poet may claim that, "Nature's lay idiot, I taught thee to love," the teaching and learning of love is surely by indirection, implication, shared exploration. Love depends more on self-surrender (and self-knowledge) than on any explicit pedagogy, and this despite the solemn ministrations of our modern technicians of sex. The sex clinicians can instruct us, perhaps, in how to have intercourse, but scarcely in how to fructify joy or delight in one another.

The leisured posture, then, is to be learned, at any rate by those of us caught in the traps of timeliness, busy-work, and prosaic language. But it consists in a learning by experience, by exposure to what might be termed, to turn a worn phrase on its head, the school of soft knocks. And some not so soft—which is why courage is needed. The will and ability to explore the self is of first importance, undergirding and complementing the ability to explore that world of other people and of symbols that is both "in here" and "out there."

For self-knowledge and for the sharpened perception that lets us really see our environment of nature and human nature, stretches of privacy are essential. Solitude in our age is hard to come by, real privacy the privilege of very few. We can be and are often lonely, but it is the loneliness of David Reisman's "lonely crowd." Beset with stimuli, we can be isolated but not calmly alone. No one has expressed the need for periodic withdrawal better than the then Archbishop of Canterbury, Michael Ramsey. His wise reflections bear directly on the nature of leisure and on important obstacles to its attainment:

#### Retreats and the World Today

When pleas are made about the desirability of retreat, many characteristics of our contemporary age are alluded to, as—for instance—that it is noisy and bustling and tiring. My diagnosis would be to use the word 'over-crowded'—our age is over-crowded, and in particular the mind and heart of man are over-crowded. Each day

a man or a woman does so many things, sees so many things, hears so many things, says so many things, and the mind has crowded into it a multitude of impressions, fleeting in and out. Some are important, others are unimportant. Some are good, some are bad. Some are worth preserving, some are merely of the moment. Some are worth pausing to consider, others are worth no consideration at all. Yet the mind and the heart are so over-crowded by this rapidity of sensations that for millions of people the power is lost to distinguish, to reflect, to reject or to approve thoughtfully, rationally, conscientiously. The power is lost to do what Saint Paul calls "to approve the things that are excellent."

It follows that purely from a humanist point of view, because man is so over-crowded in heart and mind, he is frustrated from being his own best self through a constant servitude to the haphazard. It is the reign of the haphazard that is so diabolical. From a theological point of view we can put the diagnosis a bit differently and say this: that man is the slave of the temporal flux of events, sensations, experiences, and therefore he misses the realisation of himself as a child of eternity. 'There was silence in Heaven.' 'Be still, and know that I am God.' So too man being enslaved to the temporal flux, fails to be able to 'consider,' in the biblical meaning of that word. Remember the very significant and telling way in which the word "consider" is used in the Bible. 'Consider the lilies, how they grow.' And they grow in a very leisurely way and if you are to consider how they grow you must have some affinity to their leisureliness. 'When I consider the heavens, even the works of Thy hands, what is man that Thou art mindful of him?' 'O, consider this, ye that forget God.' The power to 'consider' is undermined.

Leisure cannot be enjoyed under "a constant servitude to the haphazard," since this bondage to a barrage of stimuli inhibits or even utterly forecloses the capacity to be reflective or introspective. If the individual lacks the time or the personal breathing-space to "consider" in Canterbury's significant use of that term, then his days slip away unexamined and unsavored. Thoreau, surely one of the few American writers to have understood leisure, asserted that "most men lead lives of quiet desperation"; one might say that in the age of mass media and frantic overstimulation most men and women lead lives of noisy desperation.

It will be quite obvious, I think, that the various characteristics of the leisure process I have tried to rehearse run counter to the prevalent themes of contemporary American life. This thematic conflict is present not only in what we usually designate as the world of work, but also in those reaches of "free" or unfilled time that might be *potentially* the occasion of leisure. Our hours away from the constraints of occupations, of everyday jobs, are marked either by a fatigued withdrawal, a numbed respite from the harness of the work ethic, or else by an imitation of work life, a continuation of work attitudes now applied to the "uses" of discretionary time.

I believe it is futile to seek a strict definition of work or of leisure. Our felt experience of life does not fall into neat categories. That posture of leisureliness I have described may be adopted toward much of what we conventionally label as work; indeed, when it is the work is likely to be

more joyously and energetically done. Similarly, I should maintain, as above, that our dominant tendency is to carry over the stingy husbanding of the self, and the apprehension of being hemmed in by roles, rules, and short-term schedules that marks occupational life, into the realm of supposed freedom.

When I speak of "the courage to be leisured" I mean to imply that for us today a vulgar, foreshortened version of the Protestant work ethic is the natural and comfortable stance for dealing with our activities and relationships, and that to break with this pattern demands a stubborn boldness found in only a tiny minority of lovers and saints, poets and scientists. True leisure is premised on a willingness to wrench oneself away from the soothing ambience of busy motions and tidy calendars. And on a disposition, too, to "let go," not to dole out the self in carefully measured portions, in grudging commitment and fragmented roles. As the poet Charles Olson once admonished, "let the music lead the dance, leave the self behind."

The next section will focus on the "three traps" sketched in my Introduction, in an effort to determine why leisureliness should be such a hard-won and infrequent phenomenon. •

### **Obstacles to Leisure: Three Traps**

The young child at play, or the slightly older child dreaming the long daylight fantasies of his future in the great world, is by definition leisured. Free from the compulsions of inner appetite or of a schedule imposed from without, he does what one does when one has nothing to do. Similarly, the poet has been described as an individual who, having nothing to do, finds something to do. This parallel, among many that indicate a kinship between the artist and the child, is a significant clue to the nature of playfulness and the nature of creative effort. The psychologist Robert W. White insists that children exhibit a propensity to reach out for satisfying experiences with their environment, to toy with the objects around them. He suggests the presence of what he terms "competence motivation"; this thrust toward manipulation and mastery, seen even in the crib-bound play of the infant, appears to operate in the absence of urgent biological needs. Thus White believes that we can identify a motive to be competent, to cope with the elements of experience, that drives the child toward play for its own sake. Undriven, freely chosen, gratuitous: this mode of behavior may be the child's analogue of adult leisureliness.

As an individual matures, particularly in contemporary American society, he appears to lose (or perhaps more accurately to have taken from him or smothered) the capacity for a playful approach to the world. He somehow can no longer indulge in guiltless fantasy, no longer pleasure himself with an unpressured juggling of things and symbols. Why does



this loss occur? Why does the adult shrink away from sheer playfulness? Can a childlike—*not* childish—posture of delight, curiosity, a sense of the world as wonderful (full of wonder) be sustained or if once lost regained?

St. Paul said that when he became a man he put away childish things. Must we also necessarily put away the best of childlike things, notably an openness to fresh experience and a talent for zestful wondering? It was said of Albert Einstein that the great mathematician and physicist was remarkable for the number of things he didn't understand. I think we may take this to mean that he refused the conventional scientific wisdom and dared to ask the fundamental, embarrassing questions. The genius is like the child in being bold enough to report that the emperor has no clothes.

At least three complex, interlocking sets of reasons may be adduced for the normal adult's loss of playfulness, and hence of true leisure and creative possibility, in the modern West. I shall call these the "Rational Linguistic Trap," the "Protestant Ethical Trap," and the "Time Trap." The three hazards reinforce one another in obvious and subtle ways, so much so that we may seldom be aware of being trapped; or, if we occasionally struggle and squirm, we find the bonds of steel too strong to snap.

#### THE RATIONAL LINGUISTIC TRAP

*When those who love God try to speak of Him, their words are like the tears of the blind lions searching in the desert for water (Léon Bloy).*

To speak of God, of love, of artistic creation and re-creation: all these partake of blinded tears and muted awe. Our stereotyped habits of language, overwhelmingly directed to one-dimensional labeling of phenomena and the connections among them, prevent us from leisurely perception and rounded contemplation. Ernest Schachtel has offered one of the clearest interpretations of our linguistic poverty, and of the way in which this narrowed repertory of expression forecloses our perceptual alternatives. In his extraordinarily provocative essay, "On Memory and Childhood Amnesia," he asserts that the habits of language impressed on the developing personality insure both that the concrete freshness of childhood experience will be forgotten by the adult, and that the adult will also be hard put to attain such freshness in his *present* perception of the world. This is because the categories that language imposes on perception are confining and conventionally stereotypical. We can only make our experiences meaningful to ourselves by capturing them in language, and in our culture this is of course predominantly in verbal form: most of us do not have very rich resources of gesture or pitch, color or tone. Language sets limits on perception, in that we can see only that which we have been linguistically trained to see. Whether one fully accepts the hypothesis advanced by Benjamin

Whorf—that the categories of language wholly determine the span of possible experiences—it seems clear that our language importantly bounds what we can expect to perceive and how we record that perception for ourselves and others.

Schachtel notes that our language, and hence our experiential range, becomes “schematized” in a fashion that severely restricts our modes of seeing and diminishes the richness of the physical and interpersonal world “out there.” He argues:

Adult memory reflects life as a road with occasional signposts and milestones rather than as the landscape through which this road has led. . . . The signpost is remembered, not the place, the thing, the situation to which it points. . . . So the average traveler through life remembers chiefly what the road map or the guide-book says, what he is supposed to remember because it is exactly what everybody else remembers too. In the course of later childhood, adolescence, and adult life, perception and experience themselves develop increasingly into the rubber stamps of conventional clichés. The capacity to see and feel what is there gives way to the tendency to see and feel what one expects to see and feel, which, in turn, is what one is expected to see and feel because everybody else does. Experience increasingly assumes the form of the cliché under which it will be recalled because this cliché is what conventionally is remembered by others. This is not the remembered situation itself, but the words which are customarily used to indicate this situation and the reactions which it is supposed to evoke (287–8).

This line of reasoning is very like that advanced by Roger Fry when he tries to analyze the difference between everyday perception and looking at a work of art; Fry says that the objects around us, in the interest of psychic economy, “put on a cap of invisibility,” and that “it is only when an object exists in our lives for no other purpose than to be seen that we really look at it.” Presumably, too, this stereotyped packaging of experience, this snuffing-out of wonder and freshness, is what Proust had in mind when, in *The Remembrance of Things Past*, he has Swann complain that “the flowers people bring me nowadays never seem to me real flowers.” The repetition of the experience, and its storage in memory, have deadened his capacity really to see *these* flowers (rather than bouquets of long ago) in themselves—in their flowery essence, if you will.

Schachtel goes on to assert that:

The average adult ‘knows all the answers,’ which is exactly why he will never know even a single answer. He has ceased to wonder, to discover. He knows his way around, and it is indeed a way around and around the same conventional pattern, in which everything is familiar and nothing cause for wonder. It is this adult who answers the child’s questions and, in answering, fails to answer them but instead acquaints the child with the conventional patterns of his civilization, which effectively close up the mouth and shut the wondering eye. Franz Kafka once formulated this aspect of education by saying that ‘probably all education is but two things, first, parrying of the ignorant childrens’ impetuous assault on the truth

and, second, gentle, imperceptible, step-by-step initiation of the humiliated children into the lie' (292-3).

Rather than initiating children into "the lie," we might say that adult society imposes on them a partial truth: that thin slice of the total perceptual range for which convenient stereotypes are at hand. And this fragment of possible expressions is severely weighted toward logical discourse, toward the process of cognitive pointing at isolated chunks of the environment. The intuitive faculties, the global awareness of the relaxed consciousness, are discouraged.

To Schachtel, the creative writer is that individual who acutely recognizes the discrepancy between actual experience and conventional words, and in his work feeds on the tension. It is no accident that such a writer also may serve as a model of the leisured posture. Schachtel puts it this way:

The lag, the discrepancy between experience and word is a productive force in man as long as he remains aware of it, as long as he knows and feels that his experience was in some way more than and different from what his concepts and words articulate. The awareness of this unexplored margin of experience, which may be its essential part, can turn into that productive energy which enables man to go one step closer to understanding and communicating his experience, and thus add to the scope of human insight. It is this awareness and the struggle and the ability to narrow the gap between experience and words that make the writer and the poet (296).

Schachtel does not tell us, however, how some few people, those who move on to the intensity of leisured ferment, refuse fully to adopt the stereotypes and somehow sustain their awareness of the "gap." Perhaps we shall fail to understand true leisure until we can comprehend how the child survives in the artist or scientist.

Since our culture is premised on the efficiency of the linguistic trap, Schachtel and others see the poet and dreamer as a danger to conventional, ordered society. He sees and knows too much. Recall James Joyce's dictum that the proper strategy for the artist is "exile, cunning, and silence." A French observer even advances the notion that certain of our definitions of insanity or madness are designed to "keep poetry out of the streets," to insure that official definitions of reality are not too widely questioned.

Our language, then, tends to inhibit the roaming imagination and the wholly savored experience by imposing a stereotyped reality on us. But there is something more: linguistic style in the modern West is heavily colored by a rationalistic bias. We suffer from what psychologists have termed, "left-hemisphere dominance," in that the portion of the brain controlling the analytic, scientific, deliberative functions is exalted over the portion involved in synthetic, intuitive activities. This is one of the reasons why we find prose easier to read than poetry, why we distrust our emo-

tional wisdom, why it is less demanding to pay attention to an advertising billboard than to a Jackson Pollack painting. To our great peril, because it impoverishes our life-chances and life-choices, we separate action from thought, feeling from analysis, subject from object in those persistent and tragic dualisms of the Western perceptual tradition. We seem experientially crippled, unable to reach the fusion described in Hans Speier's sage comment:

We should learn from the poet. The notion that feeling and thought are hostile to one another is true only of perverted and exaggerated feeling or of barren thought. If uncorrupted, thought and feeling are at one. All great poets are wise (133).

Another way of underlining the restrictive power of the linguistic trap is to adopt Suzanne Langer's concepts alluded to earlier. In her brilliant *Philosophy in a New Key*, Langer sets forth two primary modes of discourse. She labels these as "discursive" language and "presentational" language. Discursive symbolism is by far the more familiar to us: it is the language of most prose writing, of science, the text, the instructional manual. It is instrumental, in the double sense that it assists in getting things done, shaping our mundane actions, and also that it is preeminently a means to an end, a pointer or reference to objects and events outside itself. The purpose of this kind of language is to inform in a deliberate, rational, analytic way. It carries a message or information, makes assertions that can be challenged. At its best, this is the speech of logical argument, appearing in its purest, most convincing form in a writer like Bertrand Russell. Langer notes that the discursive mode is so pervasive in modern industrial society that we are scarcely able to generate or respond to any other. This language is clearly the commanding discourse in our educational, occupational, and governmental structure.

Presentational language, in contrast, is non-instrumental. It does not set forth an argument, does not persuade or instruct us to do something. It is the vehicle of poetry and the other arts, and to a very great extent of religion—especially of the mystical strain in contemplative devotion, as embodied in Léon Bloy's statement, or the poetry of Thomas Merton, or the novels of Par Lagerkvist. I should argue that the capacity to create and respond to presentational symbolism, really to enter the domain of art or religion, is importantly akin to the ability to play. It demands a suspension of our ordinary, stereotyped frames of reference—perhaps Coleridge's "temporary suspension of disbelief." It entails a letting-go of the self, a hospitality to the fullness of a new experience, a generous delight in something encountered for its own sweet sake without any necessary immediate points of application in conduct. All these are characteristics that the universe of presentational symbolism shares with the universe of play. Probably it would not be an overstatement to insist that

an empathic talent for presentational discourse both requires and fulfills the best of civilized leisure.

It will be clear that I have begun with the rational linguistic trap because that undergirds and interpenetrates the other two great hazards to leisure and creative processes. The language in which we talk and think about work and about time largely determines how we can perceive and enjoy them. The charged expressions we bring to the conceptualization of our experience betray how deeply ingrained, how value-laden are the injunctions to behave in certain ways and not in others. Consider only a few of the terms the linguistic trap offers to the Protestant ethical and the time traps: duty; obligation; responsibility; punctuality; output; product; deadline; appointment; useful; useless; time is money; wasting time; frittering away time; loafing; not doing anything; lazy; hardworking; what's it good for?; only an intuition; hard facts; work-fare instead of welfare; the deserving poor; effort; achievement; success; failure; I want it done yesterday; tomorrow will be too late; schedule; program; break; rest.

#### THE PROTESTANT ETHICAL TRAP

*The Puritan wanted to work in a calling; we are forced to do so. . . In Baxter's view the care for external goods should lie only on the shoulders of the 'saint like a light cloak, which can be thrown aside at any moment.' But fate decreed that the cloak should become an iron cage (Max Weber, 181).*

Max Weber, writing *The Protestant Ethic and the Spirit of Capitalism*, did not invent the work ethic, but he gave it a telling name and revealed its central significance in modern society. He traced the roots of the Western attitude toward work to their joint religious and economic sources, and offered a convincing description of the values clustered around the occupational sphere of life. It is not essential to subscribe wholeheartedly to Weber's causal thesis that a specifically Protestant posture was a precondition of capitalistic forms of economic organization; it is evident enough that an association exists, and that Weber brilliantly analyzed the accompanying values. Briefly, he traced the way in which Calvinistic ideas about virtue and predestination emerged, and how the individual's "election" to a state of grace gradually came to be signified by his works in *this* world. Although the religious background is important for understanding the emotional loading of job behavior, we now confront a secular ethic that is as powerful without God in his heaven as with Him enthroned.

The substance of the Protestant Ethic is that hard and successful work affords a tangible sign of grace and virtue, that the man who is economically dutiful and effortful in this life thereby gives evidence that he will be among God's elect in the next. So work, which historically had been a burden to be avoided if at all possible, which had been seen as

man's harsh fate—"by the sweat of thy brow shalt thou get bread"—began to be transformed into a challenging task. In this task one might, indeed must, earn a hard-won salvation. Work became at once duty and opportunity, the individual's chief business in life as his life itself became chiefly business. The good man demonstrated his goodness every day by unswerving devotion to production and accumulation. The caricatures embodying the most vulgar truth of the matter are perhaps the wonderfully named Thomas Gradgrind and Josiah Bounderby of Dickens' novel, *Hard Times*, although Weber's exemplar was the Benjamin Franklin of *Poor Richard's Almanac*. At any rate, the proposition that immersion in work was virtuous in itself, that work was as much a matter of conscience as of exigency, had significant and largely negative implications for the spheres of play and leisureliness. If one's estimate of himself, and his presumed esteem in the eyes of others, rested preponderantly on his occupational activity, then all else in life was rendered somehow secondary—trivial or frivolous at best and sinful at worst. Nonwork forms of behavior were seen to dissipate the energies of the economic athlete, to divert him from his appointed striving.

Two corollary features of the Protestant ethical trap are its ascetic overtone and its power to foster guilt. The work ethic is ascetic in that work is not to be enjoyed, but to be endured. It is fundamentally grim and serious, sober and single-minded. Work that promises too much delight of the senses or too much pleasure of the mind is almost by definition not "real work." Popular suspicion of the artist or scientist is partly rooted in the uneasy apprehension that they are in fact rewarded for doing things that are intrinsically satisfying, things they would be glad to do anyway without compensation. Another aspect of the ascetic strain is the notion that the individual at work should expend himself to the utmost, as in a crusade or pitched battle. Thus an executive of the National Aeronautics and Space Administration once boasted to me about the early age at which he had suffered his first coronary heart attack; he took this to mean that he was really giving of himself, displaying professional zeal and valor.

If work has the characteristics I have described, then it is evident that failure to work, or failure to work "hard enough," must generate substantial guilt and anxiety. And so it is that the person caught in the Protestant ethical trap will go to extraordinary lengths to pacify his conscience, to placate his super-ego, to demonstrate to himself and others that he is worthy-through-work. Work assumes a driven, over-determined quality as the individual seeks thereby to validate himself. For men especially, the occupational role has been traditionally a source—often the primary source—of personal identity. When E. Wight Bakke sought to determine the effects of the Great Depression of the 1930s on unemployed men in New Haven, he found joblessness to be even more threatening to self-esteem than to economic survival itself. A man without work did not

seem to be fully a man in his own or his family's eyes. On the other end of the scale, most rich Americans work very hard indeed. For instance, the numerous members of the Rockefeller clan, clearly affluent enough to sustain themselves for several lifetimes, all work long hours and sedulously ration their pleasures. Leisure is not the natural state of man, but rather something to be "earned" as a reward for, or surcease from, work. The man not working feels a deep compulsion to explain himself, to justify nonwork.

The abyss, the blackest doom, is failure *at work*. Willy Loman, in Arthur Miller's *Death of a Salesman*, is the prototype. There exists no more compelling case history of the pathology of the work ethic and the cult of success. Failing in his job, and lying to himself about the failure, dreaming of grandeur and finding the dream without substance, Willy ultimately sees his very identity dissolve and can no longer live.

A convincing illustration of the fact that the Protestant ethical trap is peculiar to modern Western cultures, and not a universally shared human complex, may be found in one of the difficulties of the developing and underdeveloped nations. Here it is discovered that, although economic factors are enormously significant (especially problems of capital formation, and the shortage of technologically sophisticated manpower), motivational factors are also heavily involved. Docility and habituation to factory discipline and bureaucratic discipline is not innate, but must be learned. The need to achieve occupationally is not an invariant part of the individual's temperamental equipment. For instance, the psychologist David C. McClelland, who is noted for his research on the "achievement motive"—the personality disposition to strive, to accomplish—has been employed in India and other countries to try to stimulate or teach a need for achievement. He has succeeded in encouraging this motive through the use of competitive games and other educational techniques.

The power of the work ethic to ensnare us is reinforced by two distinctive cultural orientations which have been identified by Florence Kluckhohn. These are not peculiar to America, but they have been pronounced in our society. She terms them the "doing" and "man-over-nature" orientations. "Doing" indicates the heavy value we place on *activity* as a mode of conduct, in contrast to cultures that emphasize "being" or "being-in-becoming"; that is, we stress performance (even if it may be often busy-work) rather than individual character or personal cultivation. Similarly, we conceive man to be vigorously engaged in the conquest of the natural world, to be a dominant force over nature. This penchant is sharply different from that observed in societies oriented to submission to natural forces or to the idea that man and nature should live together in harmony. There are, however, certain faint indications that the productive abundance of a very advanced industrial society, combined with worry over the spoliation and exhaustion of natural resources, is beginning to mute these value

stances. If this occurs, it may enhance values more congenial to leisured processes.

I think I need not underline the way in which the patterns of rational language and the patterns of Protestant work values buttress one another. They come together in what an English critic once called, "the American belief that all good things can be deliberately achieved." That is, we as a people have a touching faith that rational planning, ingenuity in solving problems, and intense organized effort can in some fashion enable us to dominate and encompass life. These attitudes leave little room for playfulness, receptive passivity, the slow accretion of wisdom, or generosity toward emergent inspiration. And they are further marked by an urgent, one might almost say hungry, posture toward time. Deliberate achievement must be dated; results are to be obtained by a specified year or month, hour or minute. The hound of time, and the conviction that the beast can and must be tamed, is well-expressed in a slogan of U.S. factories during the productive exertions of World War II: "The difficult we do immediately; the impossible may take a little longer."

#### THE TIME TRAP

The late poet-psychiatrist Merrill Moore began one of his sonnets with this striking line: "The noise that time makes passing by . . ." Moore, a man voracious for experience and accomplishment, felt the passage of time as a tangible threat, a malevolent force stealing away his life-chances. Perhaps most of us cannot hear the noise that time makes, but we are nevertheless exquisitely aware of its movement, watchful and alert as time slips past us. We are imprisoned, as Loren Eiseley remarked, in "man-made" time. The artificial demarcation, the splintering of life processes, is pointedly expressed in the phenomenon of "jet lag," in the fatigue and dolour that typically accompany a sudden shift in the omnipresent clock. Here a second layer of chronological artificiality is imposed on the layer our bodies have learned to live with, and the result can range from minor discomfort to what is almost a wrenching of reality, a warping of one's private space-time coordinates. As a society of clock-watchers, geared to the inexorable passage of the hours and days, the seconds and minutes, we stave off the possibility—or threat—of watching and knowing ourselves. Perhaps one reason for the familiar American complaint that "there is never enough time" is that we are exaggeratedly alert to its passage; we can seize the moment, but cannot relax our grasp and truly live in the moment. In the filling of our time to overflowing, we are in the situation De Grazia perceives: "The moment for being inwardly attentive is never allowed to come." Caught up in "that showy strife men call the world," we can absorb neither the rich fleeting moment nor the spaciousness of eternity. This hyper-reaction to time's movement is akin to the habits of Thoreau's man



who watched for the mail; Thoreau said that if we see a man constantly visiting the post office in search of letters, we can be sure that that man has not heard from himself in a long time.

Why is the time trap antithetical to play and creative processes? Primarily, I should say, because our bondage to a short-range future that is implicit in a heightened sense of time and an anxiety about its passage makes us basically unfree to be players. The dictates of the clock are obvious enough in factory discipline and in the patterning of bureaucratic and technologic organization. But time's tyranny is also present in subtler fashion throughout the space of life called "nonwork." An individual's adjustment to the demands of time-keeping constricts his play by rendering it arbitrarily interruptible, by erecting cut-off points that intervene no matter in what valuable direction his play might be leading him. Above all, his orientation to that moving hourhand in its thrust toward the next package of activity conspires with the linguistic stereotypes to make the past virtually inaccessible, to muffle it in the dead category of time that has vanished. We find it, then, very difficult to recapture and thereby reexperience the past that is the substrate of creativity. (This may be why Proust's devotion to the meaningful recovery of his personal history strikes us as at once so bizarre, so hazardous, so unusual.) We are also, in our vulnerability to what happens next, likely to dull and flatten the flavor of the immediate instant. We are seldom able to heed Blake's admonition to "catch the joy as it flies," or to believe that, in the wisdom of another poet, "who can know, as the long days go, that to live is to be happy one has found his heaven." The relaxed anticipation of unfolding events, internal or external, that is characteristic of leisurely experience, is replaced by an anxious longing for the imagined—and we think unimaginably better—future.

For all these reasons, then, I should argue that it is an act of moral courage and of psychological risk-taking to dare to be leisured in contemporary American society. Civilized playfulness is far more demanding, requires more plenteous reserves of self-knowledge and of the capacity to mobilize mind and heart, than is most of our work life. It threatens our routine mechanisms for keeping our slice of the world under control, and our often-tenuous hold on a familiar and comfortable identity. Only the strong are equal to the challenge of that peculiar kind of loafing called "leisure." The life of fantasy, contemplation, bold originality—in a word, of serious play and playful sobriety—is dangerous to seek and only intermittently possible to sustain.

## The Spirit of Play

*There is an undeniable propriety in calling all the liberal and imaginative activities of man play because they are spontaneous and not carried on under pressure of external necessity or*

*danger. . . . By play we are designating no longer what is done fruitlessly but whatever is done spontaneously and for its own sake, whether it have or not an ulterior utility. Play in this sense may be our most useful occupation. So far would a gradual adaptation to the environment be from making play obsolete that it would tend to abolish work and to make play universal (George Santayana).*

I have speculated several times about the links between the world of leisure and the world of childhood. Nowhere are these links stronger or more evident than in the sphere and spirit of play. If most adults have forgotten or relinquished the child's aptitude for poetic perception, so too have they been persuaded to renounce the child's gift for playfulness. Our culture, although superficially pleasure-loving and apparently exhibiting an almost frenzied dedication to play, is really quite uncompromising in its orientation toward this central human activity. Play is not honored for its own sake: it is either consigned to the domain of the frivolous and meaningless, or alternatively subsumed under instrumental goal-striving as a species of work or preparation for work. Thus our often-remarked devotion to sport is un-leisured in essence, being the grimly serious pursuit of victory or perfection. The child's play is construed solely as rehearsal for the solemn tasks of adulthood. (It is indeed this, but not only or most significantly.) The adult's play, when not viewed as a category of work (e.g., the role of the professional athlete), is thought of as a kind of necessary evil, a "time-out" to refresh body and mind for the real world of work.

Play may be regarded as the essence of the leisured style. An essentially playful spirit underlies creative endeavor, as is seen in the case of the poet. And it also underlies, as we shall discover, that prototype of creative questing: sexual experience. The child's play, the artist's play, the lover's play—all are alike in being truly leisured, all alike in running perilously against the grain of contemporary American culture. Playfulness is the kernel of those behaviors that are distinctively human, that represent what men and women can be and do at their best. How then may it be identified?

Johan Huizinga is perhaps the subtlest historian and philosopher of play. In his classic *Homo Ludens* he argues convincingly that the most exalted activities, from poetry and philosophy to law and government, have their origins in play. Huizinga asserts that the first defining mark of play is freedom.

. . . for the adult and responsible human being play is a function which he could equally well leave alone. Play is superfluous. The need for it is only urgent to the extent that the enjoyment of it makes it a need. Play can be deferred or suspended at any time. It is never imposed by physical necessity or moral duty. It is never a task. It is done at leisure, during 'free time.' (8).

Play is chosen, not compelled; it is freely engaged in, in the best sense gratuitous. It might be construed as the voluntary construction of reality by the human at his most human, that is, when he is least subject to

the urgencies of maintaining civil order or satisfying animal appetites. Play is thus very close to the classic ideal of elite leisure, the pursuit of those who *need* to pursue nothing except for its own sake.

I alluded earlier to Robert White's concept of "competence" and his speculation about the child's play as an unpressured attempt to exercise himself in his environment. White believes we must account for what seems to be "an intrinsic need to deal with the environment." Children exhibit a competence motivation which operates at times when other pressing internal demands and external challenges are absent. (Is this condition perhaps the child's equivalent of leisure?) White describes competence behavior as "constantly transactional," in which:

. . . stimulation and contact with the environment seem to be sought and welcomed, in which raised tension and even mild excitement seem to be cherished, and in which novelty and variety seem to be enjoyed for their own sake (328).

He goes on to describe children's behavior in playful exploration:

Typically they involve continuous chains of events which include stimulation, cognition, action, effect on the environment, new stimulation, etc. They are carried on with considerable persistence and with selective emphasis on parts of the environment which provide changing and interesting feedback in connection with effort expended. . . . Effectance motivation must be conceived to involve satisfactions—a feeling of efficacy—in transactions in which behavior has an exploratory, varying, experimental character and produces changes in the stimulus field. Having this character, the behavior leads the organism to find out how the environment can be changed and what consequences flow from these changes (329).

Observed in these terms, the child's play would appear to be almost a precise analogue of the poet's adult play, his creative juggling of words and images in the service of nothing more (nor less) than full comprehension of the inner and outer worlds.

If *freedom* is the first mark of play, this untrammelledness, this loosening of the tight bonds of the familiar means-ends relationship, has several implications. One of the most important is that the meaning of play must be sought in the activity itself, not in the motivations that may inspire it or in the "ulterior utility" that Santayana rightly claims it may or may not possess. In the language of symbolism, play is *iconic*; its meaning is immediately embodied in itself. It does not refer to something else. The iconic property of play hints at the sense in which play, and leisure, may be called "timeless." Play is freedom from both quotidian constraints and the stifling overlay of routinized experience. Play, like leisure, never *has* to occur; hence it eludes both the ticking metronome of clock-time and the enchained wariness of instrumental striving. The player has, and must have, no answer to the persistent utilitarian question, "what's it good for?" Play's entrancing charm lies precisely in the fact that it is good for nothing, that is

nothing in our traditional vocabulary; it is good only for itself, and accordingly good for everything.

True playfulness, it may be argued, is the common element in lover and saint, friend and artist. This enduring core of leisure is marked by the freedom to be bold, self-yielding, flexible. The leisured player, like the poet at his craft, enjoys the unrestrained capacity to toy with the stuff of existence, to juggle the modalities of perception and judgment, of cognition and feeling.

Clearly, the talent for acting in this fashion is quite rare. To "let go" of the clock, the schedule, the instrumental thrust of the means-ends model exposes the leisured person both to outer censure and inner fear. Thus when I speak of the courage to be leisured, I imply the courage to defy accepted ways of conducting oneself and also the courage to rely on one's inner composure. Leisureliness demands that the individual be strong enough to fight down the demons of work-shunning guilt, of comfortable routine, of the closely defended self. If you will, leisure calls for the strength to be weak and the self-control to be abandoned.

Our ineptitude as players is a key reason why our satisfaction in salient human interactions is so much less abundant than human beings have a right to expect. I refer to friendship and love, those central relationships that are so dear to us we find them nearly impossible to analyze, and of which we often dare not speak except in the language of mechanism or the argot of selfishness. Friends and lovers can be enjoyed only at leisure, and only fully savored when we approach them in the non-utilitarian, iconic posture of play. To befriend or to love is really impossible unless we are prepared to find the reward in the relationship itself, in the doing of it, to relinquish alike the selfish ego and the thought of a vulgar utility.

For the interplay of two personalities should be exactly that: a playing together that is dedicated to immediate mutual satisfaction. The satisfaction is immediate or it is nothing. If it is not discovered in the apprehension of rewarding play, then it is perverted and deferred in the interests of some ulterior goal. Our popular conception of the psychology of interpersonal relationships—and to a distressing extent our scientific conception as well—views the interplay as merely a means to the end of power or pride, in the service of a tawdry aggrandizement. Hence we speak of men and women as "actors," intent on the applause of an audience or the attainment of status. Countless books and articles are written to instruct us in the strategies of relationships, to teach us the maneuvers that are a logical consequence of seeing other persons as objects to be manipulated. But love and friendship, like creativity, demand our full attention; they cannot be properly enjoyed if a part of ourselves is constantly looking outside and beyond, toward some end that is not embodied in the interaction itself. Yeats, in one of his wisest poems, tells us where to seek the rewards of

friendship: "Think where man's glory most begins and ends, And say my glory was I had such friends."

The satisfaction is *mutual* or it is nothing. When our usual notions of relationship are not premised on the striving for external goals of money or power or pride of possession, they tend to be centered on a selfish indulgence of the needs of one party to the transaction. Unable to release ourselves to the fullness of play, we vigilantly guard the precious ego and implicitly reckon up the gains and losses of a brittle self-regard. Again this intent self-absorption is foreign to the spirit of leisured involvement. We speak of "forgetting oneself" as if this were some error, a sinful slipping out of control, when in fact self-forgetfulness is the essence of true relatedness. It is quite true that the individual has urgent personal needs for security, recognition, and sexual release, and that these goods are most readily obtained in the interpersonal context. But the meeting of each person's needs is more properly seen as a happy by-product of the relationship, as a corollary of free play and self-surrender, than as the goal of an interaction in which the other is merely a means to one's satisfactions. The situation is precisely that which long centuries of spiritual wisdom assert: only in the radical giving of the self can the self be found, only in generous yielding can meaningful gains be realized. And the present moment is all, the playing together for its own sweet sake. As Camus observed, "real generosity toward the future lies in giving all to the present."

Friendship, then, is based on the playfulness we have rehearsed. It is both freely chosen, and unbounded in its enactment. A friend is not a tool to be used but a presence to be cherished. Friendship and love, like creativity, retain always a kernel of mystery, of the inexplicable. We do not know what subtle movements of the affections, what wellings of sympathy and empathy, impel us toward one another. Yet, as in the case of art, we intuitively know that something important is happening. One might guess that a large share of that "something important" is an implicit compact to which both partners assent, and that the terms of the compact are the terms of an understood playfulness. No one *has* to become lover or friend; the relationships are in the best sense gratuitous, entered upon in entire freedom.

The second major aspect of play, its iconic property, is also a key to friendship. As we have noted, authentic relationships can only be based on intrinsic, self-contained rewards. These are arts, and, akin to the poetry, painting, and music we ordinarily think of as art, they attain their full quality only when they exist for no other purpose than to be enjoyed.

If play and leisure are distinguished by the freedom to be bold and experimental, so too is friendship. A friend or lover is just that person with whom one can dare to be wholly oneself. The relationship is so special because here at last, and perhaps only here, can we drop the masks and

posturings that mark so many of our ordinary human contacts. It is the freedom to have done with pretense, to speak or not to speak, to smile or not to smile, to be confident that one will be taken at more than face value; these are the delicious bounty of leisured intercourse. They derive from the exquisite audacity of play. Again, however, the boldness of playful intercourse demands a plenitude of courage: the partners must have the self-confidence to take every risk, and the confidence in each other to provide security, to cushion the potential terrors of self-forgetfulness and mutual exploration.

We should not draw too sharp a line between friendship and love, between comradeship and sexuality. It is perhaps best to think of sexual love as a deepening and extension of playful friendliness. Making love partakes of all the characteristics of play and leisure that we have identified. The love relationship exaggerates these and brings them to finest fruition. Leisured sexuality might indeed be defined as the precipitate of a relaxed, free, mutually giving relationship. True sexuality, like play, can only occur in an utterly free, unforced manner; in it, one seeks nothing beyond the fact of being, the process of loving. The lack of a leisured capacity is fatal to the hope of a consummated sexual partnering. To conceive the partner as merely instrumental, as a means to one's instinctual satisfaction solely or as a badge of possessive pride, is finally both irresponsible and self-defeating. If we recall the most fully realized lovers in literature, their gentleness and self-surrender, their playful merger, is surely what strikes us. Constance Chatterley and Oliver Mellors, Lara Antipova and Yurii Zhivago: how achingly they embody MacLeish's eloquent image—

For love

The leaning grasses and two lights above the sea—

The two faces of freedom—freedom *from* constraint and freedom *to* quest—are clearly integral to a ripened sexuality. And each rests on that leisured talent for leaving the self behind. A person gambles himself, courageously dares to anticipate that in yielding his individuality he will be returned a richened self. The too-closely defended and zealously guarded ego is unable to enter the realm of creative love. Robert Frost tells us that in writing a poem, "the figure is the same as for love." And so it is: creative acts of whatever order, in art as in love, call for a willingness to play, to follow the flight of hazardous processes, to surrender the self to forces beyond one's entire control. An experimental life, which is to say a vitally experienced life, is not led easily or safely.

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# Leisure Interaction and the Social Dialectic\*

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## ABSTRACT

*Leisure sociology, existing on the margins of the discipline, has contributed to theory only in occasional illustrative references. However, when the existential side of the social dialectic is incorporated in theory, then the relative freedom of leisure becomes a salient element in developing an understanding of how decision and self-definitions enter into social action. The role-identity model is proposed as a valuable approach to analyzing the structural and existential dimensions of interaction. Leisure research is employed to begin to advance theory development on roles, socialization, primary communities, the emergent character of face-to-face interaction, and identity formation through the life course. Leisure is proposed as a social space in which the dialectical balance of social interaction may be further investigated.*

The generous evaluation is that the sociological study of leisure is an "underdeveloped" area. The critical view is that it is naive or even frivolous. The most common approach is just not to notice a subject so distant from the traditionally serious concerns of a discipline still uncertain of its relevance.

Whatever the approach, the sociological study of leisure in the United States has remained on the periphery of the discipline, practiced without much style or confidence by a few who have failed to heed the common wisdom of sociological salience. Some study leisure in time carved out of other interests and responsibilities—almost in their own leisure. Others have foregone traditional appointments to accept academic or government support from institutions with a mission related to that organized segment of leisure called "recreation."

There have been attempts to analyze the shaping impacts of theoretical assumptions on leisure sociology (Kelly, b) and to base leisure study on disciplinary theory in sociology and anthropology (Cheek and Burch). Theory-building in the field has been limited to attempts to develop systematic and research-based explanations of leisure behavior (Wilson),

\*I wish to thank Norman Denzin and Robert Stebbins for helpful comments, but release them from any responsibility for this interpretation.



rather than to submit the implications of leisure sociology as contributions to the discipline.

The aim of this paper is to begin to transform the one-way flow into a dialogue. The assumption is that the state of the art in leisure sociology has now developed to the point of offering suggestions to sociological understandings of social structure and interaction as well as receiving theoretical maxims and orientation. Further, most study of leisure has been at least implicitly from a systemic or structure-functional perspective and potentially significant conflict or neo-Marxist issues have received little attention from those designing research (Kelly, b).<sup>1</sup> In contrast, the focus here will be on the relationship between Weberian interpretive approaches and leisure interaction.

## The Nature of Leisure Interaction

### DEFINITIONS

It has become a cliché that a central problem in the study of leisure is the failure to produce an accepted definition. Bennett Berger's plaint from the ancient but agenda-setting tome on *Work and Leisure* (Smigel) is still widely quoted by those who have not followed the progress toward general consensus. Although no single definition has overcome all opposition to rule the field, the elements required for definition appear to be identified. The remaining conflict is between psychologists who prefer to define leisure as a "state-of-mind" or attitude (Csikszentmihalyi, a; Neulinger) and sociologists who opt for a behavioral definition that includes dimensions of the social context (Cheek and Burch; Kando; Kelly, d).

The inadequacies of definitions that define leisure as residual unobligated time revolve around the problem of identifying time that remains after all obligations are met (Gordon and Gaitz; Kelly, h). Leisure is frequently chosen in the midst of unfulfilled expectations and obligations. It is the choice rather than its "leftover" character that begins to delineate leisure. Further, since almost any activity can be leisure or not, it is the quality rather than the form of the activity that makes it leisure.

Two dimensions are critical to most current definitions of leisure. The first is relative freedom or choice. The second is the definition of the experience for the social actor. This definition can be understood as an attitude by social psychologists or by sociologists as involvement (Gunter and Gunter), function (Dumazedier), or meaning (Kelly, d). In general, however, the sociologist insists that this definition or meaning is more than inner attitude but contains social components including the relevant social roles of the actor.

From Aristotle (de Grazia) to contemporary analysis (Kelly, a), the

perennial defining dimension of leisure has been freedom. Sociologists usually part from philosophers on the issue of the purity or absolute nature of that freedom. Kenneth Roberts has offered a brief definition of leisure as "relatively self-determined nonwork activity." Freedom from role expectations, environmental constraints, and resource limitations is seldom complete. Further, leisure as activity rather than attitude alone is socially situated—usually outside the segmented social space of work.

Two other definitional models also have relative freedom as the central dimension. Kelly (a) has consistently argued that while classic leisure may be at the high end of a freedom-constraint continuum, too much leisure significant to persons is in company with others with whom we have primary role relationships to exclude all social expectations and constraints. Much leisure, then, is "complementary" to primary roles of family and work, especially in certain periods of the life course. The Gunters have also presented a freedom-constraint continuum that emphasizes how social structure shapes leisure choices through the social organization of time. In any case, there is no disagreement that some degree of freedom or self-determination, both perceived and structural, is the prime element of leisure.

There is somewhat less agreement on the second dimension. The French pioneer of leisure sociology, Joffre Dumazedier, defined leisure as activity apart from work, family, and social obligations that is functional to the person in an industrial society. Roberts retains the differentiation from work while admitting that some find leisure in work times and places. However, the emerging consensus is to define leisure by its relative freedom and its *quality*.

Aristotle combined intrinsic meaning with freedom in his later definition in Book 8 of the *Politics*. Following this, social psychologists have stressed the experience of intrinsic satisfaction (Neulinger) or of autotelic immersion in an activity in which all external and extrinsic elements disappear (Csikszentmihalyi, a). A sociological approach to the same dimension stresses high levels of involvement in which behavioral, cognitive, and affective engagement distinguish "pure" and institutional leisure from anomic and alienated leisure (Gunter and Gunter).

In an attempt to retain the significant element of social interaction in understanding leisure, Kelly has elaborated a simple definition of leisure as "activity chosen primarily for its own sake" (h) with a typology that specifies the social complementarity of much leisure (d). Some intrinsic meaning in a leisure experience is social, an enjoyment of the relationships in an activity context. Considerable leisure combines social meaning that is essentially intrinsic ("companionship") with satisfaction in developing and enhancing relationships with an immediate role basis such as marriage, parenthood, or friendship.—Therefore, the social role aims are integrated

with the activity form, situational environment, and resource availability to make up the variables in leisure choices (Kelly, e).

The most careful presentation of this social element in leisure is found in the institutional approach of Cheek and Burch in which leisure is analyzed as having a central function in the social system. That function is to provide a context for the development of intimate relationships. Leisure, then, is found less at the margins of the social system in "leftover time" than at the center of the working out of primary social relationships. In a social ecology characterized by considerable fragmentation and dispersion, leisure becomes a necessary social space for the location, development, and enrichment of primary relationships. One implication of this analysis is that any sociology of leisure must deal with face-to-face interaction as well as institutional affiliations. Much leisure is "relational" in that its central meaning is found in a positive valuation of the relationship that accepts, implicitly or explicitly, the constraints imposed by the multifaceted role definitions and expectations involved. Leisure, then, is not "either/or" in regard to freedom and constraint or intrinsic and social meanings. Absolute purity of self-determination or intrinsic motivation is a definitional "ideal type" rather than ordinary realization.

#### LEISURE IN EVERYDAY LIFE

While leisure may be defined by the quality and orientation of the experience rather than its form or location in time and space, there is no end to the activities or contexts of modern leisure. Although a relative few may run miles each week in an exhausting and disciplined form of leisure, many more exercise their imaginations on daydreaming that may be the freest of all leisure, engage in rather aimless "fooling around" with peers, or are semi-engaged before the residential convenience center of television. City parks may be little used while family and friendship groups crowd the shopping center on Saturday. Time at home may be relatively constrained for some parents who experience considerable opportunity for leisurely interchange at the workplace. In fact, there is increasing evidence that much leisure of great importance for maintaining psychological equilibrium may be "interstitial" and found in minutes and moments interspersed through any day (Csikszentmihalyi, b).

Just how such leisure can be identified remains more of a research question than conclusion. Csikszentmihalyi's use of "beepers" to secure analysis of ongoing activity randomized by time has revealed the futility of dividing the day into blocks labeled work, maintenance, or leisure. The element of play as autotelic and largely self-contained episodes of behavior may be interspersed into even the most task-oriented situation, if only for a few seconds. When "play" is defined by its intrinsic meaning and lack of

constraint, it seems indistinguishable from leisure (Kelly, h). However, a sub-element of play definition has been its self-containment (Huizinga). The meaning of play is found within the event in a time span during which the consequential world is bracketed and the play world taken as temporarily real. Play, then, is seen not just as something children do (de Grazia), but as episodes of non-serious interaction with self-contained meaning and consequences.

Kenneth Burke proposed that humor might be used to test social structures and performances within the role-expectation context. Cheek and Burch build on this insight in their suggestion that play is not entirely self-contained but may be a way of trying out presentations without risking the loss of social position. Especially as persons move through the life course, interstitial play may be an important way of testing role transitions and age appropriateness (Rapoport and Rapoport).

This possibility takes us one step further in the sociological analysis of leisure. If leisure is neither clearly segmented from primary social roles nor from the development of ways in which roles are taken, then a concept is needed to link self-conscious action with its social context. "Role identity" is proposed as an approach to self-definition and the social identity related to social categories (McCall and Simmons). Self-definition within the context of social identity is provided by the "role identity" in which a person reflexively "thinks of himself" taking the role and acting in relation to it.

Since social identities change through the life course, role-identity self-definitions require continual adjustment and occasional revision. If leisure and play provide opportunity for developing role relationships and presentations, then some development of role-identities might be expected to take place in leisure contexts. Already several studies have provided evidence for precisely this salience of leisure in role-identities. Chad Gordon has reviewed the place of leisure in life-course identity formation. Rhona and Robert Rapoport employed case studies to analyze the contribution of leisure to family and other roles through the life course. Kelly and Masar found leisure salient to definitional shifts in the transition from student to early establishment periods. Robert Stebbins has discovered the centrality of leisure investments to concepts of selfhood among adults who have concentrated on a single leisure skill. Such evidence supports McCall and Simmons' understanding of life events as parts of reflexive sequences rather than isolated occurrences. Leisure sociology is beginning to follow a new avenue into the heart of sociology, that of sequential role-definition and development.

## Existential Theory

There have been a number of classification schemes for comparing different types of sociological theory. Studies in the sociology of knowledge have articulated the "domain assumptions" (Gouldner) or implicit "paradigms" of the scientist (Friedrichs; Ritzer). Perhaps most representative of such approaches is that of Giddens who sees Durkheim as the prime exemplar of "structure functionalism," Weber of interpretive theory, and Marx of conflict analysis. Only recently have leisure sociologists begun to deal with how such theoretical models have implicitly shaped the research issues that have dominated the field (Kelly, b). The single book that is explicitly theoretical includes interpretive elements, but rests on a functionalist view of modern society (Cheek and Burch). The other half of the conversation, the implications of leisure sociology for theory, has been given little attention.

### TWO TYPES OF THEORY

Although the differences between structure-functional theory with its assumptions of systemic unity and bias toward harmony (Gouldner) and Marxist conflict theory with its distinction between pre- and post-revolutionary societies are profound, in one sense they are alike. Both are deterministic in their models of explanation. Both begin with the assumption that social structures, whether benign or malignant, shape human behavior. Explanation is largely uni-directional from the properties of the system to behavior.<sup>2</sup> While Marxists may see fundamental conflict in the economic relations of a capitalist society that Parsons thought in essential equilibrium due to a consensus of value-orientations, both assume that social forces within a system are based on the institutional structures. Explanation is from institutional independent variables to behavioral dependent variables.

Somewhat on the fringes of sociology is existential theory. Although there are many varieties of the approach, all include a fundamentally dialectical mode of explanation. That is, whether the focus is on Weberian interpretive modes of explanation, symbolic-interaction models, or dramaturgical metaphors, there is the assumption that explanation requires accounting for the interpretive decisions of social actors. However marginal to the discipline, there has been a persistent recurrence of varieties of existential theory that insists on a significant element of self-determination in human interaction.

Without denying the pervading power of structural forces or their basis in economic control structures, existentialist theories advocate that at least some levels of interaction require introduction of interpreting actors into the model of explanation. Some variations may focus on the "small

worlds" in which persons develop lines of action (Luckmann). Goffman (a, b) and others stress the problematic presentation of self in social situations with implicit orientations. From the dramaturgical metaphor, interaction is seen as something like the role taking, interpretation, and staging control of the theatre (Brissett and Edgley). Such analysis of face-to-face interaction episodes is intriguing and almost self-authenticating when well-presented.

However, existentialist theory goes a crucial step further. Berger, Luckmann, and others argue that the social structures taken as "real" in structural models are actually constructed out of such "small world" interaction. The regularities taken as the concrete forces of the social system are in reality the relatively fragile constructs of problematic negotiation. Therefore, decision is real in its consequences. Persistence is jeopardized as well as reinforced in ongoing interaction (Luckmann). The mediated character of knowledge is accepted "as if" perception were direct and reliable (Schutz).

#### THE SOCIAL DIALECTIC

C. Wright Mills found the dialectical nature of society to be central to the sociological enterprise. Understanding the biographies of persons requires comprehension of the historical structures of their social context. Further, much of life involves the playing of roles within specific institutions with existing role definitions. However, within that context is the person whose self-images have been developed in institutional contexts, whose "vocabulary of motives" consists of learned interpretations, and yet who constructs lines of conduct within the social milieu.

There are indeed social forces, known and unknown, that shape not only behavior but also its interpretation. Socialization processes include the internalization of cultural world views of "how things really are." Nevertheless, within the interplay of forces, not entirely uni-directional, the social actor interprets and decides in ways that have consequences for both the self and for the social milieu.

#### THE SOCIAL SELF

The simplest statement is that the social self is acting as well as being acted upon. Turner's dictum is that a person may "enact" a role but not occupy it. Rather, when a person takes a role, it is an act involving evaluation as well as acceptance (Gordon). Within role contexts a person forms self-concepts which in turn influence which roles are chosen and how their portrayal is essayed. There is a dialectical relationship between the regularities of role expectations and the ongoing process of self-evaluation of the actor. In fact, many roles are relinquished as well as redefined when self-definitions change through the life course (Gordon and Gaitz). The

"me" is not only a cognitive frame of reference for decision (McCall and Simmons) but is always in process of development and redefinition.

From this perspective, attention is focused on the interaction process in its social situation as explored by Simmel (Wolff) and others. Life is "negotiation" as well as response (McCall and Simmons) and social identities are constructed as well as assigned. In this negotiation, the *role-identity* from the actor's side of the social dialectic is how the person defines the self in the role.

From this perspective, decisions are real in their consequences. Further, they rest in the situated process of self-definition and the development of role identities. Within the social dialectic, explanation of interaction requires taking account of this interpreting actor, and the persistence of "social structure" rests on the agreed-on definitions of such actors.

Through the literature developing this existential mode of sociological explanation, there have been occasional references to leisure settings and interactions. The relative freedom and episodic character of some leisure provides illustrative material for interpretive sociology. However, a systematic introduction to the possible contributions of the sociological study of leisure from the *perspective* of the cumulative work in the area has not been attempted heretofore. What follows is anything but complete or final, but is something of a prelude to a contributing sociology of leisure.

### Leisure Sociology and Existential Theory

Support for an existential approach to the study of leisure is found in the accumulating evidence that traditional structural variables do not predict differential participation even in outdoor recreation (Kelly, 1980a) and that more immediate situational factors such as available companions, schedule synchronization, and accessibility are more highly correlated to choices than social position (Kelly, e). The decision element in leisure definitions is supported by analysis of adult choices as well as by the variations in activity patterns within aggregates defined by age, sex, education, occupation, and even life course period (Rapoport and Rapoport).

Further, all persons who participate in a given activity do not do so in the same way. Variations in style or mode of engagement are related to such economic factors as cost and costly distance (Cheek and Burch), but also to the intensity and orientations of engagement (Gunter and Gunter). Groups and individuals may travel, engage in sport or an art, or attend a party in quite different ways. There are differences between the devotee of tennis with a strong skill-development orientation and the occasional weekend convivial swinger. There are differences between drinking at a countrymusic bar and at an urbane cocktail party sponsored by the sym-

phony guild. Such differences reflect the social context, but the choices and modes of interaction also reflect the role-identities being essayed by the actors.

The essential variety of leisure offers an especially apt environment for trying out identities not fully established and the nonserious consequences of play afford opportunity for risk in self-presentations. However, the significance of leisure for the development of social identities and intimate role relationships requires that analysis be sociological rather than, in a narrow sense, biographical or psychological. Leisure is situated in the confluence of social forces and decision, of institutional structures and the negotiation of identities. The preliminary analysis that follows begins with a critical intersection of structural and existential theory, that of the social role, and moves through elements of role-identity to face-to-face interaction and the relationship of spontaneity to identity-testing. Throughout, the social dialectic is found to persist even in the most singular events.

#### ROLE THEORY AND LEISURE

If the role is the "primary point of articulation between the personality of the individual and the structure of the social system" as Parsons has declared (b), then it is crucial to social theory. However, the analysis by J. A. Jackson defines two common approaches, one through Mead and others who emphasize "taking the role" and the other stressing the functional requirements of the social system. Here the social role is understood as encompassing "improvisation" of behavior (McCall and Simmons) necessary to respond to the perceived expectations associated with taking a particular position in the social system (Parsons, b). Role is a line of identity-expressing action as well as a response. While some interaction situations may be quite structured, others are not. Simmel (Wolff), Goffman (b), and others have approached the management of identity in relatively unstructured situations that lend themselves to dramatic presentation. Although one useful research approach would be to test theory by focusing on role expectations in relatively open settings such as leisure and on varieties of role-taking in bureaucracies, there would also seem to be a place in theory-building for beginning to learn more about identity-construction in leisure situations.

Popitz traces the issue to the fundamental ones of sociology, the accounting for both social normation or cohesion and for social differentiation (Jackson). On a basic level of pragmatic social interpretation, we may have certain normative expectations of a role that place parameters around the performance limits, but also expect that each actor will play the role in a unique way. In fact, in some cases we find the role redefined by the performance. There is a dialectical relationship between role norms and how roles are taken. If, then, roles are the crux of sociological explanation, it is



crucial to understand how role definitions come to differ and performances vary as well as how they are predictable.

The analysis may return to that too-familiar phrase "how each actor will play the role." Without making too much of the vocabulary, the concept of *playing* roles may lead us back to leisure. Leisure situations are not without their own role expectations as well as incorporating those persistent role-identities that each actor brings to the episode. Even in familiar groups there may be some ongoing negotiation about the presentation of identities not fully established in the definition of the presenting actor. Nevertheless, the leisure event often has a significant analytical advantage over more regular events. Insofar as it is "play," the leisure episode is to a degree self-contained. It may be analyzed as an event with at least a major component of its social meaning worked out within that time-space frame. Simmel proposes that in social play objective interests are laid aside and the interaction itself is its own meaning (Wolff).

For example, although external social identities from economic and other institutions are one factor in the negotiations of leisure groups such as a community theatre cast or a league softball game, their heterogeneity mitigates against the transfer of rigid hierarchies or status expectations in the leisure event. As the role-identities are worked out, elements of related skills, interaction modalities, and critical investments in *the event* generally take on greater salience. The rehearsal or performance, the practice or game, has its own self-contained meaning. Each "actor" or "player" is able to not only try a variety of roles in the inaugural episodes of the line of action of play preparation or a softball season, but may reshape the role-identities of others in the process.

Further, at least some of the meaning is found within the event or sequence of events. Not only in intrinsic or autotelic meaning for the individual but also in the significance of that event-bounded role-identity, there is meaning for the actor. In the immediate dialectic of the processual taking and developing of a characterization of a role, the reciprocal actions of others in the interaction provide feedback that not only continues the role-development but yields satisfaction as the role-identity is established and verified. There is, then, satisfaction not only in the theatre or sport event but in the role-identity development.

While there is not as much research supporting this approach as might be wished, two kinds are relevant. The first is the case study analysis of sequences of leisure episodes such as poker groups in which the structure of the games provides a context for the presentation and establishment of salient identities (Zurcher). Another is from research on the psychological outcomes of wilderness use in which scaled outcomes cluster in ways that suggest that there are types of wilderness users who combine culture and age-appropriate values with modes of activity within the resource and with other users that build up self-definitions that differ on

social/solitary and risk-taking dimensions (Brown and Haas). However, knowing that choices and desired outcomes vary does not tell us how self-definitions differ either entering or during the event.

#### INTERACTION AS EMERGENCE

One feature of many leisure events is that they have open-ended outcomes. In some, indeterminacy is a major feature of the activity. This is true not only in sports but also in social situations such as bars where people come to seek new relationships. In much leisure the problematic as well as the structured is integral to the experience.

In recent years, interpretive sociologists have developed several approaches to piecing together the process of interaction episodes. Norman Denzin has been developing a method of analysis of behavior specimens that employ "naturalistic indicators" to incorporate the verbal and non-verbal elements in the emergent process of an episode. These rely on interpretive observation as well as careful recording of conversation and cues. Goffman (b, c) also calls attention to nonverbal signals in his analysis of how persons announce their identities and carry out lines of action in the social drama. However, he also stresses the taken-for-granted elements in a social situation that give a kind of structure to the staging and presentation management. From a dramaturgical perspective, this taken-for-granted is the structural side of the roles being enacted. There are not only limits beyond which a role portrayal would be unrecognizable, but also expectations related to how a person with a specific social identification customarily enacts a role. For example, age-appropriateness is an assumption that provokes the "act your age" when violated.

The leisure event is especially amenable to the analysis of emergence due to its problematic outcomes. There is a certain openness in the inaugural "Why don't we take a drive?" that begins a leisure interaction episode. In games the outcome is at least partly in doubt. In the arts the quality of the product will vary. In a conversation or a walk the route and the destination of both talk and walk are open to processual development. Unlike a task-oriented production episode, the end is not prespecified. Insofar as what occurs in the process is important, then the leisure event would seem to be well-suited for analysis of just how a particular signal or sign can turn the direction of the interaction to something unexpected. Analysis of how conversations enter the special and nonserious world of play suggests that a meta-language cue that "This is play" must be given and received (Lynch). However, that meta-language communication differs in more presentational "storytelling" episodes from those in which the play is a shared construction. When the problematic and emergent is dominant, then the risk of self-investment is also greatest and may be hedged by a number of "try-out" conventions that minimize the role-identity cost of failure.

## LIFE-COURSE SOCIALIZATION AND LEISURE ENGAGEMENT

There has been a tendency to treat role socialization as rather serious business, especially by those who believe that the social system is held together by the glue of role learning. As a consequence the problematic and play elements in episodes involving role learning have been bypassed in favor of attention to the invariant generalized other (Mead). However, the emergent and nonserious character of leisure permits and even encourages experimentation.

Attention here is on the actor who makes decisions that lead to role learning rather than as a passive recipient of preordained expectations. The active side of the dialectic may be raised to greatest salience in leisure sociology. Mead himself pointed to various games of children and youth as vehicles for learning role expectations. Leisure settings such as interscholastic sports, dances, and organized summer camps have been suggested as socialization media (Coleman; Hollingshead; Lynd and Lynd; Seeley et al.). Even with the stress on anticipatory socialization into adult roles rather than into current roles through the life course, leisure interactions have been considered important to children and youth.

What is less accepted is the significance of leisure in later-life (George). A study of men and women making the transition from university student to "early establishment" roles in the economy, family, and community institutions revealed multiple ways in which leisure is adjusted to serve the ends of making a place in the post-school world. More surprising, however, was the salience of leisure identities to the working out of priorities. The process of reconciling family and leisure aims for young parents was found to be one of both conflict and integration (Kelly and Masar).

Case studies in England demonstrated ways in which leisure settings and investments change in relation to the role identities assumed appropriate and desirable through the life course (Rapoport and Rapoport). For example, following a failed marriage, leisure often becomes the primary social space in which somewhat damaged identities are altered and tried out in new associational contexts. As work and family roles rise and fall in both salience and satisfaction, leisure is a source not only of new challenges and associations, but also of additional or recovered social identities. The man of 45 returning to sport engagement is doing more than engaging in a healthful regimen or making a last stab at fleeting youth. He may well be seeking to reestablish a social identity as a physically competent person who is ready to subject his skills to the measurement of competition or other scoring. In much the same way, leisure socialization is more than taking up new and renewed activities through the life course (Kelly, c). It is a development of self-definitions of investment and competence in activities that extend and test the self. The choice of social settings, activity

contexts, environments, and, perhaps most of all, new companions at any period in the life course is an element in role transitions that are anticipated, worked through, and accomplished with varying consequences. At any time, leisure would seem to be a social space for innovation and experimentation with new role identities as well as for the recovery and redefining of old ones with minimal risk to economic or familial roles.

#### LEISURE ROLE INVESTMENTS

As long as leisure is viewed as peripheral and residual, then there seems no accounting for a significant kind of human behavior. Athletic coaches and arts instructors often assert that participation in their activity requires too much discipline and investment to be considered "leisure." They see only the relaxation and recuperative dimensions of leisure. But what about those persons who are willing to concentrate their affective and mental energy as well as physical effort in a nonwork role more than in their employment? Those who make such a commitment to the development of skill in a nonwork activity are called "amateurs" by Robert Stebbins who finds such persons on the margins between work and leisure. However, another approach to their commitment is simply to accept their investment in the nonwork role-identity. After all, is it so surprising the work roles may lose or never provide the opportunity for self-development that is found in chosen and disciplined leisure? The ball player or gambler as well as the cellist or archeologist may find that the risk, measurement, feedback, associations, and creative and expressive potential of leisure are greater than other roles. Further, a leisure investment may provide a line of action that has a beginning and a series of recognizable outcomes in its course of engagement. Nonserious in the sense of not threatening work and family roles, leisure is an opportunity for consequential action.

Some leisure episodes have their appeal in their temporary situational nature. They do not involve long-term or costly commitments. Some leisure relationships are defined as "easy in-and-out" and some events are quite separate from other roles. In fact, the meanings of leisure can hardly be analyzed without reference to the role-salience and segregation intended by the actor. In some situations, leisure's self-contained consequences can offer opportunity for trying out identities that do not become lost in the more unyielding structures of the workplace. Further, there is some evidence that leisure may provide an alternative for those whose self-investment potential is limited in the economic sphere (Spreitzer and Snyder).

There would seem to be a dialectic within leisure as well as the one between the social structure and the social actor. If the development of role identities in leisure is a major element in decisions and meaning, then what of the stress on intrinsic satisfaction said to be integral to its defini-

tion? McCall and Simmons propose that there is intrinsic satisfaction in the fulfillment of role identities. In leisure, satisfactions in the experience—physical, social, emotional, and intellectual—may well be contained within the event itself. Role identity may take second place to the sheer autotelic experience of cross-country skiing through quiet woods when the snow is soft and the weather crisp. Nevertheless, the fulfillment of being *able* to ski well enough for the experience is also part of the meaning of the complete event. What is suggested is that the meanings of leisure are not pure but mix dimensions of intrinsic satisfaction, social meaning, and identity development. What is added to previous models here is the significance of role-identity as a third dimension to the oversimple intrinsic-extrinsic division of satisfactions or motivations. Again the analysis of central leisure roles in a New Town (Kelly, d) and of forest recreation (Brown and Haas), both indicate that social and intrinsic elements are combined in the same activity. In the interpretive framework of the social actor, they may come together in the role identity of self-definition and social role.

#### FACE-TO-FACE INTERACTION AND COMMUNITY

One danger to this approach is that the structural elements of role-identity come to dominate the analysis and the processual elements are neglected. The bias toward convenience always works against research designs that focus on process and emergence rather than presumed properties of an event sequence. In leisure sociology, the increased stress on the social aspects of what had been presumed to be primarily "activity" and on the centrality of primary relationships in leisure requires examining the quality of the interaction as well as its form and environment. "Relational leisure" in which the development and expression of the intimate relationship is the primary meaning of the event (Kelly, d) turns attention to the process of interaction. Just how does the leisure episode provide for that relation-building?

One evident element is the "play" or self-containment of the episodic drama. The construction of the event and its interaction can be aimed at the process itself as well as outcomes within the episode. While there may well be a structure within the event that includes a measured outcome or *denouement* of the little play, the quality of the interaction and the process of relating are the prime meaning of the event. The reciprocal or multifaceted process containing intersecting lines of action uses the activity and environment as a setting. The face-to-face "interchange of expressivity" is itself the most direct element of the interaction (Berger and Luckmann).

There has been some analysis of how the setting is managed so that interference or discrepancies are minimized (Goffman, a). More recently the processual nature of turning conversation to play with the giving and receiving of verbal and nonverbal cues has been studied (Lynch). The

relatively fragile nature of seemingly agreed-on meanings and orientations within an episode is attested by the countless ways in which communication can be delivered or even broken off by a misread or misplaced cue. Repairing such tears in the interaction is an ongoing element of any sustained relationship. Further, cues are often misinterpreted and require either a revised replication or a shifting of the line of action to correspond with the feedback. McCall and Simmons point out that one means of directing the intersecting lines of action is by selective perception and interpretation of cues and signals. Of all the possibilities in a complex situation, actors tend to focus on those that are consistent with and reinforce the role-identities perceived as being presented and that are complementary to self-presentation. In general, the fabric of relationships remains untorn as actors select and interpret in ways consistent with the ongoing meaning of the reciprocal role-identities.

Again, the meaning of the event is dialectical. The regularities of the relationship are built out of mutual or complementary interpretations of the interaction process. In a leisure episode as contrasted from one that has imposed external goals, the process that concentrates on the relationships is most open to both direction and interpretation. In leisure we are most able to develop and enjoy the interaction with a minimum of interference from intruding institutional requirements. Cheek and Burch's proposal that leisure is the social space for intimacy would seem to be justified.

In turn, leisure may be studied processually to understand the nature of that problematic part of the social dialectic that is most open to ongoing construction and reconstruction. Not only the taken-for-granted rules and orientations, the accepted norms of the persistent role-identities, and the structure of the event itself, but the development of lines of action that build inward to the quality of the episode and its communication rather than outward to institutional goals provide a social space for assessing the problematic in interaction.

#### THE EXISTENTIAL BALANCE

The social limits on freedom in leisure become essential to beginning to understand more of the existential elements. We may extol the significance of leisure's freedom for human expression and development. However, neglect of the social structures in which leisure occurs yields a temptingly attractive but unrealistic philosophy. The carry-over of institutional roles into leisure role identities, the persistence of class-based socialization, the unremitting character of some familial and economic expectations, and the environmental limitations of the socio-ecosystem are all parts of the context of leisure. For the most part, we tend to accept such contexts and work out our relative freedom within them. For example, the sequential time-frame

of Western culture shapes how we even begin to interpret leisure possibilities in ways that are seldom evaluated.

Nevertheless, the existential side of the social dialectic does stand peculiarly revealed in much of leisure. There is a freedom of determination, however relative, in leisure choices. There is some openness in many leisure episodes related to their problematic and somewhat self-contained nature. Leisure is not segmented and unconnected to institutional roles; but it is not wholly determined. As a consequence, the decision side of the social dialectic may be most discernible in leisure. So much of life is long-range in intents that research strategies that encompass meaning are difficult to develop. The more open and episodic character of leisure suggests the possibility of following lines of action to some completion in that context without giving up the significance of role-identity development.

Too much stress on the freedom of leisure threatens to return its study to the inconsequential. A failure to look past the structural elements to the existential surrenders the opportunity to understand both sides of the social dialectic as well as truncates the problematic nature of leisure interaction. Finally, in the role-identity model the two dimensions intersect in an identifiable and analyzable frame. The role-identity model enables the sociologist to "bring freedom back in" to the social dialectic without discarding structural contexts.

### Further Implications

The aim of this paper has been to suggest how the current and potential study of leisure can contribute to the development of sociological theory. The research implications are so extensive that even an outline would be both fragmentary and presumptuous. However, two illustrations of the implications of this approach for those who are engaged in attempting to understand leisure from a planning and provision perspective might be useful.

First, the significance of the developmental approach that includes changing life-course identities is central to understanding the outcomes that resource and program users desire from the experience (Rapoport and Rapoport). For example, the salience of sexuality for youth and their leisure engagements requires that providers not assume that interest in an "activity" is somehow rarefied and unalloyed by the exploration and working out of sexual identities.

Second, the importance of social integration for those in their later years (Liang et al.) suggests that the major contribution of leisure for older persons may well be in its significance for the development and building of intimate social relationships. Both familial and friendship relationships may

require the openness of leisure for enhancement. One proposed model based on analysis of a national survey proposes that leisure interaction contributes to social integration that then is a major component of life satisfaction and emotional balance (Kelly, h). Leisure is neither peripheral nor merely a time-filler for the retired, but a central social space in which both new and continued role-identities are strengthened.

In any case, the dialectical approach is significant dialectically. Both in the investigation of leisure and the development of explanatory theory, the structural and the existential elements of social interaction are integral to understanding.

### Notes

1. The author suggests a number of potentially significant issues that might be investigated from a Marxist perspective in both socialist and capitalist societies. Among the possibilities would be the relationship of leisure to false consciousness, social control, commodity fetishism, and alienation in both pre- and post-revolutionary systems.
2. Talcott Parsons' (a) work was an attempt to integrate the structural and existential elements in social action, and some Marxist approaches to social change incorporate elements of decision. However, structural determination has been the theme dominating both modes of studying Western societies.

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# Change, Flexibility, Spontaneity, and Self-Determination in Leisure

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## ABSTRACT

*The concepts of choice, flexibility, spontaneity, and self-determination are important in understanding forms of leisure institutions and behavior. Choice is never completely free, and numerous options do not necessarily mean qualitatively different choices. Flexibility is concerned with the timing of selected courses of action. For the individual, spontaneity is the opposite of routine and pre-planning. Self-determination describes how far the individual is free to decide the quality and conditions of his leisure behavior. Dichotomized values at the societal level of rigid/flexible institutions and at the individual level of spontaneous/programmed behavior yield four types of leisure-like manifestations: stereotyped nonwork activities, free time, recreation, and "true" leisure.*

Among the many definitions of leisure a good number embody the concept of choice. To take a few examples, leisure is time to be used according to our own judgment or choice (Brightbill), it is the whole of nonwork where individuals are free to choose (Roberts), or it is simply freely chosen activity (Godbey, b). Furthermore, the importance of choice in the definition of leisure is confirmed by sample surveys in which people are asked to say what they think leisure means. In one inquiry only about a quarter of persons interviewed opted for residual definitions such as all time not actually at work or doing essential things like eating and sleeping: for three-quarters leisure was "only the time you feel free to do whatever you like" (Parker).

A second theme in understanding the meaning of leisure is *flexibility*. Although the concept of flexibility does not often figure explicitly in definitions of leisure, it is implicit in popular notions of what leisure is all about: doing things when you choose, not having to watch the clock, being able to respond at short notice to a sociable invitation. Flexibility as applied to people is mainly a matter of being willing to change in some way or adapt to the wishes of others; it can, however, apply to institutions which facili-

tate flexibility in the individual—hence, for example, systems of flexitime in employing organizations.

A third theme is *spontaneity*, which is the ability to do things of one's own accord on the spur of the moment and without advance preparation—an ingredient of many of the arguably more satisfying leisure opportunities which life offers. Spontaneity is a characteristic of individuals or of their actions, but again it has its societal counterparts, such as leisure service delivery systems which can cope with last-minute or casual as well as pre-planned patronage.

Fourth, there is the importance of the individual being able to determine not only the timing and content of his leisure but also the circumstances and conditions under which it is enjoyed—in a word, *self-determination*. Living, as we do, in societies which play a big part in shaping our knowledge, awareness, values, tastes, habits, and so on, we are never completely self-determined in our leisure behavior. But we can discern differences between societies and between individuals in the degree to which and the ways in which leisure behavior is relatively self-determined. Because leisure is essentially expressive (end) rather than instrumental (means) activity, it may reasonably be argued that, to the extent that self-determination is minimized by society through control or prohibition, leisure in any meaningful sense is made difficult if not impossible to achieve.

Having outlined four themes in the meaning and experience of leisure, I now propose to discuss these in more detail, bearing in mind that, although each concept is perhaps more easily thought of in connection with differences between individuals, there are also variations between societies which help to account for differences between individuals and for what is defined as "normal" behavior on the one hand or "deviant" on the other.

Today people in the democracies of the West live in societies which pay considerable lip service to the ideal of free choice. Positively, the virtues of freedom are extolled; negatively, the evils of totalitarianism are denounced. Competitiveness and a market-based economy ensure that a wide choice of goods and services is offered to the consumer who has the means to buy them. Theoretically (though increasingly less often in practice) there is a choice of employment for those who need to earn a living. There is a choice of television programs to watch, a choice of vacation destinations and packages—and even a choice of religions to adhere to.

The range of choice, however, is never unlimited. It is determined historically, temporally, environmentally, situationally, and socially (Anderson). A city worker cannot choose to spend a summer evening fishing in a mountain stream, nor can a tribesman choose to attend a baseball game. Also, an important distinction has to be made between *quantitative* choice and *qualitative* choice. A large number of choices which consist of

offerings differing only marginally from each other may in fact result in less of a real choice than if only two radically different options were offered. To be offered 40 flavors of ice cream is fine only if you like ice cream. To be offered a choice of a dozen TV channels all showing the same type of stereotyped program constitutes no qualitative choice. To have the opportunity of voting for several different candidates at an election means little qualitative choice if all of them stand for basically the same electoral program.

The cost of choice has also to be taken into account. Those who maintain that we have (or are at least in process of developing) a society of leisure claim that individuals are increasingly able to make choices about where to live according to criteria of their leisure interests (Dumazedier). But those who rely on work for their income will rarely find that choice of location according to leisure interests is without its costs in terms of employment opportunities narrowed, commuting distances increased, and so on. Additionally, the cost of work decisions—often forced choices—on nonwork life should not be overlooked: to move town in order to find a job or a better job may be at the expense of losing touch with old friends and of being removed from favorite leisure locations.

The second theme of flexibility is also one which our society tends to regard as a desirable general principle in individual behavior and social relationships. It is closely related to the principle of choice, but whereas choice involves the selection of one course of action from two or more, flexibility is more concerned with the timing of the selected course of action. Flexitime has now been adopted widely in public and private employing organizations but it deals only with *when* the work is to be performed, not with what kind of work or work conditions. The limited implication of flexitime for leisure is that choice of working hours or days can be made with the timing of nonwork interests in mind.

Another and more important aspect of flexibility is the attitude of mind with which the individual approaches leisure. This has been the subject of substantive research in relation to the questionable "leisure" of retirement. For many people the set of habits which constitutes a role is changed only with difficulty. To change roles easily—as, for example, from work to leisure—requires a personal quality which Havighurst calls "role flexibility." People who are unable to substitute leisure roles for the lost work role often find retirement a traumatic experience.

Spontaneity as a characteristic of behavior is often valued more highly than its opposite of being routinized or pre-planned. Perhaps it is the very scarcity of truly spontaneous episodes of behavior in industrial society that makes us cherish them when they do occur. For some individuals, deep involvement in activities which require great expenditure of time, effort, and the use of skill (whether this be "work" or "leisure") partakes of the nature of spontaneity—in Csikszentmihalyi's terms, they

enter a state of "flow": they concentrate their attention on a limited stimulus field, forget personal problems, lose their sense of time and of themselves, feel competent and in control.

By contrast, lack of human spontaneity is exemplified by an "industrial" approach to both work and leisure activities. Instead of producing a piece of work or rendering a service, one is employed for a certain period of time. Instead of playing a tennis match, one has the use of a tennis court for a prearranged period. In modern industrial society punctuality is a virtue: you turn up at your host's home at an agreed time, or you risk upsetting the plans for the meal. In some less industrialized but more leisurely societies you may arrive for a visit at any time and the meal comes along later—thus you avoid the impression that you have come just for the meal.

Although an important meaning of leisure is that it is relatively self-determined behavior, freedom to choose how to behave is never absolute in any society. However, Clawson is guilty of cultural relativism in asserting that "Individual leisure is highly ordered by a society as a whole. This is true as to the amount of leisure, its timing, and the activities in it." Societies vary in the degree to which they allow self-determination, and individuals vary in the degree to which they conform to the prevailing norms or deviate from them. The growth of leisure means that the social system tolerates more "play" or "looseness" and more cultural diversity than in previous eras. When we speak of an individual being able to choose to spend his leisure time as he wishes, we are referring to a range of socially structured options, one of which is to be left alone to "do his own thing."

As Roberts points out, many of the options that the growth of leisure creates are open to groups rather than to solitary persons. It is not so much the case that an individual determines for himself, in a kind of inner-directed (to use Riesman's term) way, how he will behave in his free time. It is much more likely that an individual will either "follow the crowd" in an other-directed way or fall in with a much smaller crowd of like-minded individuals in group activity of one kind or another.

So far I have discussed the roles of four concepts in the meaning and understanding of leisure: choice, flexibility, spontaneity, and self-determination. I want now to consider the possibility that what various combinations of these concepts produce, in different individuals and in different types of society, are what may be called *leisure-like manifestations*, or various degrees of approximation to an ideal state of leisure. In order to do this, I shall find it desirable to make a major simplifying assumption: that choice and flexibility are variables which relate primarily to societies, and that spontaneity and self-determination are variables which relate primarily to individuals. I have argued above that all four concepts are appli-

cable both to societies and to individuals, but nevertheless, some degree of primacy of relevance will help simplify the analysis I shall try to make.

In summary form, then, the possibilities are:

- \* society offers a narrow choice of activities/experiences
- \*       or a wide choice of activities/experiences
- \* society has rigid institutions
- \*       or flexible institutions
- \* the individual is programmed in his behavior
- \*       or spontaneous in his behavior
- \* the individual is other-determined
- \*       or self-determined

Theoretically this offers  $2 \times 2 \times 2 \times 2$  or 16 different possible combinations of social structure and individual behavior. But I believe that it is possible to demonstrate empirically that most of these combinations are unlikely to occur. For example, it is probable that a society with rigid institutions will offer its members a narrow rather than a wide choice of activities or experiences, and it is more likely that an individual who is spontaneous in his behavior will be self-determined rather than other-determined. The unlikely combinations may still occur, and are worthy of sociological investigation elsewhere, but they should not deter us from attempting to distinguish here the relatively dominant trends and frequent associations.

In Figure 1 four types of leisure-like manifestations are summarized which broadly represent a movement, in both social structural and individual behavioral terms, from less leisure to more leisure. This amounts to the presentation of four "ideal types" and the positing of certain directions of change which correspond to the development of society from pre-industrial through industrial to post-industrial forms.

In traditional, simpler, primitive, or pre-industrial societies there is generally no leisure in the modern sense of time set aside for the purpose and activities which are experienced more or less consciously as leisure. Certain activities, such as festivals, hunting, and market-going, may have leisure-like qualities, but the separation of life experiences and social institutions into those of work and leisure—one important characteristic of industrial society—has yet to take place. Such societies have rigid social institutions which offer the individual relatively little choice of activities. This type of society, however, tends to breed individuals who do not expect or seek choice—they are "programmed" to conform to tradition, and so the leisure-like manifestation appropriate to this type of society may be described as *stereotyped nonwork activities*.

There are two possible main directions of movement away from this type of society. One occurs when the society becomes less rigid and develops more flexible institutions, and the other direction is the encouragement

of individual or personal development toward a greater degree of self-determination. There is no logical reason why these two types of development should not occur together, but it is my contention that modern industrial societies tend to differ in the major leisure paths which they have so far taken away from stereotyped nonwork activities.

Some contemporary societies have managed to become more flexible in their institutional arrangements while remaining populated with individuals who mostly continue to show relatively little self-determination in their behavior. Other societies seem to have remained essentially traditional while tolerating and even encouraging more idiosyncratic behavior. The guiding leisure-like principles of these two types of society are respectively *recreation* and *free time*. I should perhaps add that, despite the labels in Figure 1, countries and continents as culturally diverse as America and Europe do not lend themselves to being categorized so simply as I have done. In such cases we may have to look at the subcultural level to get a more accurate picture of whether recreation or free time dominates.

A society which develops more flexible institutions tends to reflect the conviction of its influential members that government and/or business has (a) a legitimate right to concern itself with leisure provision, and (b) an obligation to provide its citizens with the widest possible choice of leisure opportunities (we need not go into the motives of social control and financial advantage which underlie these convictions). But in pursuing these

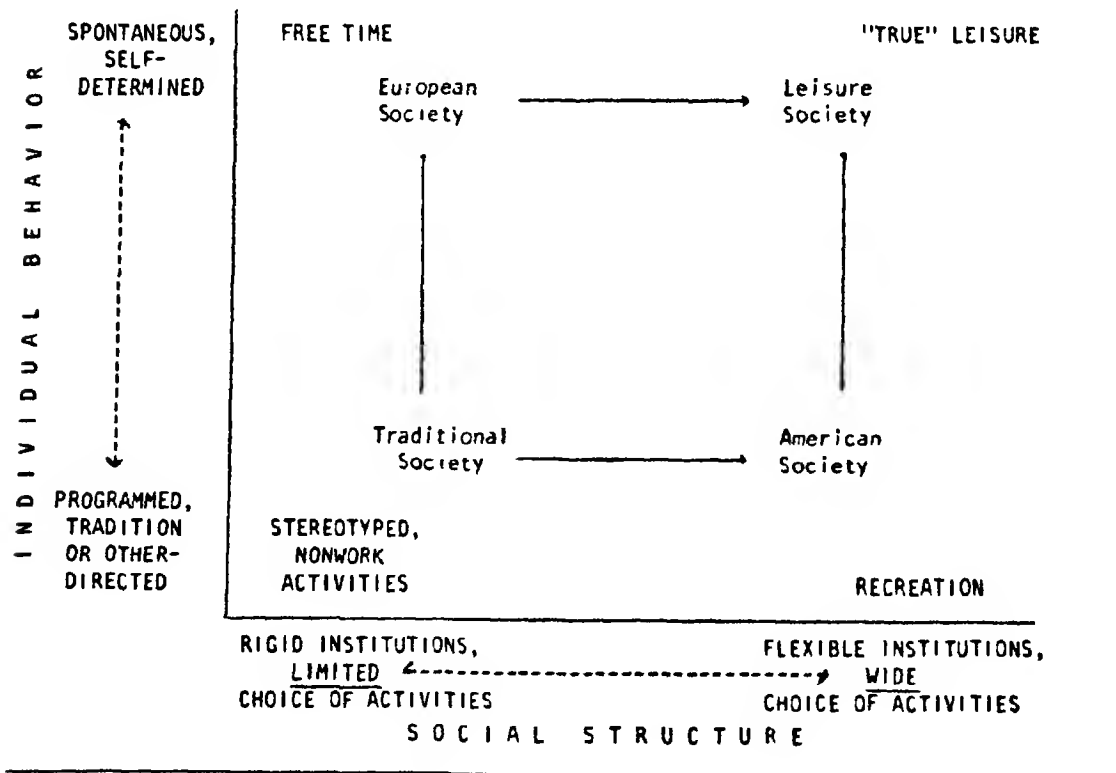


Figure 1. LEISURE-LIKE MANIFESTATIONS



twin aims society does not necessarily make any substantial change in the degree of self-determination exhibited by its citizens.

As compared with the members of pre-industrial societies, most Americans are relatively free of the confining ties of tradition—instead they are often, in Riesman's terminology, other-directed, eager to be as others wish them to be, to think and behave as others wish them to think and behave. The recreation movement is a practical expression of this social philosophy: some people are trained to become "recreation professionals," there are recognized places to go to recreate, special clothes in which to recreate, special delivery systems—including those for special populations—which are aimed to facilitate the enjoyment of the experience. All these manifestations are often graced by the term "leisure," but if the individual remains essentially other-determined and programmed rather than self-determined and spontaneous then it is questionable whether what we have is true leisure. We have instead the situation aptly described by Brehaut: "the influence of industrialization has spread itself to leisure to such an extent that leisure is no longer free time but unfree time." The concept of anti-leisure is also relevant here (Godbey, a): this refers to activity which is undertaken compulsively, as a means to an end, from a perception of necessity, with a high degree of externally imposed constraint, with considerable anxiety, with a high degree of time consciousness and a minimum of personal autonomy. In short, activity having the label of leisure while being quite remote from its essence.

This brings me to another direction in which it has been possible to move away from traditional stereotyped nonwork activities. While some societies have remained essentially traditional in institutional structure and customary behavior, they have nevertheless allowed and sometimes encouraged their citizens to have more freedom of expression. Amounts of time free from work have grown, but instead of rushing to fill such time for the masses with the paraphernalia of recreation, governments and other potential corporate providers have generally interfered little in what people do in their nonwork time. Free time activities are in some sense more "liberal" than are stereotyped nonwork activities, but they are often still accompanied by structural rigidities. This type of leisure-like manifestation is more typical of Europe than of America, although admittedly that is a sweeping generalization.

Finally we come to the tantalizing prospect of "true" leisure. To the extent that my model represents—however crudely—a temporal movement in world society toward an ideal state of leisure, then we can use it to estimate the progress being made in this direction. I agree with the spirit of what de Grazia writes about the difference between leisure and free time:

Leisure and free time live in two different worlds. We have got in the habit of thinking them the same. Anybody can have free time. Free time is a realizable aim

of democracy. Leisure is not fully realizable, and hence an ideal not alone an idea. Free time refers to a special way of calculating a special kind of time. Leisure refers to a state of being, a condition of man. . . .

However, I also believe that, as compared with a starting point of stereotyped nonwork activities, free time is on the road to leisure and is, indeed, one of its essential ingredients. Without release from burdensome labor or employment, without some degree of growth in the self-determination of the individual in how his nonwork time is to be spent, leisure is impossible.

Parallel considerations apply to the other route to "true" leisure, via recreation. It can be argued, as McCormack does, that recreation and leisure are opposite principles: "Recreation is a system of social control, and like all systems of social control, it is to some degree manipulative, coercive and indoctrinating. Leisure is not." While not disagreeing fundamentally with that proposition, I would also suggest that the contribution of recreation to the progress toward leisure should be recognized. Recreation, or more precisely the recreation movement, is the institutional component which makes approximation to the state of leisure possible. Without some degree of social organization of nonwork opportunities, without the wide and I would hope qualitative choice of activities available to the individual which that organization exists to provide, leisure is impossible.

Free time falls short of leisure because it is concerned only with the dimension of time and not with the dimension of quality of activity. With free time as a goal, individual attitudes may change in the direction of greater spontaneity and self-determination (giving rise to the collective search for identity and pursuit of cultic activity which Klapp has so acutely observed) while social institutions remain relatively unconcerned with how free time is spent. Recreation falls short of leisure because, although "leisure" institutions are introduced into the social structure, they are designed to service the needs of mostly other-determined individuals. The wide range of choice of activities which is such a superficially attractive feature of "recreation" societies masks a deeper failure to comprehend, and therefore to move further toward, the true leisure condition of man.

Radical critiques of the manifestation of leisure in contemporary Western societies point explicitly to its shortcomings in terms of authenticity and implicitly to the character of true leisure. Thus Fromm writes of the passive and alienated consumer:

He 'consumes' ball games, moving pictures, newspapers and magazines, books, lectures, national scenery, social gatherings, in the same alienated and abstractified way in which he consumes the commodities he has bought . . . Actually, he is not free to enjoy 'his' leisure; his leisure-time consumption is determined by industry . . . the customer is made to buy fun as he is made to buy dresses and shoes. The value of the fun is determined by its success on the market, not by anything which could be measured in human terms.

To the extent that this is an accurate portrayal—and I grant that the picture may be overdrawn—of contemporary individual behavior and social structure, it poses a challenge to those of us who wish to go beyond both free time and recreation toward a more truly leisured society. Remembering that leisure is an ideal to be pursued rather than a fixed state to be achieved, we may scrutinize proposals for more or "better" leisure to see how far they represent genuine advances in choice, flexibility, spontaneity, and self-determination.

There is a role for the revolutionary social critic who, ideologically at least, stands outside the society and type of behavior he is exposing. There is also a role for the more reformist free-time laymen and recreation professionals who can become more aware of the sweep of leisure history of which they are part.

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# Leisure and Socialization

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## ABSTRACT

*The conceptual dichotomy between instrumental and expressive activities confines leisure to the expressive sphere and makes it dependent on more essential instrumental institutions.*

*Ontogenetically, however, expressive experiences are prior to instrumental ones, and they serve as the criteria by which the latter are evaluated.*

*Each generation must learn that expressive experiences are available in the instrumental roles of society, or the social system is unlikely to survive. Therefore it is essential to provide socialization into expressive experience through role models who are able to enjoy their instrumental roles.* ■

Even those social scientists who are most interested in the topic take for granted the assumption that leisure activities emerge out of, and are dependent on, the productive institutions of social systems. This assumption, rarely questioned since Huizinga argued the opposite point, has been further buttressed in recent years by the tendency to equate leisure with "expressive," as opposed to "instrumental" activities (Gordon et al.; Parsons, a, b, c).

As soon as leisure is identified with expressive activities, that is, those providing immediate intrinsic rewards as opposed to delayed gratification, the logical next step is to see leisure as a nonessential, optional activity in contrast to necessary survival functions served by instrumental activities. This relationship between work and leisure is well expressed in the old fable of the grasshopper and the ant, and in popular sayings such as the following by Ben Franklin, that most eloquent apostle of the Protestant Ethic:

The honest man takes his pains and then enjoys pleasure; the knave takes pleasure and then suffers pain (Gordon et al., 316).

Although not usually expressing the relationship with such clarity, most contemporary sociologists implicitly agree with the notion that instrumental activities are functionally prior to expressive ones. Not only is leisure seen to be a superstructure built on socioeconomic foundations; it is even held that meanings or values attributed to leisure experiences are

derived from those more essential institutions. For instance, in a recent review article Wilson chides the present writer for underemphasizing "the extent to which leisure's meaning derives from our participation in the nonleisure sphere" (35).

The general agreement concerning the secondary status of leisure in relation to production is, however, based on a misunderstanding. The problem arises when the functional priority of instrumental activities (which for the sake of the present argument will be conceded), is confused with ontogenetic or phenomenological primacy. In other words, even if it were true that leisure is made possible by developments in productivity, social stratification, urbanization, and so forth (Dumazedier; Gunter and Gunter), it does not follow that individual persons learn to experience leisure activities in terms of prior instrumental experiences. Thus it is questionable that the meaning of leisure activities is determined by meanings developed in the productive sphere.

In fact, within a person's life cycle the opposite seems to be the case: experiences encountered in the context of expressive activities are ontogenetically prior, and they help to develop a standard of quality against which instrumental activities are evaluated. The first optimal experiences in each person's life include nurturance, feeding, and play; these intrinsically rewarding experiences establish a benchmark to which later events are compared. As a person matures, the most rewarding experiences still tend to occur in expressive leisure contexts such as games, sports, intimate interactions, artistic, and religious activities. These experiences provide a criterion for fulfillment that can and often does serve as a critical standard for the rest of life (Csikszentmihalyi, a, b). Moreover, while it is possible to have purely expressive activities in which instrumental concerns play no part—as in making love, listening to music, climbing a mountain—it is difficult to conceive of purely instrumental activities where a person would not be aware of how much or how little gratifying the experience is at the moment. Thus it could be argued that the most basic meaning of work and other instrumental activities is naturally determined by reference to meanings developed in leisure settings rather than vice-versa.

A good example of this process is shown in Tolstoy's novella, "The Death of Ivan Ilich." In this tale Ivan Ilich, an old bureaucrat, dying alone in his rich mansion, slowly realizes that ever since childhood his life has consisted in the pursuit of status, power, and financial advancement. Although he has succeeded in these goals, he finds that none of it had really been satisfying. The enjoyment derived from his position had been half-hearted and invidious. None of his venal friends, none of his achievements are of any help in the end. And worse than physical pain is the realization that life holds nothing worth living for. Then out of the dim memories of childhood, Ivan Ilich recaptures the memory of a summer afternoon as he ran among flowers in a meadow. It had been his last experience of pure

This refusal is manifested in many ways: the 300 percent increase in adolescent suicides over the last three decades is the most tragic sign that many teenagers find adult life not worth living (Social Indicators; Wynne). Drug abuse, vandalism, and delinquency are other symptoms of the failure of instrumental roles to attract teenagers' imagination (Csikszentmihalyi and Larson). Granted that the majority of deviant youth after a few years will turn into more or less respectable citizens (Bandura; Offer and Offer), the chances are still relatively great at this point of the life span that societal roles will be rejected.

The reasons are not too difficult to find. The disparity between expressive experiences and those that can be obtained in socially structured instrumental activities become excruciatingly obvious in adolescence. On the one hand, the rapidly maturing physical organism with its many opportunities for pleasure, sexual and otherwise; on the other hand, uninteresting jobs and the abstract routine of the classroom. There is no contest.

In some of our studies, we had groups of adolescents report the quality of their experiences for a period of one week, in response to electronic signals sent at random times of the day. The most positive feelings were consistently reported when teenagers were engaged in hobbies or sports, when they were listening to music or talking to friends. At such times they said they were significantly more happy, free, active, and strong than the rest of the day. The worst feelings were reported when they were in class, when they were working, or when they were alone; at those times teenagers rated themselves significantly more sad, constrained, passive, and weak than on the average (Csikszentmihalyi et al.). The polarization between expressive and instrumental experiences is already very clear at this age.

If adolescents are ever to grow up willingly, they must be convinced that it is possible to enjoy instrumental roles. They need adult models who can show them that being an adult is not a total loss. Unfortunately formal socializing institutions, and most "responsible" adults rarely see the necessity of stressing the expressive possibilities of adulthood. They try to interest adolescents instead in such extrinsic, instrumental advantages as grades, success, expertise, money, and material security. But a person who responds to such rewards must already be socialized into accepting symbolic goals in preference to directly rewarding experiences, and many adolescents are not. They keep looking for evidence that it is still possible to be expressive, they search for older models who can show them how to grow up without giving up what is meaningful in their lives. Thus they often fall prey to those who exploit their expectations.

### The Institutionalization of Leisure Models

The adolescent demand for expressive role models is met by a supply of individuals who are promoted to serve as symbols of expressiveness: rock singers, professional athletes, actresses, models, and so on. Adolescents live literally surrounded by icons—posters, records, and movies—which celebrate such paid purveyors of simulated expressivity. What is common to these leisure models is that they carry the message: we are not postponing gratification, we live freely for the moment, we enjoy life, and so can you. It is a message adolescents need to hear, and it seems to provide some hope in the dreariness of their existence.

The deceptiveness of this arrangement lies not in that for leisure models expressiveness is in fact an instrumental activity—for as we have argued above, the integration of work and enjoyment appears to be desirable. The present institutionalization of leisure is deceptive because all too often expressive values are in reality still subordinated to the instrumental calculus of the market place. In effect the models are paid to make teenagers feel that life has meaning, even though the models themselves usually do not believe any such thing. In past centuries, this function was performed by the clergy, and it was branded as hypocrisy. But perhaps Pareto was right, and the functional issue is not whether an institution represents truth, but whether it contributes to societal stability. On the latter count, professional expressive models might be functional in that they keep alive in adolescents a belief that life might be worth living after all.

Much more serious, and more permanently damaging in its consequences is the deception practiced by those who provide adolescents with chemical means to attain expressive experiences. Drugs allow the ultimate separation of the instrumental from the expressive, and perpetrate the fallacy that it is worth living by the latter alone. Thus they are the antithesis of many modern jobs, which are structured on the opposite fallacy, that it is possible to exclude enjoyment from experience.

Sociologists of a Marxist persuasion seem to be the only ones who have a consistent explanation for the expressive exploitation of youth in our culture (Bowles and Gintis; Larkin). But their explanation is credible only in terms of how the process works, not in terms of its causes. The difficulty of finding meaningful experiences is much too widespread to be blamed on capitalist society. It is true that Western industrial societies have their own unique problems with integrating instrumentality and expressivity—that problem is essentially polarization due to productive specialization. But one can easily find examples of cultures based on entirely different economic and political principles—from communal tribes to theocracies—in which equally severe problems exist in finding a balance be-

tween enjoyment and productivity. Certainly our contemporary examples of socialism are not immune to the dilemma. Soviet youth, which lacks outlets for its discontent except through alcohol and a pathetic need for symbols of Western individuality, is not exactly an example to be emulated. Unfortunately, the competition between enjoyment and productivity runs much deeper than the level at which economic and political changes can make a difference. As Freud correctly noted, this tension is inseparable from social life as such. Each culture, every generation, must confront it anew and find a solution appropriate to its stage of development.

One might conclude that whenever a society develops elaborate leisure institutions which do not serve other goals, and when leisure models gain high status among youth, these are symptoms indicating that instrumental roles have ceased to be satisfying. Presumably such a society is in trouble. Theoretically, there seem to be two main lines of solution. The first consists in banning all opportunities for expressive experiences, thereby depriving youth of a standard of comparison with which to assess the dreariness of their instrumental roles. This is the Maoist solution. The other is to reintroduce enjoyment into the fabric of everyday life, ending the artificial separation of work and leisure.

### **Enjoyment and Socialization**

Current theories of socialization assert that young people model adult behavior mainly because of economic and power motives. The model's attractiveness is supposed to be proportional to his or her power over resources. If youth imitate an adult it is primarily out of a desire to achieve the instrumental control which they see the adult as having (Bronfenbrenner). But this is certainly only part of the story. In a very fundamental sense, adolescents are attracted to adults whom they perceive as enjoying their lives, adults who are able to draw expressive experiences from their instrumental roles. We are not talking here about the professional leisure models from the world of sports and entertainment, but the people with whom adolescents interact at home and at school.

Teenagers respect teachers who are intrinsically motivated, who derive pleasure from the subject they teach and from the activity of teaching. They are intrigued by adults—coaches, ministers, neighbors, and relatives—who act disinterestedly, who show commitment to an idea or activity for its own sake. They admire adults who have a sense of humor, the ones who laugh with ease, the ones who show curiosity and interest. In fact, such expressive qualities might well be more important in determining which adult will become an effective model than all his or her instrumental accomplishments.

It would not be really surprising if such were the case. It makes



sense that for a growing person, it might be more important to know that life can be enjoyable than to know that one can achieve material success in it. The bottom line of one's existence is, after all, the belief that one has received the greatest rewards from life. Such rewards are, by definition, intrinsic; extrinsic ones like status and money are only potentially rewarding, useless until they can be converted into expressive experiences. Thus it is entirely functional for adolescents to be attracted to models who can demonstrate their ability to obtain the basic rewards of existence.

Genuine expressive models are essential for socialization to be effective. Without them, there is no reason for youth to want to be adults. The continuity of a culture, with its values, its institutions, and its roles, is assured only so long as each rising generation can be convinced that it is worth growing up. In this sense enjoyment is the basic "meme" (Dawkins) or unit of imitation that must be passed on if a given social system is to survive over time.

Hence the crucial importance of adult models who can socialize youth into meaningful experience, and incidentally into the productive roles on which the material existence of society depends. Important as this function is, we know very little about it. The present state of ignorance is due, at least in part, to our assumption that instrumental activities are prior and sufficient unto themselves; that expressive activities and leisure are synonymous, and that they constitute an optional, unnecessary aspect of existence.

Useful as the instrumental/expressive distinction is, it has the drawback of disguising the fact that no activity is ever entirely instrumental, and makes it easier to ignore the fact that any social system's health depends on how enjoyable its instrumental roles are.

Enjoyment is not just frosting on the cake. For a society, it is a survival requirement; in the long run, a boring system cannot last. Therefore, one of the essential parameters of any society concerns the way the opportunities for expressive experience are institutionalized. Are they segregated into leisure activities that eventually preclude enjoyment because they become ruled by instrumental goals? Are intrinsic rewards available in adult roles—in jobs, in the family, in schools, and communities? These are the questions that disclose the essential structure of a social system. It is important to know how a society produces its means of subsistence, but it might be more important to know what pleasures it can give its members.

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# A Critique of Critiques: Radical and Feminist Writings on Sport\*

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## ABSTRACT

*This paper reviews and evaluates two critiques of sport. The radical critique argues that sport is authoritarian, excessively competitive and exclusionary. The feminist critique holds that sport is male-dominated and masculine in orientation. The critiques overlap in their emphases on social inequalities and instrumentality in sport and their advocacy of change. The analysis presented here argues that the radical critique is polemical and poorly formulated. The radicals' criticisms are reanalyzed in a discussion of the secularization and rationalization of sport. The feminist critique is insufficiently developed. Feminists who call for equality in sport and a diminution of the masculine orientation fail to explicate, respectively, the meaning of equality and the institutional features of a revised model of sport.*

In recent years North American sport has increasingly come under attack. Indeed it seems that with the first major thrust of work in the sociology of sport in the mid to late 1960s, there also emerged an increasing volume of criticism of the place of sport in our society and of its structure and processes. This criticism has not abated, rather it has increased and the number of ills that have been decried have multiplied. For example, in 1968 Harry Edwards explored the problem of racism in *The Revolt of the Black Athlete*. Continuing into the seventies, in *Rip Off the Big Game* Paul Hoch provided an analysis of sport as a racist, militaristic, sexist and overly competitive support for a corrupt system of monopoly capitalism. The first textbook in the field, Edwards' *Sociology of Sport*, provided a more tempered but similarly critical view of sport. Orlick (a) and Orlick and Botterill have examined the competitive and aggressive world of children's sports and sport generally (Orlick, b). Now into the eighties, the assault on sport continues. A recent volume edited by Sabo and Runfola decries the ills of sport as a "masculine" institution.

\*This is a revised version of a paper presented at the meetings of the North American Society for the Sociology of Sport, 1980.

The references provided above are representative examples but by no means an exhaustive list of the critical works on sport. The list could be expanded greatly (e.g., Theberge). Many of the critiques fall into the general categories of radical and feminist. Although these two analyses share much in common, they have usually been presented in the literature as relatively distinct schools of thought. Radical critiques are so labeled because they argue for a reorientation of the basic structure and values of North American sport. Feminist critiques view sport as a fundamentally sexist institution that is male-dominated and masculine in orientation.

Both these perspectives offer trenchant analyses of contemporary sport and for this reason they should be recognized. At the same time, however, the writings are problematic in several respects and are, themselves, in need of a critique. This paper presents such a critique by means of a review and evaluation of the radical and feminist writings on sport. A particular purpose of the paper is to identify similarities between the critiques and to show how the insights offered by one help to clarify the issues raised by the other.

### The Radical Critique of Sport

The tenets of the radical critics have been well summarized by Christopher Lasch (a, b). Lasch argues that according to "cultural radicals" like Paul Hoch, Jack Scott and Dave Meggyesy "sport is a 'mirror reflection' of society which indoctrinates the young with the dominant values" (a, 28). These values include militarism, authoritarianism, racism, and sexism, and their inculcation perpetuates the "'false consciousness' of the masses" and prevents the achievement of revolutionary solidarity. Sport is also an agency of social repression, fostering the authoritarian interests of the dominant culture (a, 28).

Michael Novak provides a similar review in *The Joy of Sports*. The cast of characters is familiar: the critics he mentions include Dave Meggyesy, Chip Oliver, Bernie Parrish, and Jack Scott. But Novak singles out *Rip Off the Big Game* by Paul Hoch for special scrutiny. Hoch holds that sports "undergird monopoly capitalism, militarism, racism, sexism, competitiveness, sexual repression, and the counterrevolution" (Novak, 215).

It is obvious that the same themes appear in Lasch's and Novak's discussions of the radical critiques of sport: sport is militaristic, authoritarian, racist, sexist, overly competitive, and repressive. The correspondence between these two accounts is not surprising because Lasch and Novak have identified the same persons (Hoch, Scott, Oliver, and Meggyesy) as exemplars of the radical school of thought.

There are, however, other indicators that these themes are the core of current critical thinking on sport. One indicator is the contents of Read-

ers in the sociology of sport. While the works of Hoch, Scott, and others are avowedly critical and change-oriented, the motivations and thinking of those who edit Readers for academic audiences are quite different. They are concerned with presenting representative knowledge on topical issues in the field. It appears from the contents of works available that the salient topical issues are the same as those raised by the radical critics.

For example, *Sport Sociology: Contemporary Themes*, edited by Andrew Yiannakis et al. contains sections on "Sport and Politics" (a topic closely tied to militarism), "The Exploitation of Sport" (commercialism and capitalism), "Organized Sports and Kids" (competitiveness and authoritarianism), "Racial Discrimination," "Sex Role Stereotyping," "Violence" (competitiveness), and "The Competitive Ethic."

A similar message is evident in D. Stanley Eitzen's *Sport in Contemporary Society*. The section entitled "Sport as A Microcosm of Society" is especially telling, for the selections include pieces on "War, Sports and Aggression," "Football Brutality" and "Deviance." There are also sections on "Sports for Children" with several selections on competitiveness, "Sports Participation as a Builder of Character," "Sport and Politics," "The Economics of Sport," "Racism in Sport," and "Sexism in Sport." In short many of the same themes, or related themes, appear in what may be termed the popular academic literature and the writings of the radical critics.

A particular version of the radical critique locates the problems in the development of sport under capitalism. This is true of Hoch's writing, for example. Another forceful statement of this thesis is contained in Jean-Marie Brohm's *Sport: A Prison of Measured Time*. This volume is a series of previously published essays that continually drive home the evils of sport as a state controlled apparatus in the service of monopoly capitalism. The attendant evils are familiar: "All the values of the capitalist jungle are played out in sport: virility, sexual athleticism, physical dominance, the superman, muscle worship, fascistic male chauvinism, racism, sexism, etc." (15). Like the North American critics, Brohm decries the competitive ethos of modern sport, viewing this as a parallel to competition in the marketplace (see esp. pp. 65-78).

### **The Feminist Critique of Sport**

Sexism in sport is manifest in a multitude of interacting forms, both structural and cultural. Structural differences are evident in the numerous ways that women experience discrimination. The types of inequalities include funding of programs, facilities and equipment, coaching, medical and training facilities and staff, travel, number of sport activities provided, scholarships, and media coverage (see Coakley, 255-8).

The cultural manifestations of sexism in sport provide an important buttress to structural aspects. One of the first to articulate this view was Jan Felshin. Felshin characterized the social dynamic of women in sport as an anomaly. The anomaly derives from the fact that cultural conceptions of sport are masculine and women's involvement in sport and athletics is viewed as inappropriate. As an extension of this, women in sport advance an apologetic for their involvement. The apologetic affirms a woman's femininity despite her athletic endeavors and thus "legitimizes the woman's role in sport by minimizing the anomaly" (204). This legitimation is not complete, however, and social conflict over the contradictions inherent in women's sport activities persists.

A more recent volume edited by Sabo and Runfola presents a similar message. The position taken in the book is made clear in the editors' preface, wherein the first sentence affirms that "sport and masculinity are virtually synonymous in American culture" (x). That is, "a primary function of sports is the dissemination and reinforcement of such traditional American values as male superiority, competition, work, and success" (xi). In short, North American sport provides an important means of socialization into a male-dominated and success-oriented—or masculine—society.

An extension of the feminist position which is implied if not explicitly stated in the critiques is a prescription or projection of what a reconstituted, nonsexist sport would be like. Equality, of course, will prevail. In addition, the association of sport and athleticism with masculinity will be redefined, so that as an institution and cultural representation sport will be less instrumental and less competitive (e.g., Sabo and Runfola).

It is apparent that there is considerable correspondence between the radical and feminist lines of thought. One convergence concerns the common analysis that sport is overly competitive, aggressive, and instrumental. In addition, both perspectives emphasize exclusionary practices, with feminists focusing on sexism and radicals pointing to additional forms such as racism and class barriers. A final commonality is that both critiques are not simply analytical but also prescriptive. That is, they call for fundamental changes in the values and organization of sport so that it becomes more humanitarian and less discriminatory.

### **A Critique of the Radical Critique**

The first troublesome issue within the radical critique concerns competition. Most definitions of sport (e.g., Loy) treat competition as a fundamental feature. In this line of reasoning, the statement that sport is overly competitive is a contradiction in terms. In the enactment of the contest—in the pursuit of victory—sport cannot be too competitive.

The place of competition in sport is a thorny issue even among those

who are viewed as the leading radical critics. Jack Scott, for example, has written of the joys of competition and winning, and specifically argued against an ethic that replaces competition with cooperation. Such an ethic (which he terms a counterculture ethic) "misunderstands the true nature of sport." That is, "the agonistic struggle . . . is an integral part of the competitive sport experience" (b, 159).

A second feature of the radical critique deserving of scrutiny is the contention that sport is a school for life, and the life it prepares *men* for is a goal-oriented, masculine world beset by authoritarianism and aggression. This contention presents an overly simplistic analysis of the socialization functions of sport. Norms and roles are not simply internalized in the way a habit is learned. The social mold theory of human nature (Wrong; the phrase is taken from Homans, 317–9) evident in the writings of some of the radical critics of sport ignores the complexity of the process of socialization. The radicals' analysis also contradicts the available evidence, for Stevenson's review of the literature on socialization effects of sport concluded that "there is no valid evidence that participation in sport causes any verifiable socialization effects" (287).

Two of the basic principles of the radicals' critique, specifically their pronouncements concerning the role of competition and the effects of sport participation are thus contentious. Notwithstanding these problems in their analysis, it is apparent that the radicals have tapped some important ideas and offered some well-founded criticisms. They have weakened their case and sometimes damaged their credibility, however, by offering unsubstantiated claims and engaging in unrestrained polemics.

A guide to a reworking of the radical critique is contained in the writings of other more tempered observers. Particularly relevant to the discussion are works by Richard Gruneau, Allen Guttmann, and Christopher Lasch. These three writers come from somewhat different scholarly perspectives. Gruneau is a sociologist whose work concentrates heavily on issues of class and power in sport, Guttmann is a professor of American Studies, and Lasch is a social historian and sometime cultural critic. Despite these different starting points, the authors have provided interpretations of sport in our society that intersect in important ways.

Gruneau argues that one of the major subprocesses associated with the institutionalization of sport has been its rationalization. This refers to "a process whereby action pursued in an affective manner . . . becomes transformed into action that is oriented to particular ends in a secular atmosphere of efficiency and production" (22). The rationalization of sport is evident in its commercialization, bureaucratization and democratization, and in the dominance of technical efficiency as a standard of enjoyment.

The burden of much of Guttmann's writing on sport is to demonstrate that there are some distinguishing characteristics of modern sport. Guttmann identifies the following seven: secularization, equality, special-

ization, rationalization, bureaucratization, quantification, and the pursuit of records. These characteristics are the outcome of the gradual emergence of a particular *Weltanschauung* or world view that is empirical, experimental and mathematical.

Lasch's description of the characteristics of sport differ from Gruneau's and Guttmann's in that rather than providing an overview, he singles out certain aspects of the "corruption" (a) or "degradation" (b) of sports. The fundamental feature is secularization, or the subjection of sport to ulterior purposes. These purposes, or societal functions, have historically included patriotism, moral training, the pursuit of health, and profit making.

The consistent threads in the analyses of Gruneau, Guttmann, and Lasch concern the processes of rationalization and secularization. There are some differences among the authors in the logical ordering of the concepts. Gruneau views rationalization as a major process of which the other four are indicators, while Guttmann sees rationalization as one manifestation of a scientific *Weltanschauung*. Another of Guttmann's characteristics is secularization, which for Lasch is the overriding feature and which for Gruneau provides the backdrop for rationalization.

This difference in ordering conforms with typically confusing usages of these concepts (see Nisbet, 388). Historically, the processes of rationalization and secularization have been inseparable, and conceptually, they have been treated as similar by most authors. In this discussion, rationalization (following Gruneau and Guttmann) refers to goal-oriented activity in which there is a logical relationship between means and ends. Secularization (relying on Nisbet, 370) refers to "the replacement of a social order governed by religious values by one in which utilitarian or secular values are dominant." Continuing from Nisbet:

The process of secularization results in novel respect for values of utility rather than sacredness alone, control of environment rather than passive submission to it, and, in some ways most importantly, concern with man's present welfare on this earth rather than his supposed immortal relation to the gods (388).

An analysis of sport which locates its main features in the processes of rationalization and secularization provides a guide to sorting through the claims of the radical critics. Separating out the polemics issued by the critics and getting away from their social mold theory of socialization, there remain some important problems in sport which the critics have pointed to and which arise from its rationalization and secularization.

The first of these involves the very process of secularization. This means that sport is part and parcel of our everyday life. The evidence of this is clear in the ties between sport and other institutions. Textbook chapters are devoted to sport and economics/the economics of sport; sport and politics/the politics of sport; religion and sport/religion in sport. Inevi-



tably, as social problems are a part of our economic, political, and religious life, and in other ways, they are a part of sport. And more, with the secularization of sport the conflicts of interest and battle lines are clearly identified. Sport is not removed: the racial integration of baseball in the 1940s; the emergence of players' associations and unions in the 1960s and 1970s, and the boycott of the 1980 summer Olympics by many countries all are evidence of the secularization of sport. The importance of this is that we now have more demanding and explicit expectations of sport. As Nisbet says, we are more concerned with its utility and problem-solving capacities in our lives. Accordingly, when sport does not solve problems but seems to create (or at the very least reflect) them, we are troubled.

The companion process of rationalization is evident in sport in a variety of ways. Two that are particularly relevant to the writings of the radical critics are bureaucratization and commercialization. The specialization, hierarchical organization, and authority that bureaucratization entail have inevitable consequences for the suppression of personal autonomy. Weber's fear that bureaucratization would cause people to become like cogs in a machine have partially been realized in sport where, paradoxically, individual spontaneity and creativity are the very essence of performance. Alan Ingham has described this process:

In sport, bureaucratic rationality has expanded the realm of social control. The transformation of sport from player-controlled games to management-controlled big time has resulted in sport moving from an arena of informal social relationships (a *Gemeinschaft* orientation) to an arena of formal bureaucratic domination. Sport has ceased to be undertaken for its own sake and the sociability of participation. Sport is now undertaken for the organization (378).

Thus, the radical critics who decry the repressive, authoritarian, and manipulative social control in sport are correct, at least in some measure. As more rational forms of social organization have been adopted in sport, athletes have lost some of their freedom.

The critics are also correct in drawing attention to the host of evils that have been intensified or spawned by the commercialization of sport. In athletic programs which must be financially successful to survive, the effects of the pressure to win can be devastating. In the United States this is evident in the recent collegiate recruiting and eligibility scandals. It is also evident in the excessive costs of professional sports. Million dollar salaries and purses are financed in the main not directly through ticket sales but indirectly through advertising revenue and corporate accounting wizardry such as tax depreciation allowances. Whether these costs are imposed directly or indirectly, however, they are eventually borne by consumers.

The commercialization of sport is of course tied into the other features that arise from the modern condition. Thus, bureaucratization and

commercialization are closely associated. Bureaucratization, as we have seen, is a rational form of social organization and more, social control. When the pressures to win are greatest the individual athlete can become even more like a cog in a machine, treated as a commodity and retained only so long as he or she remains productive. All this is part of the alienation that is part of bureaucratization, which is part of modern life and its rationalization and secularization.

### A Critique of the Feminist Critique

What are the implications of the above for the feminist critique? It was suggested earlier that the two basic components of the feminist prescription for sport are first, gender equality and second, a shift away from a masculine orientation, so that sport becomes less instrumental. The latter in effect involves a move away from the rationalization of modern sport, and the problems resulting from its bureaucratization and commercialization. Although the goals of equality and reorientation are not necessarily antithetical, in the present situation they are likely to come into conflict. This choice is forced by the fact that the secular, rational world of modern sport, in which men and masculinity predominate, is firmly entrenched. So long as this is the case, to seek a new order is to seek separation. As we know, separate is almost always unequal. Consequently, feminists in sport face a dilemma: to seek redistribution of resources in a troubled, some would say corrupt, institution or to seek a reconstitution of that institution. Each of these approaches, however, is problematic and hence the feminist critique, like the radical critique, is flawed.

The problem with the feminist prescribed reconstitution of sport is that it is not clear what an alternative institution would look like and, were this clear, how change would be effected. Hall has written that "women's sport is rarely evaluated *sui generis*, something worthwhile in its own right as an autonomous and meaningful sphere of human experience" (2). A feminist perspective on any aspect of social life seeks to identify and explicate women's experience on its own terms, or *sui generis*. Attaining this understanding is problematic, however, because in the male and masculine world in which we live, our understanding of sport and other social experiences is male-defined and masculine. As an extension of this, our alternatives are also defined from a masculine perspective.

A guide to the problems of defining a feminist version of sport is contained in Dorothy Smith's writings on the development of a sociology for women. The issues are similar, for both sport and sociology are social activities, dominated by males and masculine imagery. As in sport, part of the problem in sociology has been an absence of interest and valuation of women's place in social life. More than this, however, the intellectual

apparatus of sociology—our vocabulary, concepts, theories, and most fundamentally, our identification of sociological issues and questions—have not addressed women's experience. Smith writes that

We had learned to practice what Rowbotham describes as a nihilistic relation to our own consciousness. As members of an intelligentsia, we had learned, furthermore, to work inside a discourse which we did not have a part in making, which was not 'ours' as women. The discourse expresses, describes and provides the working concepts and vocabulary for a landscape in which women are strangers (136).

The recent redirection of attention to bringing women back into sociology has met with difficulties because of this lack of an intellectual apparatus. Continuing from Smith:

In beginning to find out *how* and *what* to speak, we had to begin from nowhere, not knowing what it was we would have to say and what it was we would need to know how to speak. In almost every area of work, therefore, in opposing women's oppression we have had to resort to women's experience as yet unformulated and unformed; lacking means of expression; lacking symbolic forms, images, concepts, conceptual frameworks, methods of analysis; more straightforwardly, lacking self-information and self-knowledge (144).

The analogies to sport are clear. The rationalized, bureaucratized, and commercialized forms of modern sport provide the model and determine our understanding of what sport and the sport experience is and should be. This model embodies a masculine and male-oriented institution and as yet there are no clearly articulated definitions of feminist alternatives.

It would be incorrect to observe that there have been no attempts to define such alternatives. Two examples of such efforts are the writings of Sabo and Runfola mentioned earlier, and Beck. The alternative discussed by Beck is a return to the women-identified conception of participation in physical activity that existed in the United States until the middle of the twentieth century. Played by our "Foresisters and Great Grandmamas," Woman Sport promoted broadbased participation. Additionally,

Foresisters and Great Grandmamas encouraged com-testing for its own sake, play for the enjoyment of playing, with winning and extrinsic rewards being only incidental to the experience of playing hard, sweating profusely, scoring a goal, bonding with other women in the exaltation of a com-test well played (307).

Beck's prescription for a women-defined sport is no doubt attractive for many. Nevertheless, it falls short of being a developed statement of a revolutionized or even revised version of sport. What is lacking is an understanding of how the institutional features which now define sport, those deriving from its secularization and rationalization, will be replaced. That is, we have a call for social change but no analysis of how this will be effected. Our failure to develop such an analysis stems at least in part from our lack of an intellectual apparatus.

The second component of the feminist prescription for change is equality. Earlier it was stated that a basic tenet of the feminist view is that equality in sport will prevail. Unfortunately, the simplicity of this statement belies the confusion surrounding its actualization. Fundamental to the discussion of gender equality in sport is a consideration of the difference between equality of opportunity and equality in results or in sport, performance. Performance in sport is of course the outcome of the interplay of a multitude of factors, including biological, psychological, cultural, and social. Because of the gender differences in psychological, cultural, and social factors which have prevailed to date, it is impossible to know how much of the sport performance differential between men and women is based on genetic factors. Or, to pose an absurd hypothetical question, if nongenetic factors (opportunity, social support, etc.) were equalized, what gender differences in performance would remain? (And of course this is further confused with considerations of which sports, which events or positions in particular sports, etc.)

But the fact is that the differences have existed in *all* respects, and the result is that by most comparative measures (the average man vs. the average woman; the best man vs. the best woman), men outperform women. This difference in performance in turn relates back to issues of equality of opportunity, for when groups with unequal abilities to perform are provided with equal opportunities to compete, the result may be to reinforce differences in outcome (see Guttman, 35, 36).

Some of the implications of this difference between equality of opportunity and equality of performance are suggested by Coakley (264-7). Coakley's discussion is useful because it shows how the inequalities which have operated in the past make attaining equality within a limited resource structure problematic. He poses several possibilities for restructuring athletic programs and suggests the likely outcomes of each.

One option would be to open all teams to men and women. While this might seem to offer absolute equality, it would probably reduce women's chances to participate. "Because of obvious training and experience differences, all but a very small and select group of women would be excluded from ever having a chance to participate" (Coakley, 265).

Coakley's discussion provides additional examples, always coming back to the fact that the present differences in ability and interest inevitably affect the outcome of change in the opportunity structure. Recognizing this, Coakley suggests that the best approach currently is to provide an equal number of single sex teams for both men and women in sport activities reflecting the interests of each sex. Under this arrangement, special precautions would have to be taken to insure that men's and women's programs receive an equal share of resources. With such a provision, this model would be financially feasible and insure that women are represented.

An argument against segregated programs is presented by Abigail

Hoffman. Unlike Coakley, Hoffman is doubtful that girls' and women's programs can successfully challenge the stranglehold on resources maintained by boys' and men's programs. In addition, Hoffman supports the integration of sport because activities divided on the basis of sex inevitably imply that one sex (men) is superior to the other. The strength of this argument of superiority weakens, however, with the increasing accomplishments of women who have the advantage of opportunities to train and develop their skills.

The problem with the feminist critique of sport is that while the analysis of sport as sexist is correct, the prescriptions for change leave important issues unresolved. In the case of a reorientation to a women-defined sport, it is not clear how the diminution of the instrumental orientation of modern sport and its replacement with an alternative will be effected. In the case of redistribution of resources, there is disagreement regarding what gender equality in sport is, and following from this, how it may best be attained.

To complete the discussion of the implications of the radical and feminist critiques for each other, it may be said that the issues left unresolved by feminists are also unresolved in the radical formulation. Indeed, it could not be otherwise for a major thesis of this paper is that the two schools have offered much the same critique, albeit from different starting points. Radicals, like feminists, seek a movement away from the instrumental orientation of sport. They have not, however, offered an alternative to the hegemony of secularization and rationalization in the modern world. Similarly, although the tension between equality of opportunity and equality of performance is most extreme in the case of gender differences (because of biological factors), it also operates in the cases of racial and class differences. Members of racial minorities and the lower classes who are denied opportunities to participate will not develop their skills and hence will attain differential levels of performance.

In conclusion, this review of the radical and feminist critiques of sport has suggested that each offers a penetrating analysis which is nevertheless problematic in certain respects. The writings of the radical critics are often excessively polemical and their ideas poorly formulated and articulated. The feminist critique stops short of a developed analysis of alternatives to present structures and processes in sport. Notwithstanding these limitations, both bodies of work are important contributions. Their shortcomings should be seen as invitations for further investigations and analyses of sport in our society.

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the values assumed by those ideals, and are capable of fulfilling role expectations; and as a social ceremony structurally capable of fulfilling social functions comparable to those of religious ceremonies, specifically by serving as an arena for the creation of symbolic leaders and the display of heroic action. Both aspects serve the same purpose: reaffirming the values of the social order.

## Rituals and Symbolic Systems

### THE DURKHEIMIAN TRADITION

The conceptual definition of *ritual*, and the related concept *symbolic system*, on which this discussion of sport is based, are most familiar from Durkheim's *The Elementary Forms of the Religious Life* where he presented his thesis concerning religion as a social rather than psychological experience. While Durkheim's treatment is perhaps the most integrated and complete, and certainly the best known, he was not the only scholar of his time to propound that thesis. Numa-Denys Fustel de Coulanges, an influential teacher of Durkheim, presented the thesis in less developed form in his work *The Ancient City*. Fustel argued that the social organization of Greek and Roman civilizations made little sense to the modern scholar unless they were understood as civic manifestations of religion. Using the *polis* as focal point, he delineated the nature of the intricate relationship between community and religion.

W. Robertson Smith, the Scottish anthropologist, presented a similar argument in his description of the Semite communities, and A. R. Radcliffe Brown's study of Andaaman Islanders led to the conclusion that: ". . . Rites can therefore be shown to have a specific social function when, and to the extent that, they have for their effect to regulate, maintain and transmit from one generation to another sentiments on which the constitution of the society depends" (66).

However, with the publication of *Elementary Forms*, the thesis was presented in its most complete form. Durkheim's strategy was to select "the most primitive and simple religion which is actually known" (13) as a model for understanding the nature of the religious in its most basic form. Deciding that totemism was the most elementary form extant, Durkheim made use of field studies of the totemic religions of the Australian Aborigines.

On the basis of his secondary analysis, Durkheim developed his ideas about religion. To Durkheim, the religious process is divided into two aspects: beliefs and rites. Beliefs are "states of opinion, and consist in representations" (51) while rites are "determined modes of action" (51).

According to Durkheim, all religious beliefs are founded on the



fundamental classification of things as either *profane* or *sacred*. Profane things are the things of everyday existence. Sacred things are special things, protected, isolated, separated, prohibited, inaccessible, apart from the mundane world: they are invested with special properties.

Although Durkheim argues that the two categories exist *a priori*, the contents of the categories are culturally designated and are not the result of *a priori* characteristics of the things themselves. For example, in a non-Christian society, a cross is merely two crossed sticks and therefore as profane as any two sticks. But in Christian communities, the cross carries a meaning beyond its own physical characteristics. It is encoded with meaning and treated as a sacred thing.

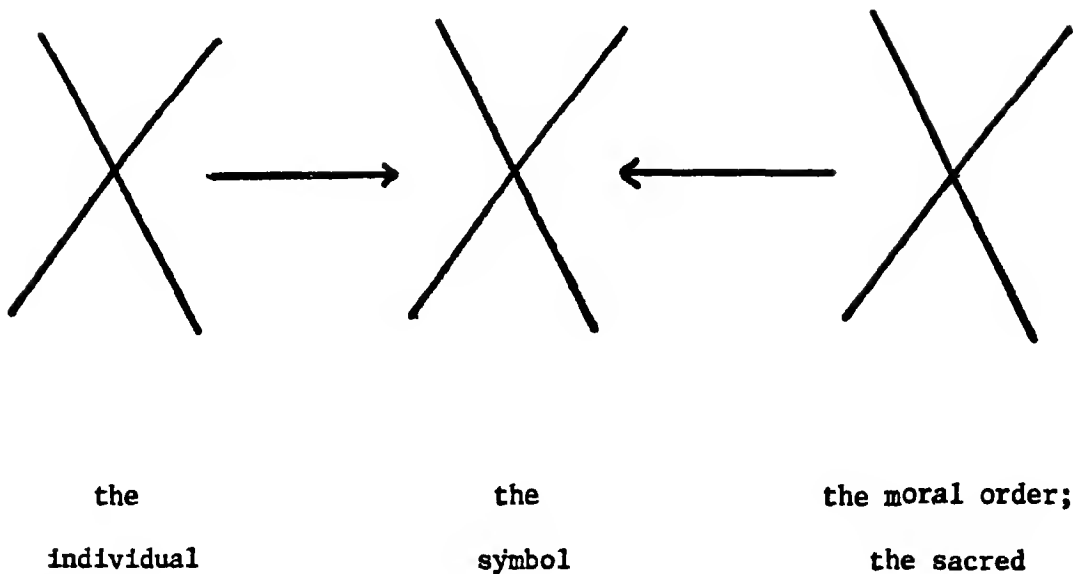
Thus the sacred may be more than the sacred idea itself (i.e., the belief in a Christian saviour) but usually includes symbols or representations of that special thing (i.e., the cross) which come to be treated as sacred. Symbols are, simply, things which stand for other abstractions. They are vehicles encoded with meanings, which serve as the basic units of meaning in rituals.

According to Durkheim, rituals are "rules of conduct which prescribe how a man should comport himself in the presence of . . . sacred objects" or their representations (56). Through ritual treatment of symbols of the sacred, the individual places self in respectful relationship to sacred things. Gradually, rituals become stylized patterns through which individuals express their respectful relationship to those objects or values designated as special or sacred.

Rituals are the *dynamics* of a process which joins together a system based on symbols. Durkheim conceives of this symbolic system as having three elements (see Figure 1). One element of the model is the individual member of a tribe or community. A second element is the moral order of the community, or the sacred: the values which are special to the community and worthy of respect and reverence. In other contexts this might be recognized as ideology. The third element is the symbol, a representation of the sacred which mediates between the individual and the moral order. Because it is difficult for the individual to pay homage to the abstract principle which constitutes the sacred, the symbol is a crucial element in the system. Through their treatment of the symbol, the individuals indicate affirmation for the abstract values for which it stands. Moreover, the symbol is a "collective representation" because it serves as a concrete reminder of the values of the community to which all individuals must subscribe and through which they maintain their community identity.

#### TRANSITIONS TO SECULAR RITUALS

Durkheim's conclusion about religion is that it is "an eminently collective thing" (63) and his definition of religion reflects that view: "A religion is a



\*This model is adapted from Munn.

Figure 1. DURKHEIM'S MODEL OF SYMBOLIC SYSTEMS\*

unified system of beliefs and practices relative to sacred things, that is to say, things set apart and forbidden—beliefs and practices which unites into one single moral community called a Church, all those who adhere to them" (62).

While Durkheim ostensibly focused attention on religious rituals, he fully realized that the principles he outlined were by no means confined to explaining phenomena labeled as religious. Clearly he viewed his thesis as a much more general one capable of explaining a class of phenomena of which religious phenomena were only one example. Thus, one of Durkheim's most significant contributions to social science is his thesis that religion must not be understood as a theological, philosophical, or psychological phenomenon, but as an experience which is eminently *social* in nature. The content of the symbolic system, the symbols venerated, the values and beliefs represented, all of these are relatively insignificant in relation to the fact that the ritual process itself serves to join the individual to a community of moral order. The *content* is not significant sociologically, but the *process* and the ceremonial effect are of profound significance.

Several scholars have built on Durkheim's general theory of ritual, emphasizing the preeminence of secular rituals. As part of his study of Yankee City, W. Lloyd Warner analyzed the Memorial Day celebration as "An American Sacred Ceremony." Warner conceived of Memorial Day as both a sacred and a secular ceremony. His thesis was that:

. . . Memorial Day ceremonies and subsidiary rites (such as those of Armistice or Veterans' Day) of today, yesterday, and tomorrow are rituals of a sacred symbol

system which functions periodically to unify the whole community, with its conflicting symbols and its opposing, autonomous churches and associations. It is contended here that in the Memorial Day ceremonies the anxieties which man has about death are confronted with a system of sacred beliefs about death which gives the individuals involved and the collectivity of individuals a feeling of well-being (8).

In the same year, Shils and Young's essay "The Meaning of the Coronation" appeared. Steeped in the Durkheimian tradition, Shils and Young argued

. . . the Coronation was the ceremonial occasion for the affirmation of the moral values by which the society lives. It was an act of national communion. . . .

The Coronation is exactly this kind of ceremonial in which the society reaffirms the moral values which constitute it as a society and renews its devotion to those values by an act of communion (67).

Both Warner and Shils and Young expand Durkheim's thesis to include secular aspects of ceremonies. However, it was left to Erving Goffman to take the thesis one step further and examine the interactions of everyday life as rituals of equal moral significance.

#### GOFFMAN AND INTERACTION RITUAL

One of the most significant and overlooked theoretical contributions made by Erving Goffman is his extension of Durkheim's ideas as clues for understanding the maintenance of social order. In a number of places throughout his work, Goffman acknowledges his debts to Durkheim and even makes explicit his intention to apply Durkheim's ideas to the study of everyday life. For example, in "The Nature of Deference and Demeanor" he states:

In this paper I want to explore some of the senses in which the person in our urban secular world is allotted a kind of sacredness that is displayed and confirmed by symbolic acts. An attempt will be made to build a conceptual scaffold by stretching and twisting some common anthropological terms. This will be used to support two concepts which I think are central to this area: deference and demeanor. Through these reformulations I will try to show that a version of Durkheim's social psychology can be effective in modern dress (c, 473).

In Durkheimian fashion, Goffman defines ritual as an activity which "represents a way in which the individual must guard and design the symbolic implications of his acts while in the immediate presence of an object that has special value for him" (b, 478; h, 62; k, 69). He also calls this, in true Goffman fashion, a "situated social fuss."

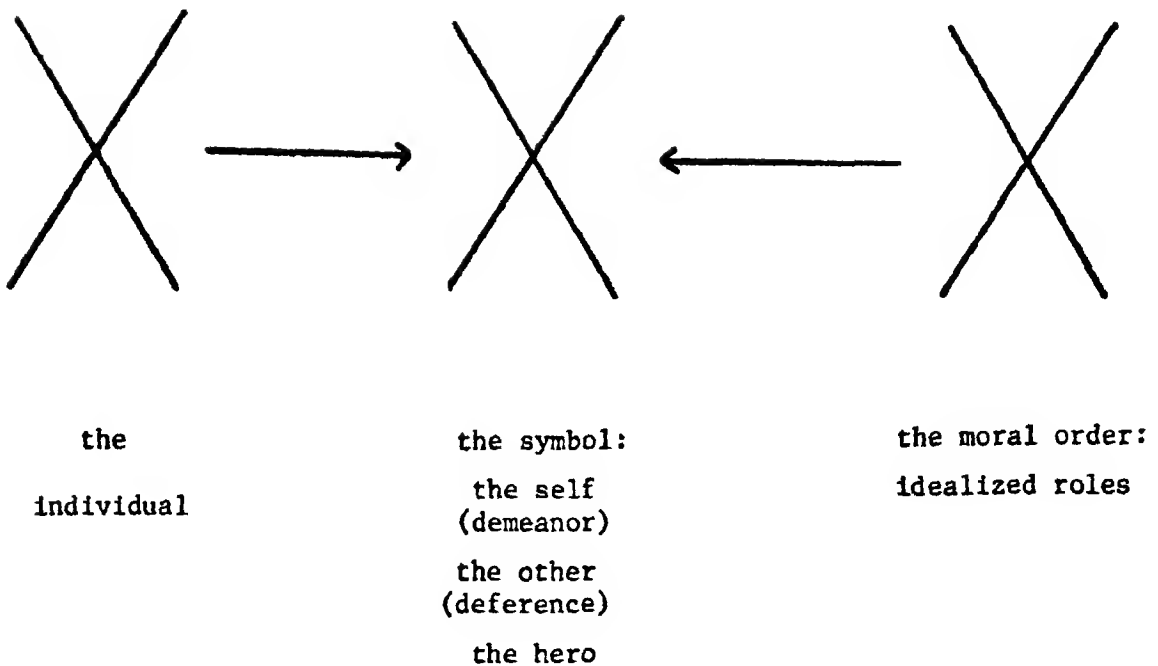
But unlike Durkheim, Goffman looks for ritual not in the eventful, exciting, or spectacular, but in the every minute, everyday-life interactions that comprise the bulk of social experience. To Goffman, those "objects that have special value" are not only crosses and waterholes and flags but

simply—and profoundly—the self and the other in everyday interaction. Goffman calls the process of communicating respect for the self and other the rituals of *demeanor* and *deference* respectively. These are ritually important because interacting individuals continually take on the responsibility of embodying important social roles. They attempt to portray the ideal qualities demanded of them in these roles—the loyal friend, the faithful lover, the loving parent, the efficient secretary, the dedicated scholar, the tough athlete. Through the idealization of performance, Goffman says, individuals are not attempting in a deceitful way to claim values for themselves that they do not in fact possess. They are attempting to demonstrate through their selves the ideal role characteristics valued by society. Through their behavior, they are reaffirming significant values of the moral order.

This reaffirmation of values takes place within the context of several specific forms of ritual delineated by Goffman with an eye toward the Durkheimian tradition. Communicating respect for the self, through rituals of demeanor, includes such practices as showing self-respect (b, 215), maintaining poise (a, 275; b, 215; d, 103), and demonstrating skills of impression management or facework (b, 216; e, 208). Communicating respect for others, through rituals of deference, hinges on identificatory sympathy (h, 66), presentational deference (c, 485), the rule of considerateness (b, 215), and giving face when others inadvertently lose theirs (b, 215). Together, rituals of demeanor and deference comprise *positive rituals*, a term clearly adopted from Durkheim by Goffman (h) to indicate rituals which affirm the sacred qualities of objects.

These positive rituals are complemented by *negative rituals* which dictate behaviors required to avoid contaminating the sacred (h). These interdictions or avoidance behaviors occur when some threat to demeanor or deference is perceived. In Goffman's terms, these negative rituals can be *preventive practices* (e, 13) which occur in order to avoid such threats becoming reality and *remedial practices* (h, 64; g, 365) used to restore order when profanation has inadvertently taken place. Preventive practices include exploratory communication (a, 333), overplay gloss (i, 134–5), circumspection gloss (i, 132), referential avoidance (c, 482), and no contest signs (i, 124). Remedial practices include the rule of open laughter (j, 372) and a six move remedial process: challenge, offering, remedy, relief, appreciation, and minimization (b, 220; i, 140–3; 1, 265).

In summary, in many essays Goffman concerns himself with the rituals of everyday interaction which celebrate the self and the other as significant moral beings. Clearly, Goffman's ideas of the significance of individuals in interaction build on Durkheim's model of religion. Figure 2 indicates that the concrete symbol within which societal values are encoded may very well take human form. In that special case, meanings are not encoded but embodied. Indeed, in Goffman's elaboration of Durkheim, every individual is a symbol; every individual is a collective representation



**Figure 2.** DURKHEIMIAN'S MODEL APPLIED TO GOFFMAN'S THESIS OF INTERACTION RITUAL

of moral values; every individual is sacred. Goffman is quite explicit about this in a number of places. For example, in his dissertation, he states:

For the actor, others may come to be seen as sacred objects. The social attributes of recipients must be constantly honored; where these attributes have been dishonored, propitiation must follow. The actor must conduct himself with great ritual care. . . . (a, 103).

. . . persons are ritually delicate objects which must be treated with care, with ceremonial offerings and propitiations (a, 175).

A few years later, in his essay on "The Nature of Deference and Demeanor," he reminds the reader of the Durkheimian heritage of his thesis by stating:

In this paper I have suggested that Durkheimian notions about primitive religion can be translated into concepts of deference and demeanor and that these concepts help us to grasp some aspects of urban secular living. The implication is that in one sense this secular world is not so irreligious as we might think. Many gods have been done away with, but the individual himself stubbornly remains as a deity of considerable importance. He walks with some dignity and is the recipient of many little offerings. He is jealous of the worship due him, yet, approached in the right spirit, he is ready to forgive those who may have offended him. Because of their status relative to his, some persons will find him contaminating while others will find they contaminate him, in either case finding that they must treat him with ritual care. Perhaps the individual is so viable a god because he can actually understand the ceremonial significance of the way he is treated, and quite on his own can respond dramatically to what is proffered him. In contacts between such deities

there is no need for middlemen; each of these gods is able to serve as his own priest (c, 499).

With such an undeniable moral current running throughout his work, it is difficult to understand the critiques of Goffman as a cynical purveyor of the alienation of man in society. Every encounter is conceived as a statement of moral reaffirmation.

Goffman's extension of the Durkheim tradition makes two major contributions. First, it extends the boundaries of ritual to include everyday life. Moreover, as Luckmann also contends, not only do interactions serve as the reaffirmation of societal values, they serve to recreate those values in negotiation. Thus encounters play a dynamic and not a passive part in the creation of the moral order.

The second contribution of Goffman is the emphasis on the individual as sacred. Interaction rituals in Goffman's work are interpersonal rituals, and the values reaffirmed are those related to the worth of individuals in the social order.

## Sport as Ritual

### THE INDIVIDUAL AS SACRED: THE HERO

Two ideas of ritual are at work here. One, the Durkheimian, focuses on the eventful, the exciting, the relatively infrequent homage paid to sacred values through ceremonial treatment. Such homage serves as a community act, i.e., an act which reaffirms the values which unite the community. The other, the Goffmanesque, focuses on the mundane, the trite, the commonplace everyday homage paid to idealized role performances presented in everyday interaction, i.e., the way actors perform their roles and react to others in theirs. This process represents a more private act, yet it is still an act of reaffirmation of values of significance to the community.

These two ideas are united in the concept of the individual in special situations, i.e., the hero as an exemplary interactant in social ceremonies. There are several developmental threads joined in this idea.

Weber's concept of *charisma* provides a link between Durkheim's concept of the sacred and Goffman's concept of character display. In its pure form, charisma is

a certain quality of an individual personality by virtue of which he is set apart from ordinary men and treated as endowed with supernatural, superhuman, or at least specifically exceptional powers or qualities. These are such as are not accessible to the ordinary person, but are regarded as of divine origin or as exemplary, and on the basis of them the individual concerned is treated as a leader (358-9).

Clearly the individual with charisma approaches what Durkheim means by

his designation of sacred: "set apart" "exceptional" "not accessible to the ordinary person" "exemplary."

Bendix notes that charisma "depend(s) on a belief in concrete persons whose authority is regarded as sacred and to whom followers or subjects feel bound in religious reverence and duty" (quoted in Nisbet, 255). Nisbet continues that it is through its qualities as sacred that charisma "becomes a major part of social and political systems" (255).

From these comments, one can see that Weber's conceptualization of the charismatic leader as mediator between individual men and the moral order furnishes a direct line between Durkheim's notion of the symbol or emblem as mediator between the individual and the social order and Goffman's conceptualization of the individual as a symbol of the moral order. A further step is taken with the introduction of Klapp's concept of symbolic leaders and his thesis of the dialectic origin of heroism.

Klapp focuses on the modern day celebrity and the enigmatic process through which the celebrity emerges. Klapp conceives of the symbolic leader as "an emergent phenomenon" (32), born out of the process of interaction. He identifies seven steps in the process which generally seems to entail an almost serendipitous discovery of an image or attitude that strikes a responsive chord in the audience. From the first stage the leader and the audience embark on a dialectic in which the leader responds to the reactions to his presented self by altering his image to be congruent with the audience demands. Thus by a process of negotiation, a celebrity is born.

Particularly important for the creation of symbolic leaders is the dramatic encounter: "The very essence of drama—the high point of its most important scenes—is usually a confrontation in which parties are thrown on their mettle, reveal and expose themselves, drop their defenses, call on their personal resources to meet a crisis" (70). This concept is not only labeled in Goffman-like manner, but is conceptually similar to what Goffman discusses as action situations.

Luckmann's concern with "the fate of the individual in modern society" (12) follows in a direct line from Durkheim's like concerns, and like Durkheim, Luckmann looks for understanding in the sociology of religion. Luckmann sees his work as an extension of the Durkheimian tradition; it also has much in common with the central thesis offered by Goffman in his writings about interaction rituals.

Both Durkheim and Luckmann can be approached through their different interpretations of the basic model presented in Figure 1 for both are concerned with the interrelationships of the three aspects of the symbolic system: the individual, the social order, and the symbolic order, or religion. But where Durkheim conceptualizes the symbolic order as a mediating force between the individual and the moral order, Luckmann sees the moral order, or world view, as the ultimate construction of individuals.

There are several elaborations of Durkheim here. One is that the interaction itself is symbolic, in the sense that meanings are conveyed through symbolic communications. Thus the symbolic order is both the construction of the social order and the tool or method of that construction. A second idea is that the symbolic order expands to become the social order, for the social order is itself constructed through the manipulation of symbols and meanings by individuals. Important here is the notion of the history of the individual in the company of others. The expectations of his behavior are based on his history of interactions with others to which he is accountable. His behavior, his Self as Luckmann refers to it (Goffman would say "face"), has been created through a symbolic interchange which is religious in nature, and his behavior, restricted by historical expectations and the feedback of others, becomes a part of the moral order.

The final idea is that of the interchange of the social past with the constructed present. Luckmann states that, empirically, man is born into a social order (a culture) which restricts the meanings he can manufacture for his own actions and those of others. On this point, a basic difference between Luckmann and Durkheim emerges. Where Durkheim implies a mediation through symbols between the individual and the moral order, he has also implied a more static model than Luckmann proposes. Luckmann's model pays attention to the moderate state of flux which surrounds individuals by explicitly attending to the active part they play in reasserting or reconstructing the moral order.

This distinction is carried out to a lesser extent through their different conceptualizations of the sacred and the profane. Durkheim sees the categories (structurally) as existing *a priori* while the contents are culturally designated. The idea of separation is a significant one which is carried out through attitudes or actions which indicate deference and respect. But Luckmann speaks of the sacred and profane "uncategorically" by implying that within the hierarchy of meanings which individuals construct, the notions of sacred and profane are located. Thus the profane is a concrete and unproblematic level of understanding while the transcendent world order, or cosmos, is sacred or religious in nature.

Like Klapp's more popular analysis, Luckmann describes the dialectical nature of the creation of the self, but Luckmann emphasizes the process as a fundamentally religious process. Moreover, Luckmann's thesis clearly unifies what Hook perceived as two divergent forces: in Luckmann's thesis, man is both the symbol and the creator of his own history.

Another important variation on Durkheim's thesis expounded by Luckmann is closely related to Goffman's perspective. Here a purely functionalist interpretation of religion is supplemented by tenets of symbolic interactionism, specifically the creative potential of the individual as interactant in shaping the moral order. Goffman's ideas about the rituals of interaction provide further insight into the scope of the thesis.



## GOFFMAN'S CONCEPT OF CHARACTER

Goffman's extension of these notions of the hero emanates from his concept of exaggerated rituals or special situations in which the moral statements generated through interaction take on a public, generalizable, and dramatic nature. Goffman believes that generally individuals try to minimize unpleasantness and the possibility of dysphoric, disruptive interaction (Birrell, b). Individuals try to avoid embarrassment and negative experience. But in some cases, some individuals approach these potential situations with relish. In "Where the Action Is" Goffman labels these situations *action situations*, and he defines them as fateful, that is, situations which are both problematic (the outcome is uncertain) and consequential (the outcome will have an impact on future events). Some individuals willingly and willfully seek out these risky situations. Moreover, only in such situations can *character* be demonstrated.

Character is an exaggerated portrayal of demeanor. It is an individual's response to action situations and entails "capacities for standing correct and steady in the face of sudden pressure" (f, 217). Goffman notes, "they do not specify the *activity* of the individual but how he will *manage* himself in this activity" (f, 217).

In action situations, when the interaction ritual is public, the actor generates character which reflects not only on self but has social significance because it reflects the values of the community. Such a situation exists in sporting contests witnessed by the public.

In many situations, as in sport, assessment of character focuses on characteristics highly prized in the particular setting. Goffman distinguishes four motifs around which character contests in North America might revolve: courage, gameness, integrity, and composure. From Goffman's definitions one can immediately perceive their ready applicability to sport.

*courage*—the capacity to envisage immediate danger and yet proceed with the course of action that brings the danger on (218).

*gameness*—the capacity to stick to a line of activity and to continue to pour all effort into it regardless of set-backs, pain, or fatigue (219).

*integrity*—the propensity to resist temptation in situations where there would be much profit and some impunity in departing momentarily from moral standards (219).

*composure*—self-control, self-possession, or poise (222).

The social significance of the demonstration of these and other valued qualities lies in society's understanding of them as inherently moral and worthy of deep respect. Goffman notes that

Properties of character . . . are always judged from a moral perspective, simply because a capacity for mobilizing oneself for the moment is always subjected to

social evaluation. . . . character traits tend to be evaluated in the extremes, referring to failures in no way expected or successes out of the ordinary; mere conformance with usual standards is not at issue (218).

Moreover, the display of character not only makes a statement about the individual, but about the sanctity with which the abstract values he embodies should be treated. There is no question about the heritage of this idea in Goffman's work:

To the degree that a performance highlights the common official values of the society in which it occurs, we may look upon it, in the manner of Durkheim and Radcliffe-Brown, as a ceremony—as an expressive rejuvenation and reaffirmation of the moral values of the community. Furthermore, insofar as the expressive bias of performances comes to be accepted as reality, then that which is accepted at the moment as reality will have some of the characteristics of a celebration (e, 35).

The following examples from the sport world are intended to substantiate the thesis that in sport, respectable qualities are demonstrated in dramatic and public situations, and that those demonstrations of character serve a dual purpose of establishing the character of the athlete and reaffirming the validity of the moral attributes the individual displays.

### Courage

Because of the physical nature inherent in sport, it is not surprising that many sports have as their central feature the demonstration of physical courage. The most prominent examples are those sports in which death is a possible consequence. Bullfighting, if one considers it a sport, provides an excellent example, for the ethic of the bullfight demands that the matador take tremendous chances in order to prove his worth to the crowd. While generally the crowd is insatiable in its demands for danger (Zurcher and Meadow), some matadors display so much courage that the crowd begs them to be more careful. Such was the case with El Cordobes, the spectacular Spanish bullfighter, who demonstrated his courage by breaking into short pieces the barbed sticks (*banderillas*) the matador drives into the shoulders of the bull; by fighting in the center of the ring furthest from help, should he require it; by leaping over the back of the bull as it charged; and, on one fateful occasion, by refusing to cancel a fight although the sand in the stadium was soaked with rain, leaving him with poor footing.

Mountain and rock climbing and their kindred pursuits furnish another example. Along with the more traditional accounts (Hilary; Houston and Bates) of such daring, the public has recently been treated to a documentary relating one man's attempt to ski down Mt. Everest and a public drama in which a rock climber scaled the walls of the World Trade Center in New York City (Moses).

Auto racing is another world in which courage is prized. Drivers believe that someone who cannot demonstrate a minimal amount of cour-

age, here synonymous with speed, does not have the character to compete with the other racers. Richard Petty illustrates this opinion when he acknowledges that

I've drafted cars that slowed down on me, and I've had to shove them a spell to get them going better. And a bunch of times I've come up behind somebody I've lapped maybe 10 or 15 times, and deliberately tapped him or maybe run him down off the track because he didn't belong out there racing (quoted in Libby, 62).

Much prestige or honor in football is related to how well one can demonstrate physical courage to the point of being macho, according to many (Meggysey; Shaw). Shaw claimed that at the University of Texas, a distinction was drawn between the hitters and the quitters. They even had a motto along this line: "if you're putting out you don't get hurt." Among hockey players, Faulkner (298) has noted a similar pattern: players use violence as a "presentational resource" for the display of honorable, respectable occupational behavior.

Hubert Green has shown that character based on courage can be demonstrated in the rather unlikely setting of a professional golf match. While leading by one stroke in the U.S. Open in 1977, Green was informed that the FBI had received word of a death threat against his life. Green decided to continue play. He finished the last three holes under police and FBI surveillance and won the Open title. As Dan Jenkins, writing for *Sports Illustrated*, put it ". . . now we know a bit more about his competitiveness as a golfer and an awful lot more about his nerve and his heart" (19).

Final examples might be drawn from the worlds of gambling. Scott states that bettors at a thoroughbred race track who bet the favorite are referred to with disrespect as "chalk-eaters" because they lack the courage to put their financial self on the line. Boyd notes that poker players classify one another as "tighties" or "loosies" according to the degree of chance they are willing to take; only the loosies are capable of displaying character, for only they are taking the big risks. Geertz discovered the same principle at work in the Balinese cock fights. Geertz labeled the phenomenon "deep play," and described a sport setting in which it is customary to bet far more than one can afford to lose. According to Geertz's account, this system produces many poor but honorable Balinese.

### *Gameness*

Gameness is related to courage for it, too, represents overcoming physical odds in order to compete honorably. In sport, gameness means perseverance and spunk, and it is illustrated by cases in which the athlete has failed to live up to expectations of skill yet struggles valiantly to finish the task. To this athlete—the runner who has been hopelessly lapped by the competition or the gymnast who falls several times in one routine—the audience awards praise for gameness and applauds with sympathy and encourage-

ment. The classic example concerns the marathon runner in the 1972 Olympics who struggled on to finish the race—but not until after the closing ceremonies had taken place.

No doubt because of the heavy physical punishment involved, boxing furnishes a setting for many examples of gameness related to perseverance (Weinberg and Arond).

It is the demonstration of this quality that turns the final, painful scene in *Rocky* to a heroic battle. "Ain't gonna be no rematch" says the bloodied defender Apollo Creed. "Don't want one," replies Rocky who has proved to himself that, at least once in his life, he was able "to go the distance." On a smaller scale, Rau reported a Golden Gloves welterweight fight in which the loser was a spunky boy whom Rau had pulled out of a car wreck ten hours before. The match was so tough that both boxers received standing ovations from the crowd. X-rays later showed that the boy had fractured a vertebra during the auto accident.

Collins and Lapierre cite another extraordinary example of gameness. When El Cordobes, the idol of the bullring, was almost fatally gored during a fight, the crowd responded "with a waving field of white handkerchiefs, beseeching (the bull's) ears for the matador who now lay unconscious on an operating table while a surgeon struggled to save his life" (313). Collins and Lapierre note: "It was an extraordinary gesture because tradition of the bullring demands that its trophies be awarded only to a matador who has completed his task by killing his bulls" (315). El Cordobes, who had proven his courage and composure time after time, was rewarded here for his gameness.

Other examples of perseverance include the crosscountry skier who finished his race in the 1976 Olympics on only one ski; Jim Ryun getting up to finish the 1,500 meters after being knocked off balance during the 1968 Olympics; and the legendary refusal of athletes like Dave Cowens and Pete Rose to give less than 100 percent in any contest, regardless of the score.

In many sports, as in other realms of life, the ability to come back after a serious injury can generate respect for an individual. Few could match the example set by Nicki Lauda during the 1976 Grand Prix season. Involved in a fiery accident in Nurhurgling, Germany, Lauda was given the last rites. Not only did he survive, but six weeks later he returned to the racing circuit wearing a flameproof mask to protect his scarred face and challenged James Hunt for the lead in the Grand Prix standings. There are some apparently who feel Janet Guthrie's quest for respect from other drivers will not be fulfilled until she has survived and bounced back from a serious accident like Lauda's.

There are some men here who secretly may be hoping that Janet Guthrie does not return, that her car hits another machine and then the wall as Tom Sneva's car did two years ago, that it runs into a stalled racer on the track and explodes, as Mike Mosley's did in 1971, or that it loses a wheel and spins wildly out of control, as Al

Unser's did in 1968. Then they will wait to see if Guthrie comes back, as Sneva and Mosley and Unser have many times since their brushes with instant immolation. Only then, the hard-liners say will she have proved that she belongs here with the big boys (Abel, 49).

### *Integrity*

Integrity is expected in sport, for without cooperative agreement to obey the rules and refrain from self-serving cheating, sport cannot continue; its nature is violated. Yet acts of integrity can be magnified in sport to provide a true test of character. The moral strength to follow the correct path when easier routes are available is constantly demanded of those who put themselves on the line in sport, for sport is an activity in which obstacles are purposely planted before one so that his or her reactions may be tested.

Goffman specifies two forms of integrity: self-discipline and gallantry. By *self-discipline* he means "the capacity to refrain from excessive involvement in the easy pleasures . . ." (f, 220). The self-discipline required by sport is such a catch word that it hardly needs to be documented. The most familiar examples center around the pain of training and the Spartan existence and self-denial necessary to shape a championship athlete. This form of physical self-discipline is such a commonplace in sport that it does not furnish as striking a basis for character display as do other aspects. Physical self-discipline is taken for granted in sport; it is expected of all top-calibre athletes; it is part of the bargain they make in their pursuit of physical excellence.

Moral self-discipline is another story. In some cases, this form of self-discipline can mean accepting one's defeat without excuses. Thus one writer was moved to commend Jimmy Connors for his restraint when, having lost two important tennis matches, he refrained from mentioning to the press that he was preoccupied with his father's serious illness ("A Man for A' That," 10).

Goffman defines *gallantry* as "the capacity to maintain the forms of courtesy when the forms are full of substance" (f, 220), i.e., when they really matter. In sport, this quality is often referred to as sportsmanship. Sportsmanship is a trait of moral character with a historical tie to social class and to sports whose clientele was drawn historically from the elite. In England during the middle of the nineteenth century, for instance, when football was the domain of the upper classes, umpires or referees were unheard of and unnecessary (McIntosh). A gentlemanly code of conduct was the norm and a sportsman was quick to acknowledge whatever unintentional fouls or violations he had committed, apologize to his opponent, and make restitution.

In a more modern example, Kroll has suggested that sportsmanship, indeed any moral decision, matters only when the stakes are highest, i.e., when the decision could determine the outcome of the match. It is demon-

strated whenever a player calls an undetectable but game-important foul on himself or when a player who has benefitted from an erroneous call restores the sporting balance, as in tennis when a player hits the next ball out of bounds after the lineman has wrongly called a point in her favor. More dramatic instances are exemplified by Lutz Long helping Jesse Owens qualify in the long jump in the 1936 Olympics, when Long knew that Owens was his toughest challenger (and indeed Owens went on to win the gold, Long the silver). (Another example is found in *Fair Play*, 4.)

### *Composure*

Composure is a form of character emphasizing "self-control, self-possession, or poise." According to Goffman, composure has a "behavioral side, the capacity to execute physical tasks in a concerted, smooth, self-controlled fashion under fateful circumstances" (f, 222-3) and "an affective side, the emotional self-control required in dealing with others" (f, 224). Of all forms of character display, Goffman considers composure the most significant because the individual's capacity as a competent interactant is revealed in his or her ability to maintain proper demeanor, particularly in trying situations.

Goffman specifies three forms that composure may take: presence of mind, dignity, and stage confidence. *Presence of mind*, or mental calmness, is a prerequisite for many sports, particularly those self-paced tasks in which steadiness is necessary. Discussing this topic, the silver medalist in the 1976 Olympics small-bore, three-position rifle shooting competition Margaret Murdock stated:

My emotional control is based on anticipation . . . I think out how I'm going to react, how I'm going to resist extraneous thoughts, how I'm going to deal with somebody coming up and telling me I'm behind or ahead, I prepare for all this, so the adrenalin doesn't go up and I stop thinking. One or two bad shots, and you're out of it (quoted in Fimrite).

Gymnasts, particularly those who compete on the balanced beam, experience the same thing (Birrell, a).

A second aspect of composure and one particularly important in sport is the demonstration of dignity. *Dignity* is a bodily form of composure which features "the capacity to sustain one's bodily decorum . . ." (f, 225). To exemplify this aspect of composure Goffman chooses surfing:

Physical aplomb and dignity of upright posture must be maintained on a flat narrow board against rumbling forces that press to the limit the human capacity for this kind of bodily self-control. Here the maintenance of physical control is not merely a condition of effective performance but a central purpose of it (f, 226).

Of course all sports require some degree of dignity, or physical control; the term "natural athlete" seems to express the envy of those who must work

hard at maintaining dignity. Goffman mentions skiers as a sport group particularly prone to displays of—or lack of—dignity.

Yet it is only in the fan-packed arena that the two aspects of composure discussed—presence of mind and dignity—are combined with and intensified by the final form of composure: *stage confidence*. Only when an individual puts self on the line in a situation of heightened emotional stress is character truly tested. Thus weekend skiing presents a challenge to one's physical control, but skiing for an Olympic gold medal intensifies the challenge, as this account of Franz Klammer's bid for the downhill title in 1976 demonstrates.

Pressure defines the essential nature of athletic heroism. If an athlete cannot contend with it, neither strength nor skill will avail him, and few athletes have borne a greater burden of pressure than the 22-year-old Austrian skier Franz Klammer in the Winter Olympic Games. . . . Klammer had drawn starting position No. 15, last among the top seeds in the downhill. All of his principal competitors had preceded him down the icy and irregular slope. The 27-year old Swiss skier Bernhard Russi was the leader at 1:46.06.

Klammer almost literally flew out of the start and careened down the hill like a bouncing ball. Still, he was .19 of a second behind Russi's pace after the first half of the run. He took even more chances in the next section but slipped farther behind the pace. In the last 1,000 meters he was confronted by the Compression, a jump followed by a dip that had proved breathtakingly dangerous to his predecessors. The Johannesweg turn lay behind that and it had already claimed two skiers. As he ripped through this part of the course, Klammer nearly went out of control, but he regained his balance to execute the turn. Then he sailed over the last jump and sped to the finish. It was a daring run, but was it fast enough? Yes. The timer showed that Klammer had beaten Russi by .33 of a second. The mountain exploded in an avalanche of cheering (Fimrite).

Much of what is admired in sport can be summed up in one word: coolness. Scott illustrates this with some observations from the race track:

The cool jockey can wait patiently with a horse in a pocket and get through on the inside, risking the possibility that there will be no opening. Coolness is waiting far back in the pack, risking the possibility that his horse will not 'get up' in time. Coolness is sparing the whip on a front-running horse when another animal has pressed into the lead, risking the possibility that once his horse is passed he will not get started again. All these activities are taken by observers as instances of a jockey's character. In short, moral character is coolness in risky situations (26).

Polsky observed the same coolness in the pool hall (45).

Another sport in which coolness is important is tennis, where Chris Evert plays with such presence of mind and dignity that she has been nicknamed "the ice maiden" and Bjorn Borg has been called "Ice Borg." Ironically, Evert's coolness is often held against her; she suffers particularly in comparison to the looser, less disciplined Evonne Goolagong, whose "walk-about" have earned her a contrasting reputation.

Coolness is important in most sports, but one would be hard put to find a group which exemplifies the demands of composure as well as gymnasts. For the gymnast, character hinges on poise, yet the gymnastics meet, by its very nature, seems designed to destroy one's composure. The irony of testing oneself to the very limit of one's ability while maintaining by face and posture the impression that one is coasting effortlessly and gracefully through a simple routine is not lost on the gymnasts. As more than one gymnast put it: "The object of gymnastics is to make something very difficult look very easy."

Cool competence is the trademark of the superior gymnast. The gymnast must give a solid performance, and her confidence in execution must be indicated by the absence of "major breaks" in the momentum of the routine as well as control over nervousness and unsteadiness. But sometimes poise is demonstrated by the gymnast's responses to problematic situations beyond her control. Bows can fall from her hair onto the mat where she must continue to perform as if its undesirable presence there is not an intrusion on her concentration; slippers can be lost during a balance beam routine leaving the gymnast with less purchase on the beam than she would desire; elaborate hairdos can become so disarranged that they hinder the sight or balance of the gymnast. Yet in all of these cases, the inconvenience must be totally ignored by the gymnast if she wishes to maintain her display of poise. In fact, kicking aside a bow or slipper or adjusting a falling hairdo are movements which the judges are likely to score as deductions from the "general impression" of the gymnast's performance.

An extreme example of character related to this sort of trying situation occurred to an injured gymnast during a meet. Because of her injury, she had an ace bandage wrapped around her knee. In the middle of her beam routine, the bandage became unwound and she finished the rest of the routine trailing the bandage. Even her dismount, which was a front somersault, she did with it flying through the air. To have touched the bandage would have permitted the judges the opportunity to deduct points under "general impression." Continuance of the routine, on the other hand, was actually quite dangerous, sparking an incredulous response from one observer "that the sport cannot make accommodations for things like that." But by continuing her performance, the gymnast demonstrated to all observing that she was a competent performer, able to maintain her composure in the face of the most unsettling challenges. Character thus has to do with keeping one's entire competitive self in order and under complete control at all times.



## Conclusions

Sport has ritual significance when character based on valued social attributes is demonstrated. In such situations, the athlete is an exemplary figure who embodies the moral values of the community and thus serves as a symbol of those values. The salience of the incidents recounted here is evidenced by the very fact that they were reported by the mass media for the benefit of those who were not fortunate enough to witness the demonstrations in person. Publication of such deeds serves as institutionalized recognition that demonstration of character should be greeted with admiration, respect, and perhaps, worship.

An equally important indication of the strength of this thesis is the fact that individuals who fail to demonstrate character in sport after having made such exaggerated claims for themselves are regarded with disrespect and scorn. A dramatic example of the effects of losing character on the way in which others regard the individual can be found in this account of a young man's unsuccessful attempt to prove himself in the bullring:

. . . To Juan Horrillo, Little Almond's horns seemed wider than the branches of an almond tree. . . . He felt he would never have the courage to confront the cow waiting for him in the ring. And yet he knew she was his to fight and kill, 'that filthy beast, before all those Palmenos with their dirty stares, looking like I would have to hang myself up on those horns before they would be happy.' Behind him, he could hear Manolo hissing, 'Go ahead, Juna, don't worry, I'll watch you.' Encouraged, he tried to go forward. But at each step, Horrillo felt his legs folding under him as though they were made of rubber. The arena began to spin around his eyes 'like a merry-go-round.' Many in the crowd sensing what they had come for, began to jeer. Still Horrillo couldn't move.

'I was paralyzed,' he could recall. 'I trembled all over. Those insults kept coming out and still I couldn't take a step. They began to throw stones and bottles at me. They started to yell *Fuera, fuera*—out, out. God, I hated it. And there was Almendrita in the middle of the ring staring at me with those murderous eyes of hers. Suddenly it happened. She came for me. I did a terrible thing then, an unforgivable thing. I dropped my muleta and ran. I ran as fast as I could for the *barrera*, as fast as I ever ran before from the Guardia Civil. When I got there, I jumped the *barrera* and fell into the *callejon*. As I lay there, shaking, they spat on me. I could hear them all screaming, whistling. It was horrible, I don't know how long I lay there listening, but I knew one thing: for me, everything was finished. I could never put on the suit of lights again' (Collins and Lapierre, 272–3).

One can lose one's character if one loses courage. And one can also lose character by failing to display integrity or poise. Ilie Nastase, the tennis player, is an example of this form of character loss: his outrageous antics on the court have irreparably damaged his reputation. A more classic example can be found in the young boy's legendary plea to his former baseball hero, accused of throwing the 1919 World Series: "Say it isn't so,

Joe." When character is so spectacularly lost, something else dies with it. If the moral order is to be preserved, those whose actions flagrantly violate the sanctity of systemic values must be regarded as villains or weaklings, just as those who spectacularly conform to moral values must be feted as heroes.

It may well be that an important aspect of the significance of sport as an arena for character lies in the fact that it is such a commonplace, everyday activity. It is true that in extraordinary cases an athlete comes to have enduring symbolic value; his or her deeds may become the basis of legends and his or her memory may be institutionally enshrined. However, an added significance of many of the examples offered above is the transient nature of the tales. The names may soon be forgotten, perhaps are never known, but the substance of the achievement has served its purpose by providing one of an endless number of reaffirmations of cultural values. Therefore it is the commonplace nature of sport that is one of the most significant attributes: on any given day people can approach the ballpark and witness not only an exciting, sport event, but also the display of attributes of character unseen in other areas of life.

Demonstration of character can be understood as something whose significance flows beyond the respect awarded to individuals by knowing peers—teammates and opponents. Character display has a more generalizable, public, and ceremonial significance. Thus the so-called "hero-worship" of athletes should not be regarded disparagingly as evidence of modern man's replacement of religious ideals with secular or even heathen images. The shift does not mark a fundamental change in social values but merely a substitution of the vessel in which they are contained.

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# The Mass Society and Group Action Theories of Cultural Production: The Case of Stylistic Innovation in Jazz\*

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## ABSTRACT

*Two theoretical orientations to cultural production are presented: the mass society and group action theories. It is argued that the two theories provide alternative, but not mutually exclusive, explanations for changes in cultural content and that the case of stylistic change in jazz between 1920 and 1940 presents an appropriate empirical test. Examining data on black jazz, its musicians and audiences in Detroit, it is found that a group action explanation has more power than a mass society explanation. Only one of the three stylistic changes isolated in a musicological analysis is causally related to the integration of jazz into the system of popular music production, whereas the other two changes are best accounted for by the purposive action of musicians as a group. Implications are drawn for the sociology of jazz as well as the study of cultural production.*

Jazz is an art form which, like film, has gone through rapid changes since its origins in the early part of this century. Such patterns of cultural change provide a rich but underutilized data base for studies of cultural production (Peterson, c). The stylistic changes that occurred in jazz between the two World Wars could be accounted for by two distinct theoretical orientations to cultural production in twentieth century Western societies. In this paper I will examine the relative explanatory power of the mass society and group action orientations to cultural production.

The mass society and group action orientations provide alternative, but not mutually exclusive, explanations for changes in cultural content in modern societies. According to mass society theory the sources of such

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change are to be found in the organization of popular culture production, which increasingly assimilates previously autonomous realms of production, such as folk or fine arts. Alternatively, group action theory sees cultural change as the result of the purposive action of various social groups, primarily status groups. I will argue that the case of stylistic change in jazz between 1920 and 1940 presents an appropriate empirical test of the relative power of these two explanations for cultural change. But before turning to matters of research design, let us review the mass society and group action theories and how they have been used in empirical studies of cultural production.

### **The Theory of Mass Society**

The theory of mass society is an orientation which stresses how elites, through their control of central institutions, manipulate the behavior of a homogenous mass of atomized individuals. This theory has intellectual roots in nineteenth century conservative social thought from de Tocqueville to Durkheim, but more recent formulations are just as likely to be found among Marxists. In reference to cultural production the theory has been most fully elaborated by the Frankfurt School of sociology (Swingewood), and in particular by Adorno in his work on music. To Adorno, the extension of the capitalist market principle of commodification to music threatens to destroy the heretofore relatively autonomous creativity of a high (fine) art. He claims that popular music, a product of the "culture industry," has lost the uniqueness and critical potential (negativity) that marks great works of art. Furthermore, Adorno believes that listeners are reduced to merely passive consumers and no longer are connoisseurs.

Adorno's work (and that of others as well) has laid the foundations for the development of more empirically oriented theories of cultural production, one of which is the "massification" hypothesis. Peterson and DiMaggio divide this hypothesis into two parts: first, increased modernization leads to reduced cultural diversity; and second, an increasingly homogenous mass culture emerges. The authors argue that the massification hypothesis can be tested with data on the evolution of a cultural form and they found support for the first part of the hypothesis with data on the change from folk to commercial country music. On the other hand, the second part of the hypothesis was only supported in their data for some time periods (up to the middle 1920s).

Similarly, in studying the transition of jazz from a black folk music to a popular music, Peterson (a) has emphasized that changes in the popular music industry (including its audiences and industrial structure) were part of an underlying process that shaped jazz as well. Peterson shares with other sociologists of jazz like Berger and Leonard (a) what Nanry has

called an "assimilationist" perspective. I would argue that the assimilationist perspective can be subsumed under the theory of mass society, whereas the other major theoretical perspective on jazz, the subcultural, can be subsumed under the theory of group action.

### The Theory of Group Action

The theory of group action is an orientation which has been most fully developed by sociologists in the Weberian tradition (Bendix, a, b; Collins; Rex) as an explicit alternative to systemic theories of society, such as the theory of mass society. The theory of group action focuses on the purposive social action of groups within historically specific institutional configurations. Three types of groups are distinguished (social classes, parties, and status groups) and each type struggles for control of economic (market), political, and cultural resources, respectively. Status groups would be particularly relevant when applied to the study of cultural production, since they are formed on the basis of a shared cultural identity. The importance of the other two group types would be historically variable. For example, social classes would be important to the degree that market institutions are involved in cultural production (Bendix, b).

The theory of group action has most successfully (but only implicitly) been applied by historians who have studied a range of black American cultural expressions (Gutman; Levine). In his work on black folk-thought from slavery to World War II, Levine has shown the tenacity of black folk culture even under radically changing social conditions. This folk culture should not be conceived as static, but rather as Levine observes, as a dynamic one under substantial control by its producers. According to Levine, blacks were ambivalent about cultural assimilation into the mainstream of white American culture and a "centripetal urge . . . continually threw them back to central aspects of their traditions even as they were surging outward into the larger society. It was precisely because periods of increased opportunity and mobility posed the greatest threats to whole layers of black cultural traditions that such periods often witnessed important manifestations of cultural revitalization" (445).

Whereas Levine's unit of analysis is the culture of a particular status-group, we can also find a group action orientation employed in studies of a particular cultural form, such as jazz. In these studies cultural producers are seen as members of either occupational subcultures or (community-wide) status groups. Most sociologists of jazz have taken the subcultural perspective (Becker; Cameron; Merriam and Mack) whereas the status-group perspective is implicit among those who emphasize the degree to which jazz has remained a distinctively black music throughout its history (Jones; Sidran; Walton). Some sociologists have criticized the subcultural

perspective for being ahistorical (Harvey) and for ignoring musicological distinctions (Horowitz and Nanry; Leonard, b). However, a middle ground has emerged where both status-group and subcultural features are taken into account in studies of changes in musical content. For example, Peterson's (a) analysis of the transition from popular to fine art jazz in the early 1940s shows how the stylistic revolution at the time (be-bop jazz) came about through the deliberate rejection of many musical and social features of popular jazz by a generation of young black musicians.

### **Research Design and Data**

As stated above, the theories of mass society and group action are orientations which provide alternative, but not mutually exclusive, explanations for changes in cultural content. By examining changes in a single cultural form over time (Peterson and DiMaggio) we can assess the relative power of the two explanations, if the processes specified in each of the two explanations converge during the time period. The case of jazz between 1920 and 1940 provides an appropriate empirical test for the following reasons:

1. This period saw a major stylistic change with the shift from small to big band jazz. This raises the sociological question as to what theory of cultural production can best account for this change in musical content.
2. The theory of mass society could have some explanatory power, since it was at this time that the audiences for jazz changed from those of the black subculture to the mass of Americans. At no other time in its history has jazz been so widely popular.
3. The theory of group action could have some explanatory power, since jazz musicians were still disproportionately recruited from a distinct status-group (black Americans), which at the time was redefining its position within American culture.

To test these explanations I collected data on black jazz, black musicians, and its audiences in Detroit between 1920 and 1940. The focus on Detroit is justified on two grounds. First, Detroit was an important center for the new jazz style (big band jazz) and was on an equal footing with Chicago and Kansas City, second only to New York. Second, primary data were more easily accessible to me in Detroit and my familiarity with data sources greatly simplified the task of data collection. Similar data for New York or Chicago still remain to be collected. I will refer to the specific data sources I used in the body of my analysis of black jazz in Detroit.

I begin this analysis with a musicological examination of the stylistic changes in jazz and continue with a test of the two sociological explanations used to account for these changes. To test the mass society explanation I will relate the stylistic changes to changes in the system of musical



production; whereas to test the group action explanation I will use the social location and orientations of musicians as a group as explanatory factors.

### Stylistic Changes in Jazz

In 1920, the chief exponents of jazz style were small bands found not only in now legendary New Orleans but throughout the U.S. The basic features of their music were the use of improvisation, the blues, and "hot" or "swinging" rhythms. By the middle of the decade a radical change in jazz style was clearly underway as "big band" jazz replaced "small band" jazz.

I collected three sorts of data in Detroit to describe these stylistic shifts: recordings, interviews, and secondary sources. Since only two of Detroit's black bands had a chance to record during this period, those who heard the bands play in Detroit are an important source of information. Therefore I relied heavily on the recollections of seven black musicians who were active in the city during the 1920s and 1930s.<sup>1</sup> In addition, I used existing secondary sources on the more well-known Detroit bands.

Within the larger shift from small to big band jazz three stylistic changes can be isolated: the introduction of arranged big band music modifications in the format of the band, and changes in rhythm and the use of improvisation. The introduction of arranged big band music was the major influence exerted by the "society band" tradition, to which we will return, on the development of jazz. Other features of the dance band format in the society band tradition were modified leading to a redefinition of American dance music as well as the jazz tradition itself. In the hands of jazzmen, dance band instrumentation was changed (Hennessey) and new harmonic and melodic possibilities were realized (Wen Shih). These changes in instrumentation involved the dropping of the violin, the tuba, and the banjo and replacing them (partly) with the guitar and the string bass. These changes allowed for a lighter, "swinging" or "hot," rhythm to evolve within the jazz tradition. In turn, to make arrangements "swing" the jazzmen redefined the rather mechanical notions of ensemble playing found in the society band tradition and brought them closer to those applying to the jazz soloist (Hobsbawm). This also gave improvisation new meaning as arrangers juxtaposed soloists against the band, or one section of the band against another. It is a common observation in histories of jazz that the most influential arrangers were those who were able to maintain individual creativity in ensemble as well as solo work (Hobsbawm; McCarthy). Thus, the creation of a "big band sound" was a collective effort on the part of several members of the band, even though it took a different musical form from the "collective improvisation" that characterized New Orleans jazz.

Two major musical strands preceding the shift to big band jazz can be isolated as major influences on the music: the black folk music tradition and the society band tradition. In the early 1920s small band jazz style was still largely a part of black American folk music. The music of Detroit's small jazz bands was largely improvised and their repertoire was a mixture of show tunes and dance numbers which were played with blues overtones and "hot" or "swinging" rhythms (Chilton, b; Hoefer). Whereas improvisation, the blues, and "hot" rhythms were musical characteristics closely associated with black Afro-American folk music, the use of show tunes and dance numbers indicate that even early jazz was influenced by existing European-American traditions. Jazz has throughout its history developed in interaction with European-American music; moreover, musical influences have to a large extent been reciprocal (for a recent discussion, see Nanry).

The second major influence on big band jazz in Detroit was the city's black society band tradition.<sup>2</sup> These bands often played for formal occasions such as weddings and anniversaries, but also in public ballrooms. Detroit's black society bands had a mixed repertoire of show tunes and dance numbers and the music (and audiences) of these bands differed very little from that of the white "symphonic" dance bands of the day (McCarthy). The black society bands formed the major conduit for European-American influences on the emerging big band style in Detroit and the jazzmen who emerged from this tradition in the early 1920s had a strong familiarity with arranged formats which allowed little or no improvisation. It was the combination of these elements with those of the folk jazz tradition (improvisation, the blues, and "hot" rhythm) which fused into a totally new style: big band jazz.

### **The System of Musical Production: Audiences**

The degree of integration of black bands into the system of popular music production can most easily be measured by the racial composition of their audiences. In a regional center for jazz such as Detroit, the audiences for local bands were found mainly in various dancing establishments, rather than among the record- (or radio-) listening public. I used a number of different types of data to determine which dancing audiences were catered to by which black bands in Detroit between the two World Wars: my interviews with surviving musicians referred to above; newspaper reports and advertisements mainly from the black press<sup>3</sup>; and secondary sources (Chilton, a, b; McCarthy).

To depict the process of integration of black bands into popular music production I related the changing stylistic features of these bands to the racial composition of their audiences, as shown in Table 1. The unit of

**Table 1.** RACIAL COMPOSITION OF AUDIENCES FOR DETROIT BLACK SOCIETY AND JAZZ BANDS, 1917-23, 1923-29, AND 1930-39 (ABSOLUTE FREQUENCIES)

Band Style	Racial Composition of Audience			Number of Bands
	All White	Majority White	Majority or All Black	
<u>1917-22</u>				
Society band	6			6
Small jazz band			2	2
<u>1923-29</u>				
Society band	4			4
Big jazz band	1	3	2	6
<u>1930-39</u>				
Society band	1			1
Big jazz band	4	13	7	24

analysis is the band and each frequency count represents the predominant type of audience played for by a particular band in a specified time period. The bands are classified according to musical style into jazz and society bands, and since there was an evolution of jazz style over time, the two types of bands are further compared across three time periods. These time periods are based on Hennessey's work, with 1917-22 representing early small band jazz, 1923-29 the years of the development of big band jazz, and the 1930s the diffusion of this style nationally.

Table 1 indicates that as jazz bands changed stylistically from small to big band music after 1922, their audiences also changed from predominantly black to white. It also depicts how in the 1923-29 period the big jazz bands joined the black society bands as mass popular bands and in the 1930s almost completely replaced them. The issue now arises as to what degree these patterns of musical and social change can be seen as causally related, e.g., to what degree are the stylistic changes in black jazz explained by the increased integration into the production of popular music?

In my musicological analysis above I isolated the stylistic changes that took place with the emergence of big band jazz; and of these changes the use of an arranged big band format would seem, given its European-American roots, to be a prime candidate for a causal link between popular music production and the big band jazz style. After all, black jazz bands adopted the big band format as ballroom dancing increased in popularity in the 1920s and the likely musical sources for this format were the black and white society bands that dominated (white) ballrooms in Detroit until the middle of the decade. On the other hand, as I also argued above, this

general format was modified in the hands of the big band jazzmen and there is evidence that the motivation for this was, at best, only partly commercial. These modifications of the big band style were sufficiently subtle for the masses of dancers (and various market-censors) to be unaware of their existence. Most likely, as Peterson (a) argues, it was the *rhythmic* elements of big band jazz that contributed to its popularity as a dance music. These rhythmic elements did not originate with the popularity of the new dance music, but rather were part of the long-term evolution of jazz style. Peterson's generalization is given support by my Detroit data. They show a decline in the popularity of the black society dance bands and the musicians I interviewed agreed that one cause of the decline of these bands was their rhythmic stiffness in comparison with the big jazz bands.<sup>4</sup>

In all, the integration of black jazz into the popular music market during the 1920s can only partly account for the new stylistic features of the music. The use of some form of arranged big band dance music is the one feature of big band jazz that can plausibly be seen as resulting from the changing market conditions for black music. To arrive at a more complete explanation for the stylistic innovation of big band jazz let me turn from the theory of mass society to that of group action.

### Group Action: Black Jazzmen

To examine the power of the group action explanation I will first describe the social location and occupational orientations of black jazzmen and then relate these to stylistic features of their music. I will capture the social dynamic involved through a comparative analysis of black jazzmen with their black predecessors (society band musicians), as well as with their white contemporaries.

#### SOCIAL LOCATION

I measured the social location of black jazzmen along two dimensions of stratification: market situation (social class) and prestige (status group). With regard to the market situation for black jazzmen in Detroit, census data show that black musicians faced job insecurity more than white musicians. For example, the total number of employed musicians declined between 1930 and 1940, but more drastically for black musicians (22% as compared to 10%).<sup>5</sup> My interviews with black jazzmen also indicate that better paying jobs went to white musicians between the two World Wars. In all, a familiar picture of market disadvantage for black musicians (Hobsbawm) also emerges from my Detroit data.

Whereas black jazzmen occupied a lower social class position than

white jazzmen in the city at large, other data indicate that they simultaneously received considerable prestige within the black community. When I compared weekly black and white (mass) newspapers<sup>6</sup> from 1920–40 I found that while there was virtually no attention given either black or white musicians in the white press, the black jazzmen and their music were given sole and positive attention in the Detroit black press.

Detroit's black jazzmen were also disproportionately from northern rather than southern backgrounds and this is likely to have contributed to their relatively high prestige within the black community. I compare the regional background of a sample of Detroit black jazzmen to that of the black population as a whole (1930) in Table 2. The data indicate that the regional background of the sample of jazzmen differed significantly<sup>7</sup> from that of the black population as a whole. Underrepresented among the jazzmen were migrants from the Deep South, which is consistent with earlier, but less systematic, studies of the regional background of the jazz innovators of the 1920s. According to Hsio Wen Shih, the typical jazz innovator of the decade was from the southern border states rather than the Deep South, and also from a middle-class background (Wen Shih; see

**Table 2.** REGION OF BIRTH OF DETROIT'S BLACK JAZZMEN AS COMPARED TO THE TOTAL BLACK POPULATION OF DETROIT IN 1930 (IN PERCENT)

Region of Birth	Black Detroit Jazzmen (N=55)*	Total Black Population (N=117,467)**
East North Central***	36.3	19.8
Michigan	21.8	14.4
South Atlantic†	23.7	36.5
East South Central‡		
Ky., Tenn.	10.9	11.5
Miss., Ala.	9.2	19.4
West South Central#	10.9	7.9
La.	1.8	5.5
Middle Atlantic##	7.3	1.8
New England###	1.8	.2
Other	0	2.9

\*Data from Chilton (a,b), McCarthy, and interviews with Detroit musicians.

\*\*Data from U.S. (b, 216-18).

\*\*\*Oh., Ind., Ill., Wisc.

†Del., Md., D.C., Va., W. Va., N.C., S.C., Ga., Fla.

‡Ky., Tenn., Ala., Miss.

#Ark., La., Ok., Tex.

##N.Y., N.J., Pa.

###Me., N.H., Vt., Mass., R.I., Conn.

also Hennessey). This contrasts with the preponderance of musicians from New Orleans in early forms of jazz.

The social significance of regional background within the Detroit setting was commented on by contemporary observers. Forrester B. Washington argued that within the black community in Detroit, place of birth was a more significant criterion of prestige than was income. The social significance of place of birth in black Detroit is not surprising against the background of its dramatic population growth from migration.<sup>8</sup>

#### OCCUPATIONAL ORIENTATIONS

As jazz became a popular dance music in the early 1920s we would expect, *ceteris paribus*, jazz musicians to adopt more of a "commercial" or "craft" orientation towards their work than before. Judging by the existing literature, however, it seems that white and black jazzmen adopted different orientations; and the data I collected for this study allow a further exploration of the meanings and social sources of these differences.

Using concepts most recently refined by Becker (b, c) in a series of articles, the differences between white and black jazzmen were those between "artists" and "artist-craftsmen," respectively. Furthermore, black jazzmen were able to define their art as a "folk art" and/or a "fine art," whereas white jazzmen were constrained to the latter occupational orientation.

There is a relatively large sociological literature on the views that white jazzmen held of their work in dance bands in the 1920s and later (Becker, a; Leonard, b; Merriam and Mack). These jazzmen saw themselves as frustrated artists in a commercial setting and they had a negative view of dance band musicians and their audiences. There have been fewer and less systematic studies of black jazzmen, but they provide some evidence for a different orientation towards work in dance bands. Hennessey's review of existing biographical sources leads him to the conclusion that black jazzmen in the 1920s and 1930s saw the musical profession not only as an artistic one, but also as a profession appropriate for those with aspirations of upward social mobility (Hennessey; see also Lax). Big band membership was deemed appropriate since it required considerable amounts of musical training and experience, as well as a craftsman's view of the commercial aspects of the occupation. According to Becker (c), the craftsman's orientation to work stresses his possession of the knowledge and skills necessary to produce objects that are useful to clients, customers or employers (see also Peterson and White).

My interviews with Detroit jazzmen give further support to existing studies of black jazzmen and can be fruitfully compared with studies of white jazzmen. The goal of such a comparison with white jazzmen will be to further explore the sources and meanings of the occupational orientations of black jazzmen. My Detroit data lend themselves to an exploration

of the sources of these orientations because most of the musicians I interviewed had at some time played in the black society bands. Given the similarity in the music (and audiences) of black and white society bands, any differences in occupational orientations between black and white jazzmen in these bands<sup>9</sup> are more than likely due to extramusical variables associated with race. I would hypothesize that one such variable is the differential position of blacks and whites in the system of stratification.

When I interviewed the black Detroit jazzmen about their experiences in society bands they stressed how black society band musicians served as models of craftsmanship. This is illustrated in the following recollections by one Detroiter:

. . . there was a fraternal feeling among the musicians. As a young musician coming out at high school age, when I would come in contact with those fellows they would treat me as a fledgling bird and showed me the ropes and would teach me things I should do in order to be a successful musician. And the things I should not do. They would talk to you in regards to your health, and how you should eat and how you should not dissipate, how you should rest in order to keep abreast of what was happening. And also . . . you should practice your instrument at least so many hours a day.

The Detroit jazzmen also spoke with pride about how society bands were able to perform a music that required musical training and experience. One of them remembers that

. . . we could play anything . . . we were thorough musicians . . . back then, man, musicians were thorough, there were no hams, like these guys nowadays . . . we were trained musicians . . . a lot of time the Gaiety would not have the pit band down there and we'd come in and play that show at sight, no rehearsal. . . .

And another musician remembers

I became an all-around musician, that was a term that was used in that day and that is what I aspired to do. Any type of situation musically I was able to fit into.

This stress on craftsmanship among black jazzmen in a society band setting takes on additional significance when contrasted with the orientations of the white jazzmen described in the literature. Whereas both the black and white jazzmen had improvisational and rhythmic skills that their fellow society band musicians did not have, only the white jazzmen chose this difference as the sole basis for their occupational identification. In contrast, the black jazzmen came to share a craftsman's orientation with their fellow society band musicians, in addition to adopting an artistic orientation.

The saliency of craftsmanship to black rather than white jazzmen in similar occupational settings (society dance bands) is probably explained by the relatively depressed market situation for black musicians described above. While a test of the implied causal link between market situation,

mobility aspirations, and occupational orientations is beyond the confines of the data at hand, Hennessey's study gives preliminary support to such an interpretation.

The black jazzmen retained this craftsman's orientation as they left the society bands for the big jazz bands. In this musical context, this orientation came to serve a similar musical function, i.e., it sustained the execution of almost any arranged dance music. It is often forgotten in "pure" jazz histories that even the very best jazz bands of the day offered an assorted dance repertoire varying with the demands of audiences and market censors (Hennessey). The following quote from my interview with a veteran of one of Detroit's foremost jazz bands shows how a "commercial" view of one's audience could be consonant with one of the major artistic qualities of the jazz musician, his improvisational skill:

. . . we gave the people what they wanted, we played requests. . . . every time you satisfy a crowd you get a crowd, I don't care who you are. . . . A lot of bands play for themselves, in fact they can't come out of their repertoire, they can't play nothin' else. . . . When we had a request outside our repertoire, if we knew the song, the whole band could play it without even no music, make an arrangement out of it right then and there, you know. . . .

Thus I find no inherent musical contradiction in the simultaneous adoption of a craftsman's orientation (with "commercial" or popular elements), and a more artistic (folk or fine art) orientation. In more general terms, the viability of such an artist-craftsman orientation has been shown by Becker (c).

The tendency for black rather than white jazzmen to adopt a craftsman's orientation was probably, as I argued above, the outcome of different positions within the system of stratification. With regard to the artistic component of this "dual" orientation it is of course the case that black jazzmen were more likely to see this artistry in black folk art rather than in fine art terms.

#### GROUP ACTION AND STYLISTIC CHANGE

The social location and occupational orientations of black jazzmen can now be related to the stylistic changes in jazz. Earlier I showed that changes in the system of musical production could only be a causal influence on one of these: the development of arranged big band music. This leaves two important stylistic features to be accounted for: the modifications of the big band format and changes in jazz improvisation and rhythm.

One of the strengths of the theory of group action is its recognition of purposive social action, which within the realm of cultural production would include artistic creativity. Such creativity is sometimes conceptualized as the solution to aesthetic problems (tensions, contradictions) posed



by the difficulty of reconciling seemingly irreconcilable artistic conventions (Swindler). These conventions are socially grounded within artistic communities and groups and sometimes also in society at large.

This theory of artistic creativity is implicit in Hobsbawm's discussion of the emergence of big band jazz. The musical problem was the reconciliation of arranged big band music with the rhythmic and improvisational features of jazz, and its solution consisted of modifications in instrumentation, harmony and melody. Artistic conventions for the performance of an arranged big band music were found only within the society band tradition and the black jazzmen set out to adapt these to the central conventions of the jazz tradition (regarding rhythm and improvisation). That this particular juxtaposition of artistic conventions was a musical condition for the development of big band jazz can be seen if the accomplishments of black big band jazzmen are compared to those of their black predecessors and their white contemporaries.

The big band jazzmen shared generational experiences which set them off from black society band musicians. While the big band jazzmen spent some time in society bands, they were younger than other society band musicians and more able to incorporate the black folk music tradition in their playing at a relatively early age. This folk music tradition was not generally available to black musicians outside of the Deep South before the black migrations North around World War I. Thus the big band jazzmen were part of a new generation of black musicians who reached musical maturity at a time when black and white musical traditions intersected in cities outside of the Deep South.

White jazzmen also were familiar with the artistic conventions of society bands but their distance to black folk music made the musical problem less frequent or intense. Consequently, their role in big band jazz was largely derivative as they adopted the big band jazz format, commercially known as "swing," almost a decade later than the black jazzmen.

The emergence of big band jazz can not only be analyzed as the resolution of a musical problem, but also as a cultural change with a certain social dynamic. In this mode of analysis the new musical synthesis between American popular music and black folk music can be explained by the adoption of an artist-craftsman occupational orientation among black musicians. The big band jazzmen were different from earlier jazzmen in their adoption of a craft orientation toward their occupation, an orientation which many of the jazzmen encountered in their interactions with the black society band musicians. This orientation was probably maintained by the relatively high social standing that the jazzmen shared within the black community with the society band musicians. The artistic component of the "dual" occupational orientation was part of the black musical heritage including that of early jazz, whereas the fine art orientation developed with the emergence of a musicians' subculture in the early 1930s. During

these years I found in Detroit's black community the establishment of a number of clubs that can be understood as an organizational feature of jazz as a "musicians' music" (Cameron). These clubs were of two sometimes overlapping types: those that were used for after-hours "jam-sessions" and those that served as rendezvous places for musicians (Björn).

The artistic and commercial viability of the new style of jazz for black musicians is evidenced by the narrowing of the musical spectrum as the black society band tradition was permanently displaced in the 1930s (see Table 1). In contrast, the white society bands proved more lasting and remain with us today. Thus the narrowing of the musical spectrum in the black community cannot be explained as part of a general trend. What distinguished the black big band jazzmen was their ability to redefine their folk heritage within the social context of the 1920s, and in so doing to bring about a cultural reintegration of the black musical community. I would argue that it was their role as "cultural synthesizers" that made black jazzmen less likely to adopt the marginal occupational orientations found among their white counterparts.

The achievements of the new generation of black jazzmen can also be fruitfully compared to those of other black artists during the 1920s. In his work on the Harlem Renaissance, Huggins concludes that the black jazzmen were more successful than other black artists in defining a new cultural identity for American blacks. This artistic (as well as commercial) success was the result of the development of an aesthetic which was less influenced by dominant artistic standards (European fine art), and was closer to the black folk tradition than other black cultural expressions (see also Lax).

## Summary and Discussion

In this paper I used two theoretical orientations to cultural production to account for the stylistic changes that occurred in jazz between the two World Wars: one based on the theory of mass society, the other on the theory of group action. Whereas the theory of mass society would explain these changes in musical content in terms of changes in the system of musical production, the theory of group action would use the purposive action of musicians as a group as an explanatory factor.

My data on black jazz in Detroit between 1920 and 1940 give more support to the group action explanation. Of the three major stylistic changes isolated in a musicological analysis only one is causally related to changes in the system of musical production (as seen in changes in the audiences for black jazz): the development of some form of arranged big band dance music. The specific musical form that this big band music took and the continued evolution of jazz improvisation and rhythm are stylistic features

that can better be explained by knowing who the performers of the new music were, rather than by how the system of popular music production was organized.

I investigated two aspects of the stylistic innovators (the black big band jazzmen): their social location and their occupational orientations; and the latter was then related to the stylistic features of the new music. Through a comparative analysis of white and black musicians I showed that the artist-craftsman occupational orientation adopted by black jazzmen had its social sources in patterns of stratification.

The causal efficacy of occupational orientations can be demonstrated if we start from observations about the patterning of musical influences. As a new generation of black musicians in Detroit, the jazzmen only selectively borrowed from the European-American musical tradition of the older black society dance band musicians. Rather than perpetuating the popular dance music of their black predecessors, the jazzmen created a new synthesis of American popular music and the black (folk) jazz tradition. This was a creative effort since it involved modifying the European-American dance band tradition within the improvisational and rhythmic features of the black (folk) jazz tradition. This cultural innovation was made possible by the adoption of a craft occupational orientation, that was shared by the black society dance band musicians; and an artistic occupational orientation, based on the black folk tradition and an emerging fine art tradition.

I also found evidence that the black jazzmen brought about a cultural reintegration of the black musical community through their redefinition of the black folk heritage within the social context of the 1920s. With a great influx of black migrants to northern cities, the 1920s was a period for cultural redefinition on the part of American blacks. As members of the northern middle class, many black jazzmen were put for the first time in contact with the folk traditions of migrants from the South (Jones). By taking on the role as "cultural synthesizers" the jazzmen were able to solidify their prestigious position within the black community and avoid the marginal social position that characterized their white counterparts. Thus I would argue that the cultural action of a group is not simply determined by the location of that group within patterns of social stratification, but it also shapes (as purposive action) these patterns.

The greater empirical support for the group action explanation as compared to that based on the theory of mass society has implications for the sociological study of jazz and more generally, for the study of cultural production. In the only major effort to synthesize the findings of micro and macro approaches to jazz, Peterson (a) has argued that the evolution of jazz can be explained by the movement from folk-to-popular-to-fine art music. This paper has revised this analysis by showing the limited degree to which jazz style was influenced by the system of musical production as a black folk music came in contact with American popular music in the early

1920s. Rather, black musicians maintained substantial artistic control as, first, their music was marketed to popular audiences and, second, as others have pointed out, jazz maintained several folk music characteristics within the fine art context, most importantly the artistic control exercised by musicians rather than composers or arrangers. My revision of Peterson's analysis suggests that a further task for a historically grounded sociology of jazz would be a comparison between the interwar years and the 1970s, when jazz became increasingly popularized, but at this time probably more distant from its folk roots.

With regard to the study of cultural production (Peterson, b) I have tried to show the relevance of a traditional sociological orientation (the theory of group action) which by its recognition of purposive action avoids the determinism of many sociological approaches to culture (see also Barrett et al.). I analyzed cultural innovation as an outcome of the efforts of various social groups (status groups interacting with social classes) to resolve problems of cultural identity (cf. Swindler, 15–16). Specifically, a new generation of black jazzmen were seen resolving the musical problem of reconciling elements of their musical environment through the creation of a new musical style. My empirical findings also point to questions worth further research. For example, the limited degree to which this single black American folk/fine art has been affected by commercialization raises questions about which social conditions facilitate various patterns of resistance (involving various combinations of folk/fine art forms) by artists. Only a systematic comparative analysis of a number of folk forms under varying social conditions, or of fewer folk forms under (historically) controlled social conditions, could begin to answer such a question.

## Notes

1. My interviews are part of an ongoing project on the "History of Jazz in Detroit" under the auspices of the Jazz Research Institute in Detroit (see also Bjorn; Weinberg and Boyd). The format of these interviews was that of a focused interview centered around each musician's biography, the music and musicians of all Detroit bands, locations where the music was played, and audiences played for. All interviews were taped in their full length which varied from three to six hours divided into two or three sessions.
2. For an excellent discussion of these bands in New York and Chicago, see Hennessey.
3. Various years of the *Detroit Tribune*, *The Tribune Independent*, the *Detroit Independent*, the *Paradise Valley News*, and the *Detroit Contender*.
4. Similar causes for the decline of black society bands are given by Lax in his study of black bands in Chicago during the 1920s. Lax also to a large extent relies on interview data to reach these conclusions.
5. Unemployment figures are available only for 1930 and 1940 (U.S. 1933, 1943). I could not arrive at a precise estimate of the proportion of musicians that played in dance or jazz bands during the period on the basis of census data. However, there are reasons to believe that a majority of the black musicians were jazzmen (Berger). Using primary and secondary sources I generated a list of 111 jazzmen by name, which is more than half of all black musicians in 1930. The difficulties in identifying the total population of black jazzmen by name are considerable and most certainly this number is an underestimate.
6. The black weeklies were those mentioned in Note 2 and the white weekly was *Detroit Saturday Night*.

7. Using the statistical  $\chi^2$ -test the null hypothesis that the sample of musicians was drawn from the total black population was rejected.  $\chi^2$  was 18.65 with 6 degrees of freedom which was significant at the .01 level of significance.
8. During the years of World War I the black population increased almost sixfold (from 7,000 in 1915) and by the middle of the 1920s it was twice that of 1920. During the 1930s the black population grew at a faster rate than the total population of the city to reach 149,000 (or 9%) by 1940 (Holli).
9. Both black and white jazzmen were hired as "hot" (jazz) soloists in society bands.

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# Neither Socialization Nor Recruitment: The Avocational Careers of Old-Car Enthusiasts\*

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## ABSTRACT

*This paper investigates the adequacy of socialization and social influence processes in accounting for the development of leisure interests. Data come from intensive interviews (N=40) and structured interviews (N=189) with antique automobile enthusiasts. Four major patterns of entry into this world are identified; the most important of these suggest that interpersonal influence alone cannot explain involvement in such activities. I suggest that Berger's conception of "underinstitutionalization" of the social environment and its subjective correlate, "subjectivization," provides an explanation for the findings. It is argued that this process may have wider applicability in studying leisure activity and social participation, and constitutes a form of social-structural effect that is distinct from socialization and social influence.*

## Socialization, Social Influence and Leisure Activity

Within the sociology of leisure, a number of studies have indicated that leisure preferences and activities in adulthood are affected by early-life socialization (e.g., Kelly, a; Sofranko and Nolan; Spreitzer and Snyder; Yoesting and Burkhead), and by family, friendships and other social influences in the immediate environment (Kelly, b; Spreitzer and Snyder; Wonneberger.). In the related area of voluntary association research, studies of how individuals come to join or participate in a given organization have also been formulated in terms of socialization (Hodge and Treiman; Smith and Baldwin) or social influence (Althoff and Brady; Babchuck and Booth; Fenn). In the case of social movement organizations, some support

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has been found for social network explanations of differential recruitment (Snow et al.; see also Marx and Wood). As applied to voluntary associations, the assumption underlying these processes was stated in its most explicit and extreme form by Palisi:

Since individuals and groups are always influenced by social group pressures there is no free choice, there is no voluntary behavior. . . . Any person who participated in any group does so because of some type of social pressure. Sociologically, we have to assume that the social pressure was great enough to influence him to participate since he is, in fact, doing so (400).

While these studies vary considerably in methodological sophistication and in the magnitude of the effects reported, their findings converge in indicating that individual leisure activities and social participation are affected by socialization and social influence processes.<sup>1</sup> From the vantage point of a general explanation of leisure activity patterns and social participation, however, the accumulated evidence provided by these studies has some significant limitations.

One limitation concerns the types of leisure activity and social participation studied. Within the sociology of leisure, findings about the effects of social influence are based largely on outdoor recreation activity and sports involvement (see Kelly, c; Smith, b). On the other hand, studies of social influence in voluntary association participation has focused almost entirely on "instrumental" voluntary associations—those with civic-oriented, publicly valued, or political objectives (Sills). "Expressive" associations—those which exist for the sake of the members' enjoyment—have been neglected. Thus, the findings regarding socialization and social influence processes in leisure activity suffer from a "sampling bias" with respect to the kinds of activities and social participation that have been studied. An enormous diversity of kinds of activity occur in leisure time, and an enormous diversity of organizations fall under the heading of voluntary association. It is unlikely that the same kinds of social processes operate uniformly across such diversity, yet the question of how the process of becoming involved may differ by types of activity or organization has received little attention.<sup>2</sup> The study reported here addresses this neglect by exploring how people become "old car buffs," and how they come to participate in antique car clubs. How adequate are the models of socialization and social influence in accounting for involvement in this form of expressive activity?

### **The Passionate Enthusiasm of Old-Car Collectors**

An underlying characteristic of the social world of old cars that must be initially explicated is the intensity of feeling directed toward the object



around which social interaction is focused and formal activities are organized. I have elsewhere (b) detailed the types of activities and corresponding meaning-structures that make old-car enthusiasm an experience of intense subjective engagement, or "flow."<sup>3</sup> Our approach to the question of how people become involved in old-car activities and associations is greatly enhanced if we begin with an appreciation of this intense quality, because in this fundamental respect old-car enthusiasm is not necessarily a *social* activity in the conventional sense—a point that will be elaborated below.

For many car buffs, the topic of old cars dominates both thought and activity in nonwork time, and sometimes at work as well.<sup>4</sup> They freely acknowledge that their intense feeling and commitment do not make sense in a way that can be rationally communicated, and when pushed on this point, disarmingly dismiss the question with responses like "I'm just crazy" (see Dannefer, b). Moreover, unbridled expressions of enthusiasm, affection, and even of reverence, characterize the orientation of the enthusiast toward the car. These themes are reflected in the following account offered by a man who had toyed with the idea of selling his Rolls Royce, then decided to keep it:

I excused myself and walked out to the garage for a minute of reverence. Looking at my dear and magnificent Phaeton, I realized that it was a heinous idea (Radcliff, 146).

The manifest subjective intensity is crucial for understanding the experience and commitments of old-car buffs. People do not become mere car enthusiasts, they become *passionate* enthusiasts. This paper explores how such intense commitments emerge biographically and interpersonally.

## Data and Procedures

Data for this study are from intensive interviews (approximate length two hours) with a sample of 40 active old-car enthusiasts/club members, followed by structured interviews obtained from a systematic random sample (N=189) of participants at a national gathering of the old-car world. Intensive interview respondents were selected from the rosters of four local chapters of antique automobile clubs in New Jersey, and interviewed in 1976. The survey was conducted to investigate further the preliminary findings resulting from the intensive interviews. All subjects were males, aged 21 to 73. Thirty-eight of the forty interview respondents were white, and all of the 189 survey respondents were white.<sup>5</sup>

### Pathways to Enthusiasm

To investigate this issue, I asked the respondents detailed questions about how their enthusiasm for cars came about and how they came to be involved with cars and with car clubs. The intensive interview data yielded a clear-cut typology of "pathways to involvement," which was refined and modified only slightly by data from the survey sample. The major findings of the survey were consistent with the patterns which emerged from the intensive interviews. Four main types of respondents were identified:

1. *Youth fascination* buffs avowed a strong and abiding interest in cars from early life onward, often from earliest childhood memories. Those who fit this pattern deny almost totally that they experienced social pressure, or even that they received any direct encouragement, to cultivate a passion for cars.
2. *Late bloomers* claim to have had the same passionate interest in cars in youth, but it dissipated or was suppressed from about age 20 until middle life when it was resumed.
3. *Family heirs* grew up in families where father or both parents were car buffs.
4. *Hitchhikers* include men who traveled several different routes to involvement. They have in common a lack of passion for cars per se. Initially, at least, they were more interested in the opportunities for social life that the social world of old cars presents. At most, they report having had a casual interest.

The distribution of enthusiasts among these four types is presented in Table 1.<sup>6</sup> A few respondents fit none of these categories.<sup>7</sup>

#### YOUTH FASCINATION

The accounts of more than half of the interview respondents led to the identification of the youth fascination pattern. I will quote several of them to convey the mode of experience represented by this pattern, and to document the consistency with which it recurs:

I wouldn't call it a hobby . . . I was just very interested in cars, from when I was very young, even before I went to school . . . at least that's what I'm told . . . Who knew what it was, I just liked cars, you know. I could name every car. When I was four years old I could name ya every car and the year of it goin' down the street . . . as far back as I can remember . . . The first thing I ever did was to get magazines, and cut the ads of the cars out, and cut the cars out, just like a little girl would cut out paper dolls, and I would have a collection . . . I had a whole box full of cut-out cars . . . and I would look at 'em . . . that's how it started. I started collecting (auto sales) literature when I was nine years old.

**Table 1.** DISTRIBUTION OF SURVEY RESPONDENTS  
ACCORDING TO PATTERN OF INITIAL INTEREST (IN PERCENT)

Youth fascination	50
Late bloomers	13
Family heirs	13
Hitchhikers	16
Other	<u>8</u>
	100

(N = 189)

I was interested in trains first. I remember riding along the road and my Dad was driving me to school, and I asked him, how does a car work; and that was the last time I was interested in trains . . . At age ten I was driving. I knew how an engine worked before I was nine.

(Interviewer: Was your Dad interested in cars, too?)  
Oh, no, not at all. It got to where he didn't want me to be interested either.

(Interviewer: Did you ever lose the interest for a while?)  
No, it got stronger . . . I was interested in other things at the time, a typical teenager, but still cars were the number one.

Oh, I'm a car nut. I mean, I've always been, I'll always be. Ever since, I guess, I was old enough to drive I've owned two cars, at least two cars at a time, nothing ever under that. Sometimes three . . . I just always liked a real nice car.

Respondents conforming to this pattern generally began with an interest in cars in general, rather than one limited to antique cars:

I've always liked cars . . . ever since I was a kid I've always cut out from the newspapers all the brand new models that came out, the advertisements, and paste them all over my room . . . and I just loved cars and I think that led into old cars, not for any specific reason . . . I just fell in love with old cars; I'm very intrigued by the lines and style. I like cars in general and I like convertibles; I think the lines of a convertible or roadster are very beautiful.

I was the car nut in that school, I tell you . . . racing, antique, modern I didn't care. I liked it. At the time I even liked modern cars, and that was the start of the great turkey war.<sup>8</sup>

excused myself, and went on out in the country and looked at the car. And I liked it, and by the time I got out there it had stopped raining. And so I said, you know, I would like to buy it, and I said, of course, I said you know, all I got in my pocket is \$50. I would like to buy it if I could buy it for \$50. So he said, fine. So that was it. But then Doug came in, and that's how I got friendly with Doug.

Doug used to stop by the body shop and he saw that car, and he came in and inquired who it belong to, and tried to get me interested in the club, and I always said, I said to him, I'm not interested in any clubs. I'm really not a joiner; you know, even if I joined I said I probably wouldn't go. And I don't think I ever missed a meeting other than being sick. It's truly the only thing that I have been completely interested in.

I had my (aunt's old) Packard for sale, and he came over to look at the Packard, and that's how I really got to know him . . . I was buying a Model A the same time, so we just got to talking about Model A's and got to be friends . . . and he is the one that got me into the club, actually.

I was goin' down Route 22 one Sunday. I see this old Buick goin' down the road, it said 'Buick Club' on it. So I chased this guy all the way from Middlesex to Dunellen . . . I followed him and chased him tryin' to catch him. I finally caught up with him, blowin' the horn like crazy two cars back . . . So finally I got to talkin' to him and he gave me the address of the Buick Club of America—he was from some faraway place. So I started readin' about these flea markets and there was one (nearby) . . . So I went up there and I started just talking' to people, askin' for parts for a 31 Buick, and I met somebody up there and he said . . . 'There's a Buick Club right in Middlesex.' So then two, three weeks later, I was comin' down the street and I happened to look over and see an old car sitting there and then I saw another one on a trailer and I said let me follow this guy and talk to him and it turned out he was the President of the BCA in New Jersey.

Thus, the late bloomer parallels the youth fascination enthusiast in his early passion, and in the individually undertaken commitment that leads him to the social participation and social commitments of club involvement.

The essential characteristic of the late bloomer pattern is the experience during youth of a fascination which subsides or is suppressed for two or three decades, and reemerges in the middle years. The critical point, again, is that the process of becoming interested and involved in a social world does not depend on existing networks, but creates new ones.

#### **FAMILY HEIRS**

This pattern describes men who trace their own interest to childhood influence. Typically, their fathers or both parents had an interest in automobiles, either vocationally or avocationally. For example, one survey respondent said:

I was brought up in an automotive family. My grandfather owned a car agency. I would just say that cars have always been a family way of life, and I wouldn't want it to be any other way.

Another enthusiast of this type showed me toy cars and trucks his father had purchased for him when he was a child:

I guess there's no denying I got this from my father. He got me a lot of stuff like this and you can see how it's been worked over (painting), we used to work on these together all the time; we still work on cars together.

Another interviewee spoke of his interest and his father's in more competitive terms:

My father was a car buff, we always had 10 or 12 cars when I was growing up . . . so I grew up with it. I just got a car I had been wanting for years, a 49 Ford convertible . . . it's almost an identical twin to one he has, only a little rougher. It won't be when I'm through with it, though . . .

Enthusiasts who fit this pattern of experience typically report a biographical intense interest similar to those who fit the youth fascination pattern, as illustrated by the following quote from a self-described "automotive historian":

I have always been a car freak from as far back as I can remember. I used to tell the make year and model of any car, even when I was a very little kid, and it would blow their minds. I was known in the neighborhood as a child prodigy on cars.

Although the element of a deeply rooted, early interest is thus similar, these men clearly differ from those who fit the previously described patterns in the *process* by which the attraction for automobiles initially developed. Instead of developing independent of interpersonal relationships, the family heir enthusiast appears to be "socialized" in a way consistent with traditional psychosocial conceptions.

The contrast of this pattern with the previous ones is not limited to the initial generation of interest, but extends to the development of a car-centered life-style, including participation in events and club membership. Family heirs began going to car shows with their fathers or parents, hence were well-accustomed to the scene in childhood, before it had occurred to the youth fascination buffs to begin seeking out their old-car cronies.

In sum, then the process of becoming a car buff is fundamentally different for family heirs than for the two types presented earlier. Although the family heir type is consistent with the established wisdom of socialization theory, enthusiasts conforming to this sample comprise only about one-eighth of the survey sample.

### HITCHHIKERS

The final pattern describes respondents whose interests in the world of cars did not initially stem from a subjective passion, but (1) directly from a social relationship or experience, or (2) from a more or less explicitly acknowledged desire for sociability. Especially for those who acknowledge their interest in sociability, the world of old cars was approached primarily as a *social* world; the emphasis on cars appears to be relatively incidental. These men constitute a significant departure from the previous patterns in that the meaning of their involvement is largely social in its content as well as in its origin. It is the influence of social relations or social experiences that creates involvement in these cases. I call them "hitchhikers" because they lack, or at least initially lacked, the kind of passionate interest that typifies the old car hobby, and that was required for its emergence. They are riding the wave of involvement and enthusiasm that was the social production of others, of the genuine "nuts." One important kind of hitchhiker is the retiree:

I had a friend who got interested in early Fords, and he decided, no, there is too many of them. He gets into Chevy. So they were going to start a New Jersey Regional Chevrolet Club. He says, do I want to join. I said, what do I want to join for, I don't have no Chevy. You don't need one. I said, okay, I'll be a charter member.

(Interviewer: Did you have any old car then?)

Had no old car at all. So . . . my friends sorta kept at me until I got interested.

Another man who is nearing retirement said:

I had known \_\_\_\_\_ before he opened his restoration shop which is about a mile from here, I stopped in to see him a few times. The cars were beautiful . . . and he encouraged me.

For such men, an abundance of leisure time creates an attitude that is receptive to the invitation of friends, or one that is inquisitive about the enthusiasm of friends who then serve as models. Of course, this can also be true of younger men as well; one 40-year old survey respondent had become a car buff only recently, because "I was in the process of a divorce, and needed something to occupy my time."

Some young hitchhikers were also recruited by their car buff friends. Some reported that they "got the bug" when they responded to a friend's request for help in working on car or scrounging old salvage yards for a part. An example:

I had a friend who was really into cars, and one time he asked me to go with him. A Saturday it was, he would go around to junk-yards and stuff. Just that one time, and I really got into it. Afterwards I thought, hey, 'I'll get an old car of my own,' so I did. And it was all set up for me since he taught me what kinds of things to look for, where to find stuff . . .

A few other enthusiasts traced their involvement in the world of old cars to social events that did not involve a direct primary relationship. Visiting an automotive museum, and casually taking in a car show, were each identified by two enthusiasts as the source of their old-car pursuits.

[wife speaking]: We took a drive up to \_\_\_\_\_ museum one Sunday, and on the way home, he said, 'You know, I'd like to have an old car myself.' And I said, 'Why didn't you say so?' So you can see where it led . . .

I was driving along one day, and I hit this terrible traffic jam. It must have taken almost an hour to go three or four miles. Finally I got to see what it was all about—it was Hershey! I stopped and got out, started looking around. I didn't want to leave. It just blew my mind.<sup>9</sup>

For these enthusiasts, sociability per se was not a primary factor as with the hitchhikers described earlier. I consider them hitchhikers, nevertheless, because their interest was mobilized by contact with the world of old cars, and thus was (1) contingent on social experiences and (2) impulsive rather than deliberate and passionate, as in the other types.

In sum, hitchhikers differ sharply from all the types previously presented in their lack of any evidence of an intense interest during youth or at any other time of their being confronted with or invited to participate in the old-car world.<sup>10</sup> Like the family heir respondents who appear to have been more conventionally "socialized," the hitchhikers fit prevailing sociological notions quite well, since they have been recruited or have otherwise responded to social influence. However, the findings presented here suggest that this model may describe the process of becoming a car buff for only about one in every six enthusiasts.

It appears that neither the hitchhikers nor the family heirs would have become buffs were it not for their contact with others in the social world of old cars, but together they comprise less than 30 percent of the enthusiasts surveyed. The important finding thus concerns those for whom conventional models of socialization and social influence do *not* fit—the youth fascination pattern and its variant, the late bloomer. Together they constitute a substantial majority of both the interview and the survey samples.

### **Underinstitutionalization and Subjectivization**

Some clues to the interpretation of these findings are provided by the work of Peter Berger and associates. Since the time of Durkheim, there has been a substantial sociological consensus to the effect that social isolation is more frequent in modern than in traditional societies (see also Lasch; Nisbet). Not only social contact, but also personally meaningful frameworks for interpreting social life and making sense of one's existence are more

likely to be problematic. Berger has described this as the diminution of the "sacred canopy"—the overarching religious and cultural worldview that provides a plausible interpretive framework for approaching existence. These two tendencies operate jointly to produce a social environment that is "under-institutionalized" (Berger and Luckmann; Berger et al.); one in which significant areas of experience are not structured by established social and cultural formations. While the amount and distribution of "underinstitutionalization" varies across the social landscape, it is generally assumed higher in modern, urban settings than in more traditional ones. Underinstitutionalization tends to be greater, particularly in the domain of leisure, or more broadly, of the private sphere of experience. Away from the public and binding demands of the institutions of work and those of government and education, individuals experience themselves as "free" to select their own commitments and associations.

A major subjective consequence of the decline in the organization of individual consciousness and activity by social and cultural forces is that the individual must decide how to organize his or her activity. Meaning and direction must come from within the self. Berger has described this process as *subjectivization*. Lacking the ballast of strong social relations and a plausible cultural framework, both of which traditionally were internalized as aspects of self and enacted as roles, the self is emptied of content and becomes "weightless" (Berger).<sup>11</sup>

Here, then, is an alternative process by which the social context may condition individual activity: by its absence. If consciousness is not socially provided with adequate forms of meaning and activity from the social environment, the individual may devise his or her own.

Of course, this argument does not imply that subjectivization means "freedom" in any objective sense. Car-buff consciousness focuses on objects that are designed to be attractive, and then are glorified and celebrated in color advertisements and showroom windows. Thus, I hypothesize that the old-car phenomenon reflects the internalization into consciousness of powerful social and cultural images. The lack of social influence suggests that it is a *direct* mediation of mass culture to individuals, largely by-passing interpersonal relationships that might cushion its effects.<sup>12</sup>

I suggest the world of old cars is thus constituted by a process very different from the *conscience collective* in the classical sense of a consciousness that one develops through socialization, which is internalized in the course of everyday, face-to-face social interaction. In the context of a comparatively weak and fragmentary organization of experience by social relations, the individual is bombarded directly by the glamorized images of mass media. Thus assured that it is a worthwhile object, the individual appropriates the image of the car to the point that it becomes part of his own identity.

As noted, automobiles and related paraphernalia are hardly the only



artifacts of mass culture to be seized as objects of intense devotion. Stamps, posters, coins, bottles, telephones, phonographs, and records, pencils, campaign buttons, baseball cards, and now even beer cans are passionately sought collectibles. The findings presented here raise the question of generalizability to other such phenomena, and to more distantly related pursuits. There is some indication that the network of beer can collecting has been generated by a similar kind of process:

In October 1969, a newspaper article brought six collectors together. Before the article appeared, each had thought he was the only person in the country collecting beer cans. The next year, Beer Can Collectors of America was formed . . . (Salem, 20-2)

Although this argument offers an explanation that is both plausible and grounded in social theory, it is conjectural. The data do not contain a direct measure of subjectivization. However, if subjectivization is the subjective counterpart of underinstitutionalization, it is possible to look for some objective features of the biographies of these respondents that may be linked to it.

It should be clear that subjectivization is neither a universal phenomenon nor a random one. If it occurs as a result of social conditions, it should be empirically related to such conditions. Who are the people who develop and sustain a passion for cars? What social characteristics might set them apart from the general population? The biographical time of subjectivization for the youth fascination buffs—those who are most significant both in creating and in representing the social world of old cars—is obviously youth. Thus, the question becomes, for what kinds of young people is experience relatively unstructured and unorganized? Of course, the common social and cultural ground shared by all the respondents—and virtually everyone else in the world of cars—eliminates a large amount of variation in the levels of institutionalization; none of these men grew up in preindustrial villages or in utopian communes where the individual would be more fully embraced by the social order. The sample provides no basis for making urban-rural comparisons. Within the limits of their common social and cultural background, what other factors might be considered?

### A Social Correlate of Subjectivization

While the data contain little information that could illuminate this question, they do contain one significant piece of information that emerges as relevant to subjectivization during childhood/adolescence: the condition of "siblinglessness." It seems reasonable to assume, *ceteris paribus*, that only children spend more time by themselves than do children with close sib-

lings, and hence their experience should be relatively less structured by conversation, by social interaction, and by more organized social activity than that of those who have close siblings. Based on associations that appeared in both the interview and the survey data, I suggest that being an only child may be associated with subjectivization and hence, if one is a car buff, with being a youth fascination type.<sup>13</sup> Table 2 provides preliminary support for this exploratory hypothesis, and simultaneously lends support to the subjectivization interpretation itself. Following Toman, a person is classified as an only child if no siblings are closer than five years of age, since the interactional contributions of siblings five or more years distant is of much less weight relative to that of peers and parents. Thus, those without siblings closer than five years can be defined as "functionally" only.

Those who are "true" onlys, without siblings at all, are also presented, primarily to provide a basis for comparison with a larger and more typical population. The best estimates on national figures for number of siblings come from the General Social Survey data and indicate that of all white males between the ages of 20 to 70 (the subpopulation that most closely matches the demographic characteristics of the sample of car buffs), 6 percent are only children. Thus, the survey data reported here clearly support that car buffs are more likely than typical individuals to be both "true" and "functional" only children. They further indicate that, among car buffs, those fitting the youth fascination pattern are most likely to be only children, as the subjectivization hypothesis outlined above would predict. Thus, the hypothesis is supported by both internal analysis of the data I collected from auto enthusiasts, and from external comparisons of those data with a data base representing the population at large.

I hasten to emphasize, it is not my contention that "only child" status is necessarily a major or central antecedent of subjectivization. I suggest it only as an accessible, albeit rather crude, indicator of a social

**Table 2.** PROPORTION OF EACH ENTHUSIAST TYPE WHO ARE ONLY CHILDREN (IN PERCENT)

	Functional Only Child	True Only Child	
Youth fascination	45	23	(N = 95)
Late bloomers (Y.F. variant)	40	27	(N = 25)
Family heirs	13	6	(N = 25)
Hitchhikers	25	13	(N = 31)

location that, by virtue of its structural characteristics, tends to reduce the availability or presence of others, and hence of face-to-face social relations. Thus, siblingness should be seen as illustrative of the kinds of objective indicators that can be identified and related to underinstitutionalization, and hence to subjectivization.

## Conclusions

It appears that (1) people may develop interests and commitments through processes that by-pass the effects of face-to-face social relations, as described in theories of socialization and social influence and that (2) the likelihood of doing so may be predicted from factors in the external social environment. If this is so, the face-to-face social relations that have been emphasized in conceptualizing the effects of the social context on leisure pursuits need to be supplemented with a systematic delineation of the consequences for the development of the self of (1) the extent to which everyday experience and particularly free time is socially organized—implying a continuum of isolation-integration in everyday experience and (2) historical changes in the operative types of social and cultural forces that directly affect the self. Specifically, the findings presented here suggest that mass production, mass media, and mass advertising, which do not exist in traditional societies where culture is mediated and individuals are socialized through community level relations, may have novel effects on the nature of modes of activity and interpersonal association that emerge in modern society. Glennon, Wexler, and others suggest the need to reconceptualize primary relations in contemporary society because the instrumental relations characterizing the public realm tend to carry over into the expressive spheres of domestic and private life; the findings offered here suggest that not only the structural and topical features of face-to-face relations, but also the dynamics of socialization processes, may have changed substantially. Interpersonal relations are to a significant extent supplanted by mass media as the bearer of the content that is to be internalized.<sup>14</sup>

## IMPLICATIONS FOR THEORY AND RESEARCH

The findings presented here are exploratory, and the usual qualifiers regarding their tentativeness must be acknowledged. Nevertheless, they make a clear case for exploring further the development of new models of the relation of self and social context in leisure time. A number of important questions that would contribute to developing a subjectivization model have scarcely been touched on. The data presented here are from individuals who were chosen because they have a specific interest in the present, which prevents a controlled comparison of individuals who may have experienced youth fascination, but did not later pursue it. The factors

accounting for interest loss versus continuity over time are not explored here. In addition, the question of how the selection of a specific interest occurs is not addressed: What determines the specific content of subjectivized consciousness? In the case of cars, it may have to do with a masculinity, but I suspect it has much to do with the fact that automobiles are designed to be attractive and with the effective exploitation of their potential attractiveness through media advertising. A third set of questions for further research, then, concerns the relative importance of socialization, social influence/recruitment, and "subjectivization"-type processes in other expressive pursuits, and the extent to which these processes distinguish expressive from instrumental voluntary associations.

### Notes

1. An extended discussion comparing these studies substantively and methodologically is beyond the scope of this paper. For detailed critical reviews of the literature on leisure activity, see Kelly (c), Smith (b), and Wilson; on voluntary associations see Smith (a), and Smith and Freedman.
2. Smith makes a similar point, calling for a "specification of the range of voluntary associations for which it holds" (a, 254). He suggests, for example, that the hypothesis of a positive association between voluntary association participation and social status (e.g., Lemon et al.; Wilensky) may be reversed in the case of associations devoted to protest or revolutionary activity.
3. Csikszentmihalyi uses the term "flow" to describe "autotelic" activities: "An activity was assumed to be autotelic (from the Greek *auto*=self and *telos*=purpose) if it required formal and extensive energy output on the part of the actor, yet provided few if any conventional rewards" (10). Csikszentmihalyi offers an innovative and sophisticated approach to conceptualizing and measuring autotelic activities, but gives little attention to how such activities relate to other aspects of the actor's identity and experience (Wilson, 35) nor to how engagement in a given autotelic activity is related to social location. This present study thus also can be seen as an attempt to link autotelic experience with social location.
4. Some of these men report using time at work to hunt for old cars or parts (while traveling, or through paging hobby periodicals) or other such activities. One respondent reported turning down numerous better jobs because a geographic move would be required and the volume of his collection of cars and parts means that a move does not seem feasible.
5. I have shown elsewhere (Dannefer, a) that the social world of old-car enthusiasts is very clearly a male-dominated and white social world, but one that is very heterogeneous with respect to age and occupation and other indicators of class; the intensive interview sample was selected to represent this heterogeneity. Potential respondents were screened for "activity," defined as averaging at least one evening a week of involvement. Only two persons were eliminated by this criterion, and only one refusal was encountered.
6. Quantitative data presented in tables is taken from survey results only. However, distribution of survey respondents is similar to that obtained in the intensive interview sample; twenty-three of the forty persons interviewed were also youth fascination types. Respondents were classified as youth fascination types if (1) they indicated an intense interest began prior to age 18 [mean age was 9.8 years old]; (2) they claimed to have received no encouragement or reinforcement in the initial pursuit of the interest from others, and (3) they report that it has continued to be an active interest throughout their lives. Late bloomers shared (1) and (2), but experienced a disruption of activity if not interest; the youngest age at which any of them reported a return to old-car activity and club involvement was 34. Family heirs shared (1), but reported that parents or other close relatives served as models for their interest. Hitchhikers are most heterogeneous with respect to the age at which they became active; regardless of age they reported being invited or recruited by a friend.
7. With respect to the other participants: ten respondents fit none of these categories. These included a few men who apparently became involved in the world of cars to exploit its com-

mercial possibilities (although it is not a primary occupation). Two men reported that they happened to have an old car sitting around and suddenly became taken with its attractiveness and value as an antique. A few others resembled youth fascination types in that they developed an intense interest without apparent social encouragement, but not until their late twenties or later still in adult life. While these may constitute an interesting variant, my hunch from both interviewing and from unsystematic information gleaned from observation is that these constitute a very small proportion of participants in the old-car world.

8. This is a reference to automotive styling in the late 1950s.

9. "Hershey" refers to the largest national car show and swap meet, sponsored by the Antique Automobile Club of America and held each October in Hershey, Pennsylvania.

10. This does not mean that they are unwilling to work on behalf of the club. On the contrary, the importance of the social dimension makes some hitchhikers work exceptionally hard for the club, holding office, taking care of drudge-work, and participating in club activity. The point is simply that they lacked the "autocentric" interest to produce a car club, were none in existence.

11. Arnold Gehlen (see Berger et al.), has described voluntary associations and other forms of social organization which arise in an underinstitutionalized context as "secondary institutions." However, the voluntary association is more typically conceived as a community level phenomenon, reflecting a public issue or else serving the sociability needs of its members. The data presented here demonstrate how subjective and individualized the roots of such phenomena as automotive enthusiasm are, and thus suggest a distinction between secondary institutions oriented to public or communal interests and those that are grounded in a shared subjective intensity, which serve to facilitate the pursuit of private purposes rather than sociability. I have suggested elsewhere that this latter type of association—exemplified by the social world under investigation here—may be called an "anchoring institution," since it is organized around an individually selected "anchor" for subjectivized consciousness. (Dannefer, a).

12. An additional manifestation of the fact that this phenomenon represents primarily a link between the individual and mass culture is found in the typical development of an individual's activity within the hobby itself. Many respondents join national clubs before joining local chapters. Thus, their first involvement is with an impersonal, abstract club body rather than with a local one.

13. This issue was one of the key questions to which an answer was sought from the survey respondents. On the basis of theory and of the patterns found in the initial interview data (see Dannefer, a) the outcome obtained in the survey was predicted, and it was because of the prediction that the question was asked in the survey.

14. Psychoanalyst Joel Kovel portrays a similar process in his description of a new "social" individual, whose appetites are stimulated by advertising and for whom the primacy of family structure has been replaced by media and school. Kovel's description is based on cases seen in analysis, and thus can hardly be considered representative. Yet it is a significant one, not only as representing a tendency or trend, but also because such cases present a constellation of symptoms that are seen as requiring new treatment strategies.

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~~CONFIDENTIAL~~



## PART II

# Sociology: Parochial or Universal?\*

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## ABSTRACT

*Sociologists in the main define their field as the scientific or systematic study of human groups, human societies, or human social behavior, a formulation that does not imply any spatial limit upon its scope. In practice, most sociological studies are conducted within one society, and few theoretical propositions can find support in research results based on comparable data from many societies. To counter such undesirable parochialism, this paper calls urgently for recognition of the need for much more emphasis on cross-societal comparative research and proposes ways in which such an expansion can be facilitated.*

*Science . . . is not, as is sometimes thought, a way of building a solid indestructible body of immutable truth, fact laid precisely upon fact in the manner of twigs in an anthill. Science is not like this at all: it keeps changing, shifting, revising, discovering that it was wrong and then heaving itself explosively apart to redesign everything. It is a living thing, a celebration of human fallibility (Thomas, 1).*

American sociologists seem everlastingly to conduct probing and frequently skeptical evaluations of the sociological enterprise. In private conversations, in their own courses, and in print they are wont to call into question even the most sacred foundations of our discipline. Mayhew's recent charge that "American sociologists are generally unfamiliar with a sociological apprehension of social phenomena" because the field "has been dominated by an individualist, psychological perspective" (335) is a good case in point. My own purpose is to direct attention to what I regard as a serious shortcoming of sociology, to point to the available means of overcoming the problem, and to propose ways to facilitate the movement in what I see as the desirable direction.

The defect to which I refer is parochialism. My awareness of this situation began over 15 years ago soon after I became a visiting professor in

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a university in southwestern Colombia. Its School of Social and Economic Sciences had under way a new master's program in business administration, and I was asked to present a short course in industrial sociology. When I appeared to give my first lecture, I found that the students were mainly middle- and higher-level executives in various enterprises in the city of nearly one million people enrolled in a part-time program in which classes were presented early and late on weekdays and on weekends.

The request to deal with industrial sociology was welcome to me; though by no means a specialist, I had been teaching the subject for several years. Hence, I launched into the lectures with confidence. But from the first I felt uneasy as I realized that my presentations were based on the results of research that had been conducted, almost without exception, in the United States. As I grew more conscious of the problem, I went to the library and searched bibliographies, hoping to enrich my lectures and make them credible by building on the findings of studies carried out in Latin America or, at least, in other countries of the less-developed world.

To my distress, I discovered little that I could use. The few sociological articles that I came on were for the most part simply descriptions of situations in other countries written by United States sociologists with short-term appointments in foreign universities, mainly as Fulbright fellows. The literature at hand could make no more than a tiny difference; I had no alternative to offering instruction in an industrial sociology based on generalizations derived almost entirely from research work carried out in our country. I completed my course of lectures embarrassed by the implied boldness of teaching Latin Americans, including many top community leaders, theoretical and applied material based almost entirely on research in the United States that might have been irrelevant or inapplicable to their society.<sup>1</sup>

Following this chastening experience, I sought to determine whether industrial sociology was a special case or whether the lack of comprehensive research findings for areas outside the United States was general to the field of sociology. My sobering conclusion was that the narrowness and provincialism that marked industrial sociology was not at all atypical.

To say that American sociology in general is parochial<sup>2</sup> is not to say that all American sociologists are parochial or that no sociologists are conducting and publishing work based on data drawn from outside this country. For one thing, it seems obvious that the amount of cross-cultural research reported in books and journals has increased fairly substantially in the last decade or two. A number of works on the methodology of comparative research have been published (e.g., Armer and Grimshaw; Marsh; Smelser; Vallier), an annual review of comparative social research was established in 1978 (Tomasson, a),<sup>3</sup> an *International Journal of Comparative Sociology* is issued quarterly, articles reporting cross-cultural research appear in our major journals much more often, and a considerable number of

books has been published. Yet, despite this and other activity, it remains true, in my judgment, that parochialism is the rule in American sociology. For despite the increase in cross-cultural studies, they remain a peripheral activity in the field.

When Tomasson (a) analyzed the programs of the annual meetings of the American Sociological Association in 1956, 1966, and 1976, he found that "strictly comparative papers," that is, those involving the systematic comparison of two or more societies in some respect, amounted to 1 percent in the first year, 7 percent in the second, and 4 percent in the last. An examination of the most recent volumes of two major sociological journals also is informative.<sup>4</sup> Of the 47 articles and research notes published in Volume 45 (1980) of the *American Sociological Review*, 6, or 13 percent, clearly were comparative; two others (4 percent) utilized data from non-North American societies. Six, or 9 percent, of the 69 articles that made up Volume 58 (1979-80) of *Social Forces* were comparative according to the same definition, while 3, or 4 percent, more were focused on non-North American societies.<sup>5</sup>

When I characterize American sociology as parochial, the implication is that sociology ideally is not parochial. As everyone in our field is vividly or even painfully aware, formulating a widely acceptable definition of sociology is at worst an impossible task and at best a difficult one. Alex Inkeles in his monograph *What Is Sociology?* "has shown that this is a complex issue that can be approached from numerous points of view and that it is difficult, if not impossible, to offer a conceptual definition that sharply distinguishes sociology from other social-science disciplines and that would be acceptable to all social scientists who claim this identity" (Gross and Fishman, 309).

Although there is considerable variation in detail, introductory textbooks tend to present sociology as involving the scientific or systematic study of human social relationships or groups or of social behavior or social life. A few years ago, for example, Everett Wilson defined the field as "the controlled observation and interpretation of differing patterns of human relationships—their sources and consequences" (7). His intent was to highlight both the subjects and methods of the field, but the core phrase is "patterns of human relationships." A more common form of definition is that of Ronald Federico in his recent textbook, "the study of the structure, functioning, and changes in human groups" (700). A more psychologistic conception is offered by Lowry and Rankin: "the scientific study of human social life that focuses upon the social self, social roles, social interaction, culture, and forms of group behavior" (688). It can be seen that none of these definitions imposes any limits on the scope of sociology: the patterns of human relationships and the structure, functioning, and change in human groups are not delimited as to either time or space. The clear

implication is that sociology as a field strives to develop knowledge that, under specified conditions, will hold true for all times and places. When we turn from textbooks to works intended mainly for the eyes of other sociologists, we find definitions that are more complex and more specific but which still do not interpret differently the nature of the field (Gordon, 3; Gross and Fishman; Homans).

Thus, I feel confident in advancing the proposition that sociology is a universal science that ultimately will develop abstract generalizations about human behavior that are not restricted spatially or temporally. From what has been said, it would seem that for several generations sociologists have been saying one thing and doing another; that is, they have been construing their field as one that deals with human behavior generally, and have so been informing their students and others, while at the same time they have been acting as if it were either place-limited or time-bound. To the extent that the field relates to the concrete, individual, or unique, it is nothing more than a species of history; to turn to one of my own health studies as an illustration, I can say that in 1973, in Hale and Fayette Counties in Alabama, the average number of untreated critical symptoms decreased as social-class level rose (Webber and Ritchey). If in reality sociology as practiced is as circumscribed as I have suggested, then sociologists do indeed deserve Goode's (40) criticism of their pretentiousness.

### The Comparative Study of Societies

It is obvious enough that in my judgment the solution to this problem lies in a genuine effort to test theoretical propositions by utilizing data relating to more than one society. But such a proposal requires us to face up to various questions. The first of these is this: In methodological terms, can an increased emphasis on the use of the comparative method be justified?

Comparative studies have an honorable history if one judges by their status in the armamentaria of the sociological pioneers. Auguste Comte referred to the comparative as "the chief method, which consists in a comparison of the different coexisting states of human society on the various parts of the earth's surface. . . ." (249). He was preoccupied, of course, with the observation of different degrees of social evolution. Both Weber and Durkheim, who published statements of their methodological convictions, placed strong emphasis on comparative analysis, though neither detailed his procedures to any great extent (Smelser). Durkheim's forthright statement in *The Rules of Sociological Method* has been quoted often by students of the comparative method: "Comparative sociology is not a particular branch of sociology; it is sociology itself, in so far as it ceases to be purely descriptive and aspires to account for facts" (139).

Many later scholars likewise have voiced their conviction that comparison constitutes the fundamental analytic method. One of the clearest statements is that of Swanson:

Thinking without comparison is unthinkable. And, in the absence of comparisons so is all scientific thought and all scientific research. No one should be surprised that comparisons, implicit and explicit, pervade the work of social scientists and have done so from the beginning . . . (145).

The term comparative study is, however, a broad umbrella. It subsumes the analysis of roles, institutions, communities, and other structures within as well as outside given societies. Likewise, comparison is the essence of the experimental method. The comparative studies to which I refer in this discussion are of a particular type, those that involve at least two societies. The purpose of such analyses is to identify, describe, and explain the invariant or universal properties of social systems or, failing to discover such universalities, to identify and describe the variations that prevail among societies (cf. Smelser, 2; Treiman, xv).

Various terms have been employed to identify research of this kind: cross-societal, cross-national, cross-system, cross-cultural, as well as comparative. Cross-national implies an emphasis on political organization, while cross-cultural suggests, at least to some extent, an anthropological interest. Comparative research, as we have just seen, constitutes an essential analytic operation and encompasses much more than investigations embracing two or more societies. Cross-system, or cross-social system, has the disadvantage of not denoting clearly that the systems in question are societies. Hence, the term *cross-societal comparative research* or analysis most accurately describes the procedures to which I am referring (cf. Grimshaw 5; Newman, 69; Smelser, 2; Tomasson, a,1).

My contention that cross-societal studies ought to play a much larger part in sociology than they do at present raises the question, Why are cross-societal comparisons superior to those carried out utilizing data from only one society? The answer is that the more that can be found in common between or among populations or circumstances that seem quite different, the more powerful are our generalizations. As Cohen and Nagel have said, "It is the isomorphism found in diverse subject matter [in this instance, the diversity revealed in cross-societal comparisons] which makes possible theoretical science as we know it today" (139). As a matter of fact, it is differences in social phenomena that stimulate our interest in understanding and explaining their existence. By definition, cross-societal comparisons are likely to involve more substantial differences than intrasocietal ones, and it follows that the results of our analyses of cross-societal differences should be not only more interesting but more consequential for the advance of knowledge.

The history of cross-societal research in American sociology has

been cyclical. As we have seen, the pioneers were comparativists. Comte, Weber, de Tocqueville, Durkheim, Sumner, Small, Ward, Thomas, Ross, and Park deserve to be so classified. Armer and Grimshaw refer to a study by Frank R. Westie that reveals that while a large proportion of the earlier presidents of the American Sociological Society/Association had occupied themselves with cross-societal studies, none of the 20 presidents who served from 1931 to 1950 was known mainly for such research. The earlier expansive period in sociology had been succeeded by one in which the study of American society emerged as a major preoccupation, a period that has been called the stage of "national empiricism" (Tomasson, a,3). Since 1950, the pendulum has begun to swing back toward the comparative side, though as we have seen, studies that are unqualifiedly cross-societal in nature remain a minor part of all work reported in our journals and books.

The character of cross-societal work likewise has changed fairly sharply since the latter part of the nineteenth century. An earlier generation of sociologists drew on anthropological monographs (Goode) for their data, and the effect in many cases was illustrative and little more. Others, like Ross (e.g., a-d) traveled extensively and published reports that were based upon bare observation (Wolf), interviews with residents of the country visited, and such statistical and other descriptive material as came the author's way. In more recent decades the scope of data bases and the modes of collecting data have undergone a transformation. In part the reawakening of interest during the last three decades or so can be attributed to the work of sociologists who following World War II sought to understand the process of modernization and development. The classification of countries into developing and developed and their further classification according to regions of the world and predominant political systems invited comparisons of societies and categories of societies.

The use of cross-societal comparison is not limited, of course, to the field of sociology. Students in history, anthropology, law, economics, political science, and the medical sciences, among others, employ the strategy (e.g., Booth and Seligson; Kohn and White; Mesa-Lago; Moore; Pflanz and Schach; Verba et al.; Wolf). The interrelatedness of the methods is suggested by the sociological use of comparative historical analysis in Skocpol's recent study of social revolutions.

Even a cursory review of the literature highlights the astonishing variability of cross-societal research undertakings. In fact, although the term cross-societal research leads one to expect some semblance of unity of approach, that turns out to be lacking. To define the nature of such research in contemporary sociology requires us to examine the several dimensions on which the differences occur.

The data analyzed in cross-societal studies, for example, may be primary, secondary, or a combination of the two. The scope of the investigation may vary from nonrepresentative samples of opportunity or con-

venience, purposive nonrepresentative samples, and representative samples to (in theory, at least) the universe. The matter of representativeness is further complicated by the need to pose the question on two levels, societal and intrasocietal. Another key question is whether contextual information is obtained and brought to bear, either in the analysis or in attempts to interpret the results. Finally, the organization of data collection may range from one investigator who conducts all of the field operations to various combinations of teamwork with same-country colleagues as well as those of the societies in which data are being sought.

Those cross-societal studies in which the data for analysis are gathered in the field by one or more investigators are relatively rare in the literature of recent years; the vast majority of the reported research projects have their basis in secondary data of various kinds. The scarcity of investigations requiring field work in several countries is easy to understand, of course, in terms of the implied commitment of time as well as money. Form's study of stratification among autoworkers in four countries exemplifies this kind of study. Planning and field work began in Italy in 1962 and continued in the United States, Argentina, and India during four years. Data collection involved not only interviews with workers in their native language but extensive review of the history and social situation of the community, factory, union, and local industrial network as well as interviews with officials of unions, management, and government. Form's commentary (xv-xviii, 273-299) helps to bring into high relief the theoretical, conceptual, and methodological problems with which he and his collaborators had to cope.

Studies that rely on secondary data and that, therefore, do not contain a foreign-country field-work component account for the bulk of monographic accounts that have been published in recent years. The Human Relations Area Files (Murdock) is a well-known example, dating from the late 1930s, of a bank of data drawn from many societies. As social data from social surveys here and abroad are made accessible to scholars, such material provides the basis for more and more analyses. For example, Treiman studied occupational hierarchies in over 60 countries by analyzing popular evaluations of the prestige of occupations as revealed in social surveys; he also utilized data on occupational skills and levels of wealth from census records and other sources. Crano and Aronoff made use of a cross-cultural sample of 186 societies to test hypotheses regarding expressive and instrumental role complementarity. Still another illustration of the use of sample-survey data is found in the work of Covello and Bollen, who examined status consistency in 9 societies, holding constant the level of industrialization.

Despite the relative and growing importance of social-survey data, a number of other kinds of secondary information are being used. These include nonsurvey statistics, especially social, economic, demographic, and



political, published by governments (e.g., Stack; Nolan); and constitutions of nation-states (Boli-Bennett and Meyer). Berelson and Steiner's inventory of findings about human behavior of some years ago may be regarded as a sort of comparative study whose data were reports of behavioral-science research that could satisfy criteria as to generality,<sup>6</sup> while Rogers' survey of research findings from diffusion research falls into the same general category; in each instance, the intent was to produce a list of generalizations with adequate cross-societal support to justify their inclusion. It seems logical also to regard Lenski's book on social stratification as a comparative analysis drawing on the evidence of ethnography, history, and sociology.

Of the analyses already mentioned, four of the designs depended on samples that probably can be defended satisfactorily. Skocpol, for example, was interested in explaining the occurrence of social revolutions of a particular type, "rapid, basic transformation of a society's state and class structures, accompanied and in part carried through by class-based revolts from below" (33). Her sample of three cases, France, Russia, and China, she regards as representative of her universe. Mesa-Lago's universe was Latin American countries with social-security programs. His sample of five countries was selected in terms of such characteristics as the time of inception of the social-security system, the scope of the system, and the nature of the sociopolitical regime. Treiman as well as Crano and Aronoff presented their coverage of societies as representative.

My unsystematic observations have led me to believe that most cross-societal comparative investigations must be classified as of limited scope; the findings cannot be generalized, in most instances, beyond the societies that were studied. The four societies studied by Form in his investigation of blue-collar stratification comprised a sort of purposive sample, for example. Robinson and Kelley looked at class as conceived by Marx and Dahrendorf by analyzing survey data from national samples in the United States and Great Britain. Tyree, Semyonov, and Hodge's research on how social mobility is influenced by level of economic development in 24 countries provides an instructive example of some of the difficulties involved. Their selection of acceptable studies was affected by their research question and by their "subjective sense of what is and what is not acceptably reliable" (413). They limited their analysis to males and to mobility between the white-collar and blue-collar categories. The year of data collection ranged from 1949 to 1974, sample sizes from about 500 to nearly 24,000, and coverage from one city to entire nations.

Taking into account contextual or background factors that may bear on the research question must be a particular concern in cross-societal comparative studies. Good practice dictates, of course, that such variables be made a part of the research design. Studies carried out in a small number of countries with on-site field work offer the best chance for this to

be done thoroughly. For example, Whiteford's comparison of social classes in two cities, one in Colombia, the other in Mexico, lent itself to placing each set of data in the national cultural context. In instances in which it is not feasible to control background variables, the other factors may be considered in the phase of interpretation; Form acknowledges, for example, that class solidarity may have been affected by factors other than technology, the variable controlled in his study. The problem is most acute in analyses of information elicited in large numbers of social surveys that were conducted in various countries, at various times, by various investigators, with various research purposes. In such situations the only apparent solution is to invoke secondary data, usually collected by governments, as measures of the societal variables; and given the likelihood that different governments may have employed varying definitions of the concept, achieved different degrees of reliability, and used distinct time periods, and that universe definitions in the sample surveys may be only more or less compatible, the validity of the design must remain problematic.

Since most cross-societal comparative work in sociology is based on data from secondary sources, it follows that the organization of data collection does not differ much from that characterizing other studies utilizing quantitative materials. Thus, the library and the computer often are indispensable tools in the enterprise. On the other hand, making and recording observations in two or more countries creates the need for a relatively elaborate field organization, and for the present purpose it is essential to note some of the demanding aspects. When language differences exist among the societies, the investigators must either surmount the barrier by learning to use unfamiliar languages or arrange to work with local collaborators; and in many cases, it proves wise to have co-workers in other societies in order to solve difficulties of gaining access, obtaining field personnel, and dealing with the intricacies of local arrangements (cf. Weber). Mesa-Lago's work in Latin America exemplifies the case of the single investigator who can successfully carry out by himself all of the information-gathering tasks, but such instances appear to be rare. Newman's investigation of comparative deviance offers an illustration of the more usual case of teamwork in the field. While he does not describe his methods explicitly, Newman suggests that the research "would not have been conducted without the work of scores of people in the field, and the many people involved in the massive job of organizing the conduct of our social survey in six countries, ever mindful of maintaining comparability of research design" (ix). Insuring the highest feasible degree of comparability in the face of language and other cultural differences confronts the principal investigator with daunting difficulties, especially when the interviewers and their supervisors may be, again on account of the sum of cultural factors, including barriers to full communication, somewhat less than fully cognizant of the implications of the principles guiding the research design.

## Expanding Cross-Societal Comparative Research

My effort to show that the logic of sociology as usually defined requires that sociologists take steps to make their discipline less parochial and, to that extent, more nearly universal in scope has been followed by a thumbnail description of the nature of cross-societal comparative studies. This review suggests that in order to do cross-societal research investigators must cope with a number of serious difficulties. My purpose now is to name several major obstacles and propose ways of overcoming them.

### RECOGNITION OF THE NEED

The primary obstacle, in my opinion, is not the practical problem of methods and resources but rather the failure of most of us to comprehend the urgency of introducing a more substantial cross-societal dimension into sociology. As I have tried to point out, the lack of attention to such comparative studies can be attributed to a certain blindness that has characterized sociologists viewing their own field. To accept the conclusion that sociology is undesirably narrow and limited will be to turn in the direction of accepting an obligation to promote cross-societal comparative research. When Brown and Gilmartin measured articles published in the *American Journal of Sociology* and the *American Sociological Review* in 1965 and 1966 against those that had been published in 1940 and 1941, they concluded that at the end of the 25-year period "substantive research in our journals is . . . even more culture-bound and time-bound than in 1940-41" (287). My own impression is that in the subsequent 15 years the proportion of American sociologists publishing reports of cross-societal comparative research has risen; yet the paucity of such studies, as we have seen, still is a serious problem. The solution to this problem seems to lie in heightening awareness that the issue raised in this paper is significant; it is hard to see how further discussion of the question cannot lead to the conclusion that more cross-societal effort is called for.

### ORIENTATION TOWARDS CROSS-SOCIETAL RESEARCH

The greatest challenge beyond achieving an acknowledgment by sociologists of the need for cross-societal work lies in convincing our colleagues to look favorably on comparative investigations of that kind. One is tempted to speculate that sociologists can be divided into two classes, those who tend to be preoccupied with the structure, processes, and change of their own society and those who tend always to consider the organization and functioning of American society in a world-wide context. My impression is that many sociologists—a majority, I would guess—turn cold eyes on no-

tions of engaging in cross-societal studies, evidencing a kind of ethnocentrism that might be seen as peculiarly unsuitable in sociologists. Armer and Grimshaw believe that when the many sociologists who worked in development programs overseas during the 15 years after World War II returned to this country, the largest number turned their backs on the comparative dimension in their experience; a much smaller number found their way into area studies; and the smallest number became genuine cross-societal comparativists.

The disinclination toward such research to which I refer here is closely related to the failure to perceive the part that cross-societal analyses can play in enabling sociology to realize its potential. The key to altering the situation lies in changing the depth and breadth of the curriculum for doctoral education. Greater emphasis on history, on understanding areas of the world in broad, interdisciplinary terms, and—in particular—on mastering and *using* foreign languages will be helpful, but a greater stress in all courses on non-United States evidence and analyses likewise should help to establish a positive attitude. Provision of opportunities for graduate students to spend time in a foreign country collecting data undoubtedly would have a favorable effect. The President's Commission on Foreign Language and International Studies concluded that "Americans' incompetence in foreign languages is nothing short of scandalous, and it is becoming worse" (5) and also that this incompetence "explains our dangerously inadequate understanding of world affairs. Our schools graduate a large majority of students whose knowledge and vision stops at the American shoreline, whose approach to international affairs is provincial, and whose heads have been filled with astonishing misinformation . . ." (7).

#### METHODOLOGY

Despite the publication in recent years of several works dealing explicitly with the methodology of cross-societal comparative research and the appearance of reports of such studies in monographs and journals, the special aspects of this approach do not seem to be receiving adequate attention in doctoral training. Some aspects of comparative research present difficulties peculiar to cross-societal studies.

Tomasson (a) properly calls attention to the inappropriateness of some sophisticated techniques for much cross-societal investigation. To the extent that American sociology has come to place heavy emphasis on complex mathematical methods for analysis, current training may be said either to deter young sociologists from making cross-societal analyses (because the accessible data do not lend themselves to what they regard as acceptable analytical measures) or to encourage them to employ unsuitable techniques in analyses that they do make. Tomasson (a) also deplores the tendency for sociologists to elicit new data to the neglect of great stores of

materials already in existence. The teaching of methodology should take account of "the certain amount of sloppiness" that "is inevitable" (Form, xviii) while at the same time straining toward lessening that sloppiness.

Cross-societal studies in which evidence is collected in the field pose intriguing and often hard-to-solve problems of teamwork. Training, therefore, ought to attempt to come to grips with the variety of issues involved: how to establish fruitful contacts with colleagues in other countries; how to organize responsibility, whether hierarchically or democratically, among the major investigators in the several societies; how to insure that items in data-gathering instruments have equivalent meanings in the several languages; how to supervise field operations adequately, so as to assure control of quality without appearing to doubt the professional competence of one's foreign collaborators; and a number of others. Knowledge of languages also becomes an issue in training for cross-societal field work; understanding and speaking the languages of the societies in which he works gives an enormous advantage to the investigator, even though he has local collaborators.

Standardizing, analyzing, and interpreting existing data from several societies is a process that demands careful attention as part of the effort to make methods training and practice more effective. Rokkan has proposed the "data confrontation seminar":

The essence of the Data Confrontation Seminar is intensified interaction in joint analysis: interaction between scholar and computer and between scholar and scholar. This cannot be achieved through the traditional presentation of papers. . . . Participants in this novel type of seminar do not prepare papers in advance but machine-readable data files. . . ." (24).

Scholars from various countries go with their files to the meeting place for the purpose of "confronting" their several data sets under circumstances favoring face-to-face collaboration. The first such seminar convened in Ann Arbor, Michigan, in 1969, and the second, in Mannheim, Germany, in 1975. While the technique in operation has proved to involve problems as well as benefits, it serves as an example of innovative effort (Jennings and Farah).

#### TIME AND COST

The factors of time and cost emerge as special obstacles when the study design requires that evidence be obtained through field work abroad. In general, cross-societal investigations take longer than intrasocietal ones in all or nearly all phases, from planning through reporting. Essential travel; arranging for suitable collaboration in other countries; reconciling the design to fit varied conditions; assuring that data-collection instruments present the same stimuli in different languages; communications among the

investigators; selecting, training, and supervising field workers; analyzing the data; achieving interpretations that represent common understandings among different investigators; and writing and reviewing manuscripts—all are likely to be more time-consuming in the case of cross-societal investigations. Moreover, as in all teamwork, the principal investigator, if there is one, has less control over outcomes than would be true in national or local studies. Many of these drawbacks are less applicable, of course, to designs in which data are collected by only one investigator.

Most or all of the components of the research process just noted lead to greater costs in cross-societal than in intrasocietal research. Foreign travel, which always has been costly, is becoming more so. Planning meetings, as a result, become major items of cost. Therefore, while social-science field research is expensive, cross-societal comparative research is very expensive.

Problems of making time and funds available to facilitate cross-societal research will have to be solved primarily through convincing university faculties and granting agencies and other organizations of the high priority that must be placed on such lines of investigation.

## Conclusions

For over a century sociologists have defined their field as the systematic or scientific study of society, human groups, human social interaction, or human social behavior. Such definitions impose no spatial or temporal limits on the scope of the discipline; sociology is, then, just as universalistic as biology, chemistry, and physics.

Meanwhile, sociologists have been parochial in their research and, therefore, in the generalizations that they have been able to construct. The United States, in which a majority of the sociologists of the world live and work, has been the site of a large share of the studies carried out. Cross-societal comparative research in which societies are the units has been relatively scarce, though some such studies always have gone on, and they continue to comprise a part of the sociological whole. It follows from this provincialism that today few if any statements about human relationships can be made whose level of generality even approaches the universalistic.

If sociology is to become what it pretends to be, it is incumbent on sociologists to turn seriously to cross-societal comparative research. Only as we move toward encompassing more of the world in our studies can we extend the reach of our findings and conclusions. In doing so, we shall find ourselves in the honorable company of pioneers like Durkheim and Weber, who recognized that the method of comparison is fundamental to all sociological analysis and that we must aim toward the ideal of molding theories that have world-wide applicability.

Although costs and educational and training needs must be considered, without question the two most important steps to be taken are, first, to accept frankly the centrality of cross-societal comparative analysis in sociology and, second, to bring about a favorable orientation toward such analysis through changed emphases in training programs and the literature of the field.

## Notes

1. Instructors teaching in Canadian universities have encountered a similar problem, not only because many sociologists trained in the United States (and thus tending to rely on the materials from this country with which they were familiar) joined the Canadian faculties but also because published works rested largely on findings from the United States and lacked benchmarks in data from other societies.
2. Tomasson's (a) review of Western European sociology convinced him that it lags behind American sociology in carrying out cross-societal studies. Of course, the very use of the word "American" as the modifier denoting the United States betrays our parochialism. The American continent extends from the Queen Elizabeth Islands in the Arctic to Cape Horn at the southern tip of South America; Canadians, Mexicans, Brazilians, and Chileans also are Americans.
3. The first volume, issued in 1978, was published as *Comparative Studies in Sociology*.
4. Of course, some may disagree with my classification of the individual articles. However, in order not to understate the occurrence of reports that are cross-cultural, I probably have tended to err in the direction of including rather than excluding marginal articles.
5. Flinn has recently noted that since the 1960s, the decade in which much overseas development activity took place, fewer rural sociologists have been trained for international work. Graduate students find it difficult or impossible to obtain travel grants enabling them to conduct dissertation research abroad; assistant professors may stay clear of work in international development because its status in the eyes of review-committee members may be low; and fewer professors of rural sociology are actually engaged in international research.
6. However, Berelson and Steiner pointed out that "except for anthropological materials a large proportion of these findings are based upon data collected in the United States. The behavioral sciences are so much more active here than elsewhere that this is inevitable. . . . We have tried always to be conscious of this limitation, . . . but the reader should know that a good deal of what follows is limited, strictly speaking, to Western culture and even to the American scene" (8).

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# The Structures and Meanings of Social Time

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## ABSTRACT

*This paper proposes a paradigm for the sociology of time. After exploring some defining characteristics of social time, it presents a preliminary typology of social times corresponding to different levels of social structure. The linkages among levels of social structure and the temporal variables of embeddedness, synchronicity, and stratification raise important questions about the ways in which the forms of social time may be related. From the typology concrete propositions are extracted which address these questions as well as the question of how these relationships may affect organizations and individuals. Finally, the paper suggests possible ways to ground parts of the theoretical presentation in operationalized hypotheses ready for empirical testing.*

In the years since Sorokin and Merton published their groundbreaking article on the sociology of time inviting us to probe its qualitative and meaningful features, a number of insightful books and articles have dealt with aspects of social time. Most sociologists, however, treat time as incidental to other sociological problems rather than meriting investigation in its own right.<sup>1</sup> Consequently, there is a dearth of theoretical work on the sociology of time that might lead to a general paradigm for integrating the disparate empirical studies in this developing subfield.

More regrettably, many sociologists do not include time as a crucial variable in their studies, or else they introduce the temporal dimensions of social organization and interaction in an *ad hoc* fashion to assist explanation of social behavior. If social time received the attention it deserves in sociological investigations, no study of human organization and interaction would be considered reasonably complete unless it examined their temporal organization.<sup>2</sup> Sorokin stated, "Human life is literally an incessant competition for time by various activities with their motives and objectives" (209). Presumably, sociologists experience this competition with at least as much intensity as most others in modern societies; and yet, ironically, they often neglect its importance in the lives of those whose actions they intend to explain (cf. Hendricks and Hendricks).

Typical temporal demarcations are the concepts of past, present, and future. Each of these times can be defined with physical or social points of reference. Physical time so expands into the past and future relative to the human context that we rarely refer to points in physical time more than a few million years ago or a century in the future. Events in physical time outside these limits concern astronomers and theologians but few others. In contrast to reckoning time by a succession of physical events such as the orbit of earth around the sun, time can be humanized by establishing temporal references to *social* events. When we say, "I'll meet you after the game," we are using social time rather than physical or mechanical mathematical time, as would be the case if we were to say "I'll meet you when the sun is at its apex," or "at 12 o'clock noon." As Sorokin and Merton observed, physically based time-reckoning inexorably marches on in relatively homogeneous units, while social time unfolds with varying rhythms; sometimes rapidly, sometimes slowly, and sometimes with breaks (e.g., sleep or holidays).

We experience time both as physical passage and as a social procession. This intermingling of physical and social time is clearly displayed by the way we use the calendar. Significant social events such as the death of Christ are taken as temporal markers from which astronomical time units are defined. Similarly, when we refer to a child's mental age we are simultaneously taking social, psychological, biological, and physical time into account. Being reminded that an event took place "shortly after World War II" communicates more social meaning than being told that it happened in 1947. We tend to locate social events temporally with respect to other social events or periodicities and to use wholly physical time-reckoning devices simply to "mark time" between social events of interest.

Social periodicities do not appear when time is measured by merely physical succession. Social phases and cycles are identifiable in social time which may even appear nonperiodic in physical time. Sorokin and Merton explain:

The search for social periodicities based upon the unquestioned adoption of astronomical criterions of time may have been largely unsuccessful precisely because social phenomena involved 'symbolic' rather than 'empirical' equalities and inequalities; social processes which at present seem to lack periodicities in terms of astronomical measures may be found to be quite periodic in character in terms of social time (626).

The unevenness of social time compared to physical time emerges in reflection on the wealth of social meanings attached to events occurring in the development of the average American male between the ages of 12 and 18 as compared to, say, the ages of 32 and 38, or the impact of such colossal events as the Great Depression (Elder) which have lasting effects on individual lives. These major points in social time enter the definition of human

generations and are more useful sociologically, than defining generations in physical, biological time (see Mannheim; Ortega).

This discussion suggests not only that social time is quite distinct from physical time but also that it permeates every region of social life. This pervasiveness can be elaborated theoretically by identifying the different types of social time operating at different levels of social structure. To do this, however, we need an adequate theory of social structure. There are various views among social theorists about aspects or levels of social structure: How many distinct levels or components of social structure exist? How should they be described? Some theorists propose such aspects or levels of social structure as the individual, group, morphological, systemic, and cultural (e.g., Udy), while others find fewer levels of social structure or describe them differently. Most sociologists, both classical and modern, would agree that social structure can, and, for analytical purposes, should be conceived as embracing several more or less clearly definable parts or levels of analysis—for example, individual, group, societal and cultural levels.

Consequently, we treat the individual, group, and cultural level as the most common divisions represented in existing theories of social structure, and we use this tripartite division as the point of departure for the development of a typology of social times. That is, we show that each of these levels has its own forms of social time: at the individual, "self time"; at the group level, "interaction time" for informal interactions and "institutional time" for bureaucracies and other formal organizations; and at the broad, societal-cultural level, "cyclic time" (the day, week, and seasons) which cuts across the entire society.

To simplify the presentation of this typology of social times, Section I discusses self time and interactional time as representing the micro level of social organization, and this is followed, in Section II, by an analysis of institutional time and cyclic time which constitute the macro level of temporal social organization. The aim of these two sections, then, is to provide a "purposive, planned selection, abstraction, combination . . . of a set of criteria" for building a "constructive typology" of social time to foster the development of sociological theory (McKinney, 25).

Although a coherent typology of social times is vital to the advance of theory and research in the sociology of time, a fully articulated theory of social time requires that our descriptive classificatory scheme be joined with a *processual* analysis of the dynamic relations among the structures of social time given in the typology. Principles must be formulated to answer such questions as: How is self time related to interaction time? How is an actor able to move from one temporal structure to another in everyday life? How do individuals and social institutions resolve tensions and incongruities among conflicting timetables arising from the different levels of social time? Just as there is a need for society somehow to integrate different

levels of social structure into a unified process, there is a corresponding need to organize and coordinate the structures of social time.

A useful starting point for the conceptualization of these processual questions about social time is afforded by Gerth and Mills, and Mills. Their orientation to the study of social organization preserves the tension between the individual, biographical level of the social world and the collective, social structural level as they intersect historically (cf. Weigert). The individual's social roles constitute these points of intersection, and it is therefore predictable that the processual issues related to social time become manifest in role strain generated by the multiple levels of social structure and their corresponding temporal structures converging to produce conflicting temporal demands.

In Section III, we move toward a substantive theory of social time which yields a useful formulation of the aforementioned processual questions and tentatively suggests the social processes by which they are produced and may be partially resolved. The three key concepts we use—embeddedness, stratification, and synchronicity—are diversely present in the extant literature on the sociology of time, but they are not systematically investigated individually, much less in relation to each other. Our purpose in Section III is thus to formulate an orderly set of propositions around these three leading concepts which may serve as a basis for future theoretical and empirical work. Finally, in Section IV, we offer examples of how a researcher might deduce testable hypotheses from the set of inter-related propositions we have developed.

## I. Biographical and Interactional Structures of Social Time

### SELF TIME

Heidegger stated flatly, "There is no time without man" (16). This statement is patently absurd if taken as an assertion about physical time as process or pure duration. Of course, such was not his intention; nor did he intend that the statement be interpreted as a reference to social time. As an existentialist, Heidegger was interested in time as it appears immediately in the experience of the solitary ego. We might call this "self time." What Heidegger wished to point out is that without a sentient, rational being there is no past, present, or future, nor even a before or after. These structures are imposed on the world by our minds. They are not given in existence; they are projected onto existence in the form of remembered pasts, experienced presents, and imagined futures.

Heidegger claimed, moreover, that time is actually four dimensional. The relations between past, present, and future depend on a fourth temporal structure that, following Kant, he called "nearing nearness" of "near-

hood" (*Nahheit*) (15). It is the temporal equivalent of our experience of nearness in the spatial sense. When, for instance, we are in a car speeding along a road, we see objects in the distance slowly approaching us, quickly passing, and then fading away again in the distance. In addition to the experience of objects, we experience the process itself of their *becoming* near and distant. This is the experience of "nearing nearness" in the spatial sense. Similarly, temporal "nearing nearness" connects past and future to the present in that experience of events in the past and future are ordered with respect to their nearness or remoteness to what is taken as present. Hence, Heidegger conceives of the present as reaching out in both directions to embrace the past and future, not as an instantaneous razor's edge on which we are perched. Just as objects appear differently depending on their spatial nearness, events also appear differently when they are temporally near than when the "same" events are temporally distant.<sup>3</sup> Temporal *panic* is a reaction to the nearing time which is approaching faster than the person's ability to finish the present act requires (Lyman and Scott). During eras of rapid social change, self temporal panic becomes cultural "future shock" (Toffler).

Unlike physical time, self time is not homogeneous. Events in the individual's life which are quite distant in physical time may be represented in consciousness as vividly as memories of what happened five minutes ago. Cottle refers to this as spatial time in that our memories can manipulate events in time as if they were ordinary material objects that can be moved around at will. Spatial time contrasts with the linear conception of time by which events occur in an inviolable temporal sequence. In spatial time, we can bring a past event into the present and make it into a different event; histories and life stories are never fixed. During totally engrossing activities such as playing chess, performing surgery, or rock climbing, self experiences a formless sense of flow in which time seems to cease (Csikszentmihalyi). The types of experiences one has, their temporal nearness, and their spatial forms in memory assure that each person's sense of "self time" is unique and has significant effects upon interaction with others.

#### INTERACTION TIME

Whenever two or more people are interacting directly, self time is partially overlaid with a different type of time frame, namely, "interaction time." Since it is an intersubjective reality, interaction time is only partly within the experience and control of each self. The flow of interaction time depends on the (incompletely predictable) actions of the other as well as the prevailing rules which define appropriate "turns" in interaction. The relative social statuses of the interactants entail norms that govern turn-taking and other temporal intervals in interaction time (cf. Schegloff).

In addition to generalized cultural norms of interaction time such as turn-taking, there are special interactional rules which develop among such social dyads as friends, best friends, acquaintances, strangers, adversaries, bitter enemies, or salesperson-customers. When one person appears to be following rules of interaction time which the other views as inappropriate for their presumed relationship as he or she defines it (e.g., meeting a friend on the street and treating him with nothing more than a curt "hello" normally reserved for acquaintances or even strangers), some type of account or conversational repair work is immediately called for. Otherwise, their relationship is subject to redefinition (Scott and Lyman).

A critical structural feature of social time which influences the temporal organization of interaction is the fact that all social acts are temporally fitted inside of larger social acts. We call this *time embeddedness*. For example, a person who stops during the rush hour to buy a newspaper on the way to the bus stop knows that the interaction time available to chat with the newsvendor is quite limited; conversely, the newsvendor knows that he should not attempt to engage customers in protracted exchanges. To do so would profoundly disrupt the orderly flow of daily life for everyone involved and, indirectly, countless others. The dire consequences of even small deviations in the temporal progression of embedded actions are painfully illustrated by the reverberations which follow when an international airport is forced to shut down for only a few hours, or when a big job falls through just as a big debt falls due! Time embeddedness is reflected in the multiple perspectives which make up social worlds and the multiple roles which make up self-awareness (Mead, a; Tillman).

Time embeddedness forcefully affects the definition and process of interaction in those instances where one or both persons have their temporal structures tightly embedded (e.g., "I am sorry, but I must leave because I have another appointment"). Tightness of time embeddedness varies by social class and age group, and it is closely correlated with one's image of the future. Cottle and Klineberg note that humans spend relatively little time in the present compared to other animals; our thoughts and actions are oriented toward the future and (to a lesser degree) the past. One of the most pleasant aspects of *pure* recreation is that it allows us to live wholly in the present as the experience of passing time temporarily ceases. Probably, small children are most present-oriented and older persons are most past-oriented. For this reason, elders tend to be less emotionally anxious in contemplating an uncertain future than are young adults who not only have a much longer future-oriented temporal horizon but are also far more preoccupied with attempts to control oncoming events as they become both near and distant.

Persons differ in salient time focus according to whether their primary concern is with short-term or long-term futures (Polak), and whether they take a fatalist or self-determinist view of their futures (Bell and Mau;

Coser and Coser). These variations in future-related temporal structures have implications for the way in which people manage interaction time and the time embeddedness of their lives. One extreme implication is demonstrated in the experience of two persons who grow old together (Schutz). The past and futures of long-time friends or lovers are inter-subjectively experienced as so intermeshed that their interaction time takes on special qualities of shared experience not found in any other interactional context. Members of living groups, like families, must synchronize their variously embedded times if they hope to attain temporal coordination among themselves as competent family members (Kantor and Lehr).

## II. Institutional and Cultural Structures of Social Time

Not only are self time structures embedded within interactional time structures, but both of these micro-level temporal structures are, in turn, embedded within the larger, macro-level temporal orders of social institutions and of the culture. This embeddedness constitutes the temporal integration of the different levels of social structure and gives rise to the need for temporal "stratification" and "synchronicity"—two concepts which we develop here and in Section III.

The distinction within the macro-level structures of social time between institutional and cultural structures is based on their differences in both form and scope. Within the institutional realm, individual organizations (schools, factories, etc.) which make up each institutional sphere construct their own time schedules and rules. Although they may (and typically do) take into account time structures of other organizations with which they must conduct exchanges, the norms and sanctions governing the use of time in any particular organization extend directly only to its own members. On the other hand, there are culturally based time structures (day, week, seasons) which extend in some form or other to all functioning members of society.

In industrial societies, there is also a marked difference in *form* between institutional and cultural time structures. The most fundamental difference is that the cultural structures, such as the day, week, and seasons, recur in endless cycles or what we simply term cyclic time, whereas most organizations operate on linear time in which persons and objects pass through temporal frameworks that are nonrepetitive or repetitive at irregular intervals. For these reasons, we separate the discussion of macro-temporal structures into the two headings cycles of time and institutional time.



## CYCLES OF SOCIAL TIME

The future in social time is not the colorless, textureless, empty expanse it appears to be in purely physical time. On the contrary, it contains all our hopes, fears, and aspirations. As such, we approach it not as a smooth and standard sequence of ticks of a clock to be lived through, but rather as a sequence of whole blocks of time which contain partially predictable and broadly recurring sets of meaningful events. It is around these blocks of time that we construct the cycles which organize social time into smaller and larger temporally embedded structures. We turn now to a description of the three cycles that in our society are based more or less on three natural sequences defined as meaningful units of time. We call the social cycles corresponding to these three units the daily round, the weekly routine, and the yearly season.

*The Daily Round*

The physical day with its sequence of darkness and light and the gradations of dawn, morning, afternoon, evening, twilight, and night, provides the basis in everyday life for the quotidian social temporal structure, or daily round of activities. The daily round is marked by the two organic events of waking in the morning and falling asleep at night.<sup>4</sup> A modern industrialized and rationalized society can function only if most of its members follow a highly patterned and dependable daily round. Simmel observed that the portable timepiece or wristwatch helped create a mental life for urban-dwellers which enables them to mesh their daily rounds with a precision unattainable in a rural setting where time is marked by more natural rhythms (e.g., the crowing of a rooster). Unlike the farmer, city-dwellers must synchronize their watches with those of millions of others. Thus their participation in the maze of daily rounds is so carefully coordinated that each day society's daily round is launched precisely on time (cf. de Grazia). Not to have the correct time in modern society is to risk social incompetence. Consequently, the offer of correct time is a form of mutual aid which cannot be refused, even among city strangers who may have nothing more in common than their collective bondage to clock time.

In some ways, our subservience to rigid clock time constitutes a form of alienation from what would be the ordinary course of social events not dictated by clocks.<sup>5</sup> Church services, college courses, television programs, lunch time, and many other activities are orchestrated so as to "fill up" precisely the length of clock time allotted for them and to end on time, regardless of the social or psychological state of the participants. By thus reifying clock time, we rob the social world of much spontaneity, creativity, and novelty. As smaller and smaller units of clock time become invested with increasing significance, everyday life may resemble the tight temporal structure of total institutions in which the daily round is precisely regulated

(Goffman, a). With this tightness come greater stress, tensions, and time-induced anxiety.<sup>6</sup> Ironically, despite the stress-producing effects of reified clock time, we not only *expect* rigorous temporal control of events but positively *value* it, as is evident from the anger and frustration felt when scheduled events are delayed, postponed, or cancelled. Perhaps in societies less mechanized and bureaucratized, spontaneous disruptions of the routine course of social life are more welcomed, or at least less threatening.

### *The Weekly Routine*

The daily round occurs within another temporal structure—the weekly routine. The seven-day week of the Western calendar reflects an ancient religious motif of six days of work followed by a day of religious activities and physical rest. The establishment of Sunday as a day for religious observance is an example of the traditional power of organized religion in structuring the calendar in such ways as to foster religious consciousness through the creation of holydays. The attempt to secularize the calendar in the French Republican calendrical reform of 1793 was met by stern resistance, and its failure demonstrates the deeply rooted institutionalization of the Western temporal framework (see Zerubavel, a).

Nevertheless, the definition of Sunday and the activities legitimately performed on that day have changed significantly in recent decades. Professional sports now fill the relatively empty temporal slot available on urban Sunday afternoons. The takeover of Sunday for spectator sports was completed with the coming of television. Another example of the secularization of “the Lord’s Day” was the battle over retail and commercial establishments doing business on Sunday. While many laws prohibiting Sunday business operations have been repealed, the battle is still being waged for sale of over-the-counter alcohol. Finally, the secular shift is further manifested in declining attendance at Sunday church services and the shift of church services to other days of the week. During the historical period which included the secularization of Sunday, the labor movement was winning a shorter work week (see ISR Newsletter, 12). These two developments help divide the week into two distinct periods: work days and the weekend.

The importance of the weekend as the dominant temporal marker of the weekly routine has gained highly symbolic recognition. For example, the national sacred days, like birthdays of Washington and Lincoln, are now moved to Monday regardless of the actual day of the week on which the birthday falls, further strengthening the weekend as a sociocultural time out (Lyman and Scott). The meaning of the weekend as a period of time free from mundane work is so firmly ingrained in our culture that even patients in mental institutions appear to take time off from their crazy behavior on weekends, provided that the hospital staff is properly attentive to their symptoms of craziness on weekdays (Melbin, a). Even needed

professionals like doctors, dentists, and clergy now take off for all or part of the weekend. The borrowing of the English term, weekend by European languages indicates diffusion of this temporal meaning.

The daily round is itself modified depending on the day of the week. The folk definition of "blue Monday" receives empirical support from a study of American boys' evaluation of the days of the week (Osgood). The days were ranked in the following order of positive to negative evaluation: Saturday, Friday, Sunday, Tuesday, Wednesday, Thursday, and last, Monday. The negative meaning and effects of Monday are firmly signified by the fact that, of men having no previous history of heart disease who suffer a sudden-death heart attack while at work, fully 75 percent die on Monday (Rabkin). And, of those who died at home, 46 percent died on Monday. The weekly routine suggests that Americans live *for* the weekends while merely living *through* the weekdays. The fact that most special events and leisure activities are appropriately scheduled for weekends is a significant occupational disadvantage for those whose occupational time is structured so that days off occur on weekdays rather than on weekends. Clearly the meaning of the weekly routine is an important feature of our everyday lives, and a full understanding of ourselves, our family, our neighbors, our health, and our job requires that we grasp the meanings of the phases in the weekly routine and how they merge with institutional demands.

### *The Yearly Seasons*

Like the days of the week, the weeks of the year have special meanings. For such purposes as paying bills, weeks are temporally organized into monthly cycles, but months are generally less significant in our lives than are the *seasons* in which they are grouped (see Michelson). Particularly in the temperate zone, the four seasons provide a powerful natural sequence which humans have from the most ancient times transformed into socially meaningful periodicities. The seasons elicit different affective responses from individuals depending on their social class. For example, the junior executive moves to the cool of a lake cottage and enjoys a leisurely season of work-plus-recreation during the hot months of summer. The executive lives a summer season with a meaning contradicted by that of the inner city ghetto janitor for whom the summer season brings stifling heat, humidity, smog, constant street life, thin tempers, and the threat of riot over a long, hot summer. The meanings of the yearly seasons thus affect our everyday lives relative to our position in society (cf. O'Rand and Ellis).

Two significant changes come with the seasonal cycles and add to their distinctive social definition. First, our life-styles reflect changes in weather and temperature as we adopt the food, clothing, work, and recreational activities appropriate for the season. Second, each season is characterized by dominant holidays: Christmas–New Years, Easter, Fourth of

*July, and Halloween–Thanksgiving.* The meaning and mood of each season are set by its principal holiday, and this aura penetrates our lives from season to season probably far more than we realize consciously, whether we react with joy or depression.

An evaluation of the seasons and months of the yearly cycle is also included in the study of teen-age American males (Osgood). Winter is evaluated lowest, mainly due to the lowest month of the year, February, a slightly better evaluation of January, and a somewhat more positive evaluation for December. There is a sharp rise in positive evaluation for March, and somewhat more for April and May, resulting in a combined judgment of Spring as the second most positive season. The positive judgment peaks for June, the highest of all months, followed by a slight drop for July, the second highest month, followed by a sharp decline for August, almost to the level of March. Altogether, Summer is the highest ranking season. The decline in evaluation for August continues for September, followed by a slight rise for October and a slight decline for November, making Fall the third ranking season. In interpreting these findings, we must recall that they refer to teenage males. Persons of other social status, or from differing geographical regions, would likely offer different rankings.

This discussion of the yearly seasons finishes the temporal cyclic structures based rather directly on cosmological sequences and the biological necessity to alternate periods of activity and passivity. Since the seasonal sequences are objectively in nature, they are universally available for temporal meanings. Through symbolic transformation, these sequences enter our lives and become part of the reason why we think, feel, or decide in one way or another at this particular time. People committed to an astrological interpretation, for example, fashion their lives differently from disbelievers. So too, a Christian structures the yearly seasons, weekly routines, or daily rounds differently from a Jew; or a 70-year-old farmer differently from a 26-year-old apprentice carpenter.

Cyclic and repetitive time is essential for the experience of stability and sameness, even as time passes. Day follows night and night succeeds day; weeks cycle recurrently; yearly seasons repeat themselves. The great temporal order and permanence of the cosmos is experienced through the cycles which provide the basis for repetitive time.

#### INSTITUTIONAL TIMETABLES AND INDIVIDUAL CAREERS

We have two experiences of personal time. The first we share with all living organisms: life begins, we grow, age, and die. In everyday life, however, these physical stages are symbolically transformed into socially and psychologically defined stages and personal identities (cf. Erikson). It is this second biography, the social biography, which is of particular interest to sociologists. The temporal structures of biological "living clocks" (Ward)

control our physical biographies. Extreme life-styles can somewhat slow down or speed up the aging process but, in any event, the living clock marches onward.

A very different type of clock—social time—regulates our social biographies. Society tells us when it is time to vote, drink alcohol, drive a car, go to war, go to school, marry, run for political office, and retire from work. In our society, we are physically mature before we are defined as socially adult. This lack of correspondence between the two biographies places teenagers and young adults in our society in an awkward and ambiguous position. In a way, herding large numbers of these persons into colleges provides an appropriately ambiguous environment where they may be conveniently stored during a psycho-social moratorium until, it is hoped, their two biographies are synchronized.

Society lays out a time track for persons, and, from the track, we derive appropriate timetables and schedules (Roth). The schedules force each individual to construct a biography by passing through statuses partly determined by nature but more importantly by the conventional structures of social life. This biographical time is marked by the individual's "status passage" (Glaser and Strauss) through the various positions and identities available in society. Clearly, the availability of these positions is strongly influenced by chronological age, sex, race, and social class of origin. Such factors determine at what point in social time, if any, these positions and identities become available to the individual. The *timing* of such access is as socially consequential in many respects as is the simple *fact* of access—a factor rarely appreciated in the usual types of research in social stratification and mobility (see SanGiovanni).

The passage of the person through a number of statuses which are meaningfully related to each other in a recognized sequence makes up a career (Hughes; Roth). Instead of the common definition of career in the occupational world, we use the term to refer to "the moving perspective in which the person sees his life as a whole and interprets the meaning of his various attributes, actions, and the things which happen to him" (Hughes, 137). In this sense, career refers to sequences of statuses which make up a unified time period, like the four years in high school or a mother's child-bearing career. Over a period of social time, people develop a perspective through which their various careers are endowed with certain valuations and meanings. And each career is subject to reassessment as the person passes from one to another.

Careers establish a critical sense of social time. Like a clock, social biographical time runs or ticks away as the individual passes not only from status to status within an institution such as a high school, but also from institution to institution such as from high school to the labor force to the retirement home.<sup>7</sup> This is why social time moves rapidly from birth to adulthood and much more slowly thereafter, as the older individual typi-

cally goes through fewer (and socially less significant) status passages. However, the pace of status passages increases again in the later years: people often experience retirement, the death of a spouse, declining health, changed living arrangements, and the like.

Most statuses have their own socially prescribed durations. A career has its timetable (Roth) which contains a schedule allocating the normal time for each person to pass through the designated statuses. To pass through at a faster or slower rate results in being identified as a social outcast or age deviate, as having extraordinary personal traits producing the departure from age-graded normalcy (SanGiovanni). The prospect of being singled out as abnormal is often sufficiently disconcerting to compel individuals either to under- or overachieve in an effort to conform with the timetable—another example of fitting social activities to the dictates of social, calendar, and clock time. Where the timetables are arranged in sequences, to be off schedule on one timetable condemns one to arrive early or late at the next.

Consider a typical middle-class biography. The infant is born in a hospital, in a highly organized setting which sets the style for standardized statuses. Even before the child enters kindergarten, he or she has probably experienced the bureaucratic timetables of a pre-school, day-care center, or nursery school. Each of these institutions has well-established criteria of efficiency, standardization, and relatively clear markers of progress. This is anticipatory socialization for the next twelve to perhaps twenty-five years during which the person's educational career unfolds within the bureaucratic organization of the schools.

When this person enters the occupational world, a different, but equally demanding, timetable appears. A significant difference between such free professionals as artists or writers and most other workers, especially blue-collar workers, is their relative freedom from control by clock time (see Sennett and Cobb): they are not as constrained to a rigidly prescribed daily round (Zerubavel, b;c). Unlike the factory worker, the free professional can avoid the daily grind of rush-hour traffic, the long lines at the grocery store from five to six o'clock in the evening after work and, most of all, the rigid hours put into work and their distribution on a daily, weekly, and seasonal basis. From the standpoint of social stratification, this is an intangible, though real, advantage. Indeed, some people gladly exchange more tangible benefits, including money, in order to have it.

There is, as well, a definite *stratification* among the times we have described. Generally speaking, organizational time demands precedence over interaction time, and interaction time, in turn, demands precedence over self time. Two employees, for example, would get into serious trouble with their supervisor if they habitually allowed their lunch-hour conversations (interaction time) to extend beyond the time allotted for lunch (organizational time). Similarly, one would risk being defined as quite rude

were one to begin working on a grocery list (self time activity) in the middle of a conversation (interaction time). There are, of course, special circumstances in which this ordering of temporal stratification may require reversals of the usual priorities, but such exceptions are relatively uncommon and demand an appropriate account (Scott and Lyman). The creation and maintenance of social order require temporal stratification, and there are strong sanctions to assure the stability of this system, since stratified times are also embedded within each other.

The rigidity of bureaucratic timetables is a consequence of the future-oriented character of organizational time. Organizations are more consistent, systematic, and rational (in the Weberian sense) in their temporal orientation toward the future than are most individuals. Bureaucracies continually estimate the time required to complete current projects and anticipate timetables of future projects. Once resources are allocated based on these timetables, workers are under pressure to comply with the timetables. Even though companies which have cost overrun provisions in their government contracts may not suffer economically if they deviate from the timetable, they nevertheless endure political pressures from both the government and the public. Although tight temporal structures are more characteristic of bureaucratic organizations than of other types of organizations, even voluntary associations tend to make escalating demands on one's schedule and total time commitment once one assumes a central role in the organization (Moore, b). The basic social source of rigidity in organizational time is its high degree of stratification and embeddedness. In factories and many other types of organizations, production is organized according to a fixed sequence of phases or stages. When one phase of the process takes too long, it disrupts the timing of other phases. This has clear policy implications for anyone seeking to instill greater temporal flexibility in an organization (e.g., "flextime").

In summary, organizational time has the same basic dimensions found in other types of social time. The principal difference, however, is that in organizational time these dimensions (future-orientation, "blocking," stratification, embeddedness, etc.) are far more structured and contain much less undefined or free time. This is one of the features of organizational time which makes it difficult to coordinate the simultaneous social times which continuously impinge on one's everyday life. Self time, interaction time, biographical/career time, organizational time, and other forms of social time would be difficult enough to manage alone, but we must face the enormously more complex task of somehow coping with the cross-cutting temporal demands they exert on us at single points in physical/astronomical time. Try as we may to keep these times segregated in physical time, they all have the nasty, imperialistic feature of invading other temporal realms.

As a result of temporal stratification, when competition between

forms of social time arises, it is self time which is frequently the loser. In a sense, self time is always potentially with us, but it is submerged under other types of social time much of the (physical) time. In modern industrial societies which value individualism, it is not at all uncommon to hear the complaint that someone just wants "to be left alone," or "to have time for myself."<sup>8</sup> This realization is symptomatic of the increasing temporal embeddedness of social times. Attempts at planned segregation to alleviate temporal pressure are destined to be self-defeating since it is *planning* itself which is often responsible for modern manifestations of temporal embeddedness (Berger, et al.). We might as well attempt to extinguish a fire with gasoline as reduce pressures from temporal embeddedness by more planning. The fact that our first impulse when faced with the temporal problem is to seek a solution through rational planning only demonstrates the pervasiveness of the rationality of organizational temporal structures. As a generalizable solution to time problems, it is, nonetheless, an illusion. A planned rupture in the seams of temporal embeddedness is a contradiction. This would seem more obvious if our faith in the moral values of the organizational society were less complete (Presthus).

Interaction time, like self time, is typically subordinated to organizational time in two ways. First, organizational time dictates the points at which interaction time may enter (e.g., the coffee-break; the end of work). Second, organizational time partially controls the *pace* of the interaction as, for example, two persons quickly finish their conversation when they must return to work. In our society dominated by work and mass media, leisurely conversations are the exception rather than the rule.<sup>9</sup> Furthermore, they are concentrated in the weekend (and evenings) which likely contributes to the sense of the weekend as a break from mundane time. The pressures on interaction time are felt acutely by anyone who has attempted to keep up letter correspondence with old friends. Many may try but few succeed for long.

An important subcategory of interaction time, family time, is reserved for interaction with relatives, particularly one's immediate family. When organizational time interferes with other categories of interaction time, we are sometimes a little angry or frustrated. But when it disrupts desirable family time, the resentment may be far more intense and the consequences correspondingly more severe. For married persons, especially women, managing the cross-pressures between organizational time and family time is a difficult problem in temporal juggling. The rising divorce rates as more women enter the full-time labor force is not unrelated to this problem.

Marks has laid the foundation for a productive controversy over how this tension between interaction time (especially family time) and organizational time is best explained sociologically. Marks opposes those theorists (e.g., Coser, b; Goode; Moore, a; Slater) who argue that the



emergence of multiple roles in modern society, each of which competes for one's time, makes time into a scarce resource. Insofar as there is never enough time to distribute among these social roles, role strain invariably results. From this perspective, we may speak of time as if it were money. We can save time, invest time, waste time, borrow and spend time, or capitalize upon time (cf. Stephan).

Marks contends that this scarcity theory of time is essentially an ideology proposed by these authors as an excuse and rationalization for over-commitment to their own occupational roles. The felt strain between one's occupational career time and family interaction time is, according to Marks, the product of the husband's over-commitment to his work role and under-commitment to the family role which forces the woman to be over-committed to the family role, leaving her under-committed to the work role. For Marks, if each had a balanced commitment to his or her roles, the time scarcity problem could be resolved; thus, social time is *not* so inherently scarce in modern society—at least not in the way the relationship between family and occupation has been interpreted by mainstream sociologists.<sup>10</sup>

The weakness of Marks' argument is that he does not tell us how to define a proper level of commitment to a role, nor how to transcend the limits imposed by physical time on social time. Presumably, proper role commitment is determinable only after the fact—that is, after the wife threatens to leave her husband who spends too much time at the office, or the husband complains that his working wife is neglecting the children. As long as no problem arises in one's various roles and relationships, who is to say what is over- or under-commitment to any of them?

Hence, what Marks ignores is that the expectations of *others* contribute to the determination of optimal levels of commitment and temporal allocation among multiple roles. High family demands and low work demands form a commitment pattern opposite that of low family demands and high work demands. The strain arises, then, when both family and work demands are high. Marks treats this combination as a consequence of an improper commitment structure on the part of the individual. It might be more profitably thought of as a result of a commitment demand put on the individual from the outside, or as the inherent scarcity of time in the broadest sense. The individual may, to some extent, attempt to renegotiate these demands, but it is a serious mistake to regard them as always internally generated, or as resolvable in principle. A sociology of time forces us to recognize time, no matter how defined, as a physically and phenomenologically grounded *scarcity*. Thus, like all scarce commodities, it acquires meaning relative to one's position in the stratification system and one's resultant ability to control it.

To illustrate the limited negotiability of role-based social time, it helps to return to the notion of careers. As a useful simplification of the

problem, consider the dual labor market distinction (see Gordon). The occupational career of a worker in the secondary sector of the occupational structure is characterized by low earnings, low job security, little opportunity for advancement, and the like. This sector is exemplified by most types of unskilled labor. Conversely, the primary sector is marked by high earnings, job security, opportunity for advancement, high social status, etc. Professional occupations are the prototype of the primary sector. In terms of social time differences, the total temporal commitment demands of the secondary sector occupations are far below those of the primary sector. Indeed, for many types of secondary sector occupations, the temporal demands are virtually fixed by clock time. There is no incentive, and even little possibility, to extend personal temporal work commitment beyond clock time specifications. On the other hand, many types of professional work make unspecified temporal demands. Success in the profession requires that standards of professional competence and availability be displayed, and the person is held responsible for investing however much time is required to fulfill these conditions. Consequently, the idea of occupational commitment, when understood as time commitment, has different meanings when applied to primary and secondary sector occupations.

Defining over-commitment in primary sector occupations is far more complicated than Marks seems to allow. One can be over-committed in terms of one type of social time but not another. In organizational time, it can be said that a professional is under-committed whose performance does not meet the minimal standards of the employer or the profession, but it would have to be shown that inadequate performance was the result of an insufficient time commitment rather than simply a lack of professional talent. In terms of organizational times, over-commitment similarly appears as performance beyond minimal professional standards. However, from the perspective of *biographical* time, extraordinary performance in one's professional role may represent only the commitment level required to keep one's career on schedule; therefore, it constitutes over-commitment only in organizational time. Even then, the commitment is "over-" in a statistical, not normative, sense.

Passing from organizational and biographical time to interaction time, we see another sense in which one may be temporally over-committed to a role. If a person pursues occupational goals at the expense of the temporal demands of interactional roles, then he or she can be said to be over-committed to the work role from an interaction time perspective. Our point is that over- and under-commitment to multiple roles cannot be adequately defined without explicitly specifying which type of social time is being used as the reference criterion, and without explicitly facing the synchronization issue. We may complicate analysis even further by considering the meaning of over-commitment to a role in *self* time. In self time, one is over-committed if one *feels* over-committed in terms of alternative

uses of time, regardless of how well one may be meeting social time distribution obligations in the judgment of others.

Self time, interaction time, organizational time, family time, biographical time, biological time, clock/calendar time, and physical time all move along, like all processes, at their appointed pace and, like all structures, resist change. When they collide—and their collision is a permanent feature of the human condition and characteristic of modern society—we bear the incongruities as best we can. One way is simply to wait. We wait for people and things all our lives. Schwartz (a, b, c) has studied waiting extensively as a social phenomenon. He shows that waiting is a revealing indicator of power relationships in interactional settings, such as waiting in the waiting room to see a physician, lawyer, or politician. If the fluxes of the various times we have discussed always coincided perfectly, nobody would have to wait unless disruptions from unpredicted natural causes occur. When we sometimes grow weary of waiting, it might help to imagine how frightfully dull life would be in such a world! Yet, because the social timetables are not coordinated, *someone* must wait, and wait, and wait—and it is the person with relatively little power whose time can be wasted by those with more power.

The analysis of decisions to wait is a useful approach in understanding the process by which incongruities in social timetables are resolved in everyday life. The young couple waits to buy a house or to have the first child; the young mother waits to reenter the labor force; the child waits for the privileges of age; the junior executive waits for the opportunity to move up in the corporation. Although deferred gratification may eventually lead to nongratification, tolerance for waiting remains an essential element in many aspects of modern socialization. In fact, in the fullest meaning of the word, only humans wait. Animals opt for immediate gratification of biological impulses whenever they can. Since humans only partially transcend their genetic heritage, babies and young children must be taught to wait. Those who do not learn the lesson well suffer the consequences as adults when their inability to wait and their attempts to have all things at once lead to ulcers, divorce, bankruptcy, and other possible symptoms of serious dislocations in social timetables.<sup>11</sup>

In traditionally institutionalized role passages, waiting is prestructured; for example, four years or 120 credits before graduation, or thirty-five years before retirement. In emergent role passage, however, like the transition into secular society after ten or fifteen years in the religious life of a convent, careful waiting for the appropriate moment to make each move becomes a strategic task for constructing a meaningful passage and successful transition to a new identity (SanGiovanni). For someone making an emergent role passage, such as ex-nuns, new hostesses, transsexuals, or ex-prisoners, time takes on a clearly qualitative reality captured in the sense of waiting to make the move at the right time, lest all be lost. For

dramatic artists, stand-up comedians, or competent citizens, waiting for the opportune time is of the essence.

### III. Toward a Paradigm for the Sociology of Time

The exploration of social time is a deeply fascinating and in principle inexhaustible inquiry into a basic feature of human sociality. Time, like space, is a deep process/structure of human life at the conjunction of physical and symbolic reality made meaningful as a symbolically transformed environment (Harre). We have touched on only a few facets of social time. The primary consideration is that social time be interpreted as another form of human *meaning* constructed in the processes of interaction, limited by the physical realities of organism and nature, and structured into the institutions and organizations of each society (Berger and Luckmann; Blumer; Mead; Sorokin; Zerubavel, b).

We emphasized three features of social time which are at the heart of our constructed typology: embeddedness, stratification, and synchronization. Embeddedness recognizes that human life and the social actions which constitute it are a complex overlap of actions and meanings at various stages of enactment. A seemingly privileged middle-aged male careerist gradually senses the tensions of a mid-life crisis as he makes the inevitable transition into another season of life. A constitutive feature of the middle-aged person's experiences is the embeddedness of social time: he discerns the fundamental anxiety-generating fact that he, like us all, will soon die and can count on only fifteen or twenty more years of productive life; this dawning awareness is embedded within a marriage which is presumably running its course for the rest of his life, no matter how long or short; embedded within these social times is that stage of his career which has now plateaued for the foreseeable future, the time of his children's education which is now more expensive than ever, and the last vigorous period of his physical life for living out youthful fantasies or fulfilling adult plans. No wonder he senses temporal panic (Lyman and Scott).

Temporal embeddedness works as a mechanism making the experience of self continuity, a permanent identity across differing situations, plausible. Temporal embeddedness is a plausibility structure for the experience of the unity and continuity of an increasingly complex modern self (cf. Berger and Luckmann). In traditional religious societies, self time is embedded in a transcendental time: the afterlife, eternity, or the collective life of the group. The social organization of religion supplied the plausibility structure for a sense of self continuity and identity throughout life and after death (Bellah; Berger). In secular and pluralistic modern society, temporal embeddedness is limited to the mundane realities of lifecourse, career, institutional timetables, and personal plans; it offers a more precarious

continuity of subjectivity and self but it is the only type of plausibility available to moderns (Berger, et al.; Toffler).

The second feature of social time is its stratification. Experientially, as the phenomenologists emphasize, time and space extend out from the felt and embodied present with self as the 0,0 coordinates. Sociologically, however, if we study the organization of time in Durkheimian terms, its objective and constraining facticity and the organizational power or interactional sanctions attached to conformity or deviations from that time, then time devolves on individuals from the societal and cultural levels. The state tells individuals when they can vote, marry, enter contracts, possess civil rights, or qualify for social security. Self time, in this paradigm, is like an experiential, imperfectly transparent amniotic sac within which we live and through which we experience the myriad of objective and constraining times structuring our experience.

As a central structural feature of human life, the stratification of social times works as a mechanism making the experience of self-control and social control plausible as a single reality. The objectivity of human life derives in part from its locations in the stratification of social times, within which the self acts now as a free individual, now as a follower of timetables of the state, now as moving through the career scheduling of an institution. The objective predictability of individual action is insured by the objective social times structuring everyone's life. Properly meeting the expectations of timing stratified into a society warrants a person's moral character and displays his or her normalcy. To fail to time one's life according to the stratification of social times elicits labels of laziness, shiftlessness, untrustworthiness, and clearly inferior selfhood—constant puritan reactions to those who do not time their lives according to that of modern bureaucratic society. Such negative responses have cause in spite of faulty interpretation: it is not only that time is wasted, a form of one of the Seven Deadly Sins (Lyman); but also that the absence of a common stratification of social time makes it impossible to plan—a key modern organizational and psychological feature.

As the third structural feature of human life, synchronicity is a derivative of temporal embeddedness and stratification and works as a mechanism for making the rationality of human action and planning plausible. Rationality involves the ordering of actions and expectations as means for the achievement of future goals. Such ordering is an intersubjective emergent: rationality is essentially a public reality by which a number of individuals make the same sense of the future. Synchronizing one's life is a public achievement which merges the unbridgeable individuality of personal existence constituted out of embedded time with the irreducible collectiveness of social order constituted by stratified social times. The synchronicity of an entire society continually recreated by the billions of multifarious actions of millions of citizens makes plausible the rationality

of that social order. Breakdowns, strikes, terrorist attacks, earthquakes, and other societally unsynchronized events challenge the public rationality.

The formal organizations of modern society are marvels of synchronicity, captured in the split millisecond timing of a moon landing or the carefully calculated pace of an assembly line. These are also the most rational of institutions—as long as they remain synchronized! The complexity of modern society is directly related to the increased synchronization of stratified and embedded times. Thus do personal lives take on the rationality of their historical period by synchronizing themselves with their structured environment (Harre). In this way, the objective and constraining structure of stratified times becomes the subjective and meaningful embeddedness of self times, and persons live synchronously, and thus plausibly, with their historical times. The external structure of social times become the taken-for-granted normal form of each person's existential time (cf. Cicourel). They feel at home.

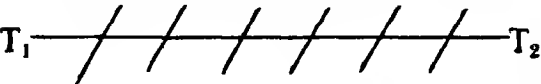
When our typology of social times is theoretically integrated with the concepts of temporal embeddedness, stratification, and synchronicity, we have some of the necessary building blocks for a formal theory of social time. To illustrate this potential, we extract and relate key propositions and corollaries which are implicit in our discussion thus far. Given its preliminary quality, it is offered as an outline to be refined by future theoretical and empirical work.

**PROPOSITION 1.**

*The greater the number of temporally embedded events between two points in physical time, the shorter is the perceived temporal distance between the points:*



(temporal distance between  $T_1$  and  $T_2$  appears relatively long)



(temporal distance between  $T_1$  and  $T_2$  appears relatively short)

Where // = two temporally embedded acts and  $T_1$  and  $T_2$  = two points in physical or clock time

*Corollary:*

- 1.1 Increasing the social integration of an individual by adding more social roles causes him/her to preceive an increase in the pace of social time, thus reducing perceived temporal distance between points in physical time.
- 1.2 Because older persons have longer temporal "horizons" or memories,

a year appears as a temporally shorter distance to someone 80 years old than to someone 8 years old.

Among a group of interactors, as the number of embedded events within the space of a given span of physical time increases, the interdependence among the actors increases. For instance, people have to be available for interactions at precise and predictable points in time (appointment schedules, etc.) in order for the expanded set of embedded acts to be completed within a fixed cycle of clock time. This coordination of embedded interaction times is possible only if there is an increase in the temporal synchronicity of the various social times involved. Therefore, analysis of the social consequences of temporal embeddedness leads directly to the matter of synchronicity.

As Durkheim observed, the most fundamental process of social change has been the transformation from mechanical to organic solidarity. The functional interdependence of diverse social roles which characterizes the organic mode of solidarity is most pronounced in contemporary industrial societies. Concurrent with the changes in the social organization of other institutional spheres there has been a sweeping reorganization of social time (self, interactional, institutional-organizational). Generally, these changes may be described as a quantum leap in the temporal synchronization within and among the elements of every level of social structure. The concept of synchronization refers to the process of fitting together different timetables so that highly complex and intricately embedded sequences of social action unfold on schedule according to *all* of the relevant social times involved in the process.

It is possible to trace the social consequences of this ever-growing synchronicity not only for the organization of society at large but also for the consciousness of its individual members. These consequences are displayed by the following set of interrelated propositions and corollaries:

**PROPOSITION 2A.**

*The greater the interdependence of actors, the greater the necessity for temporal synchronization.*

**PROPOSITION 2B.**

*The degree of difficulty in temporal synchronization is a positive exponential function of the number of timetables involved.*

**Corollaries:**

**2.1** As synchronization pressures increase in a social system, the norms of interaction time and organizational time increase in number and specificity.

This has feedback effects which further increase synchronization pressure.

2.2 Synchronization in one subunit of an organization creates synchronization pressure in related units.

2.3 As synchronization advances, smaller and smaller units of physical time become socially meaningful.

2.4 As smaller durations of physical time become socially meaningful, the perceived "scarcity" of physical time increases (Iutovich, et al.).

2.5 As perceived scarcity of physical time increases, perceived control of events in one's life decreases. This sensed loss of control eventually leads to anxiety, depression, feelings of role incompetence, and similar psychological symptoms of temporal panic (Bull; Cottle; Lyman and Scott; O'Rand and Ellis).

If, as we have suggested above, the continuing demand for temporal synchronicity in modern societies leads to an increase in the perceived scarcity of time, one should be able to apply the same principles of social stratification to social time as have been applied to other scarce resources. As the scarcity of a resource increases, its value increases, thus intensifying competition to acquire it. This competition for time assures that, given the basically fixed quantity of clock time available, any reallocation of clock time for one social time (self time, interaction time, organizational time) necessarily requires, as argued in Section II, adjustments of clock time allotted among other social times. The competition among social times is, therefore, a nearly zero-sum game. Because of this increased demand on clock time and its limited supply, the only solution to the issue has been the development of a *stratification* of social times. The relations of temporal stratification to temporal embeddedness and synchronicity, along with some of their social consequences, are given in the following propositions and corollaries:

#### PROPOSITION 3.

*Social times are stratified in the following hierarchy (from highest priority to lowest): cyclic time, institutional-organizational time, interaction time, self time.*

#### Corollaries:

3.1 Time scarcity is generally passed down the hierarchy of social times. [For example, perceived scarcity within organizational time is resolved by methods which increase the scarcity of interaction time (e.g., working overtime) which, by the same process, creates temporal pressures leading to greater scarcity of self time.]

3.2 The most effective way to create more self time is by *fiat* and escape, not by planning (Cohen and Taylor).

3.3 The greater the impersonalization of occupational roles, the less self



time is available in the work setting and the more rigid are the cycles which define the temporal structure of the occupation (Zerubavel).

3.4 [in conjunction with Propositions 2a, 2b, and their corollaries]. In a social system, as the extent of temporal embeddedness and its attendant disruptions of temporal synchronicity increase, the stratification of social times become more formalized and rigid (Iutovich, et al.).

3.5 Like other social stratified systems, the stratification of social times is supported by an ideology and by sanctions.

In the following section, we suggest possible fieldwork strategies for empirically testing our propositions and corollaries regarding the theoretical relations among temporal embeddedness, synchronicity, and stratification. This discussion indicates the empirical import of our model, and, consequently, areas where concrete research may contribute to further theoretical refinements of our typology and propositions/corollaries.

#### **IV. Research Implications of the Theory**

If, as our theory asserts, social times are embedded within each other in the ordinary course of everyday life, the ideal research sites for the study of social times are those social settings in which each of the types of social time is maximally distinct and observable in its operations. Formal organizations (e.g., schools, hospitals, military installations, etc.) offer such sites. It is not that organizational time constraints do not enter into social interactions in more informal settings such as weekend gatherings among friends. They certainly do. But the methodological problems of studying the relations among social times are far more complex in these informal settings, because different individuals have different configurations of organizational times shaping interaction time activities. In contrast, a single organizational time structure dominates interactions in formal organizations and its features are relatively visible to the researcher. Until theory and research in the sociology of time advance beyond their presently modest level, we should concentrate research efforts on the simplest cases first. Zerubavel's (c) study of the temporal organization of hospital life illustrates this advantage.

A second consideration in the selection of research sites for the empirical investigation of our propositions/corollaries would be the degree of recent disruptions of temporal synchronicity in the organization, inasmuch as some of our propositions and corollaries concern social time *dynamics*—processes governing changes in the organization of social times during periods of temporal strain or adjustment among different types of social time. By studying organizations undergoing significant changes in their temporal organization, we can better explore the processes by

*which embeddedness, synchronicity, and stratification break down and are reconstituted.*

Let us illustrate one such possible research setting in which elements of our theory could be tested effectively. A researcher could study an industrial manufacturing firm for a period of time before and after it has reorganized its factory. This would occur, for instance, if the company were diversifying its product line or revamping its production technology. New work schedules would emerge, new status-roles would be added, others would be redefined, and the older patterns of social time would no longer synchronize within the new organization of the factory. A before-after research design would assist in the identification of those structures of social time (of all types) of the older organization which conflict with those arising from the changes instituted in the organization. Regarding the measurement instruments of the study, some combination of survey questionnaires, structured interviews, and direct observation could be employed.

Shifting attention to the possible concrete tests of our propositions and corollaries in such a research setting and design, several testable hypotheses may be deduced. From Proposition 1 and Corollary 1.1, we would hypothesize that employees whose jobs have expanded as a result of the reorganization of the factory (i.e., they have more official duties, must interact with a larger number of individuals in a given span of clock time, etc.) will perceive a shorter distance between points of clock time than they did prior to the changes. The latter could be measured by a Likert scale composed of such items as: "When I look at my watch, it is often later than I would have expected." We would also hypothesize similar differences between managers and assembly-line workers.

From Proposition 2b, we hypothesize that, for those subunits of the factory which are directly interdependent with a larger number of other subunits, there will be a greater number of modifications of organizational timetables extending over a longer period of calendar time, and this increase will be exponential rather than linear. The researcher would have to engage in on-site fieldwork to determine the informal interdependencies that exist in addition to those officially recognized in bureaucratic hierarchies and rules. The number of modifications of official schedules and the total duration of timetable instability are more easily observable and quantifiable.

As synchronization pressures increase in the organization, the corollaries of Propositions 2a and 2b should become applicable. We know that synchronization pressure is increasing on a timetable when it must be coordinated with a growing number of other timetables, and, from Proposition 2b, we hypothesize that the degrees of freedom available for scheduling events which require the coordination of multiple timetables shrink drastically as more and more timetables must be taken into account. Corol-

lary 2.1 predicts that one way in which the organization adjusts to this pressure is by tightening the norms regulating social times. For example, the allowable coffee breaks or other time-outs may become fewer in number, shorter in duration, or subject to stricter scheduling. From Corollary 2.2, we would predict that even the lower levels of the organization (secretaries, plant workers, foremen, etc.) indirectly receive synchronization pressures as pressures grow at the upper levels. Corollary 2.3 can be checked by comparing individuals' daily appointment schedules before and after the organizational changes. Finally, the psychological correlates described in Corollaries 2.4 and 2.5 could be investigated by interviews or survey questionnaires.

From Proposition 3 and its corollaries, we hypothesize that these disruptions of organizational time have derivative effects on interaction time (both inside and outside of the organization) as well as on the self time of members of the organization. According to Corollary 3.1, we would expect definite changes in the form and substance of communications in the organization. As perceived scarcity of organizational time increases (measured by survey questions such as "It seems I hardly have enough time to get my work done every day" or "I seem to accomplish fewer of my daily work goals than I used to do"), individuals will engage in less interaction time activities while still on the job. The trend will be toward short, organizationally instrumental conversations. This variable can be measured by timing the length of telephone calls within the firm and by doing a content analysis of conversations and inter-office memos. Many other effects, ranging from more working lunches to less after-work drinking and socializing, can be expected as a result of the intensification of temporal stratification. At some point, absenteeism will increase as a result of stress-induced illness (Corollary 2.5) or the demands of self time for escape (Corollary 3.2). Last, but certainly not least, there will be an increased scarcity of outside time, leading to strain within members' families and alteration of friendship networks.

From this suggestion of a possible strategy for empirically testing propositions derived from our theory, it is evident that, because the theory incorporates multiple levels of social structure, it is pregnant with potential empirical interpretations. Moreover, these interpretations are open to investigation through survey research, interviewing, content analysis, participant observation and other standard sociological methodologies. Although we have not developed a completed research design, further refinements of this preliminary design should yield fruitful research opportunities.

## Conclusions

As the complexity of industrial societies grows through increasing rationalization of institutions, the temporal embeddedness of events in organizational, interactional, and personal time structures becomes more complex. Consequently, the synchronization of acts and actors within organizational timetables and individual biographies becomes far more problematic, and often can be resolved only by robbing clock time from some other form of social time. As a result, the stratification of social times becomes more pronounced, leading to social time conflict with ramifications throughout the whole structure of society, profoundly affecting the quality of life of its members. Hence, it is no accident that the development of a typology of social times and the specification of research implications of our theory reach into a wide range of specialties within sociology—a final reminder of the critical importance of social time to sociology and to society.

## Notes

1. Two significant exceptions to this generalization are Sorokin and Gurvitch.
2. From a sociology of knowledge standpoint, it can be argued that many sociologists have viewed time in a reified frame as if clock time and calendar time represent the only, or at least most relevant, temporal contexts. This limited conception of time is very much an artifact of living in an advanced industrial society wherein, indeed, mechanical/mathematical time frames predominate. It is only when we cease taking this orientation for granted and treat it as historically and culturally relative that the salience of social time emerges. Thus, we do not recognize the full importance of time as a sociological variable if, as is often the case in longitudinal studies, we regard it exclusively in terms of calendar time instead of social time (see Heinch).
3. The concept of "nearing nearness" offers another criterion by which one can differentiate the experiences of individuals. That is, not only do we all differ in the set of experiences we have had; there are also differences among most of our common experiences in terms of their temporal location to the present ("nearness"). Sociologists frequently collect data on life events (marital status, education, income, etc.) without regard for the temporal distance of these events in the self time of the individuals they are studying.
4. Although what happens while one is asleep is important for other psychological purposes, it does not occur while the person is, so to speak, in play in everyday life. Dreams, for example, are not structured according to the times we experience in the waking world. They require their own type of analysis (see O'Neill).
5. Evans-Pritchard's study of the Nuer society provides us a glimpse of what life would be without the need to fit social activities to clock time. The Nuer have a twelve-month calendar, but the months adjust themselves to the annual cycle of activities. In stark contrast to industrial society, this pastoral society lets "... activities regulate the calendar instead of the calendar regulating them" (201). The daily round for the Nuer is temporally controlled by the activities themselves rather than by any kind of mechanical time-reckoning. "The daily time-piece is the cattle-clock, the round of pastoral tasks and the time of day and the passage of time through a day are to a Nuer primarily the succession of these tasks and their relation to one another . . ." (207).
6. Members of modern society who are unable to regulate their daily lives according to the clock are judged to be unreliable, irresponsible, lazy, immoral, or simply inferior. This judgment has been made by puntan whites with reference to the time orientations of Latins, Blacks, and Indians (see Hall). Punctuality has become a modern virtue based on the importance of knowing and observing precise points in time, like meeting at "12:15 sharp."
7. The taken-for-granted power of normal rhythms of social time is manifest in situations in

which individuals lose control over and knowledge of the timing of events. Examples are prisons, mental institutions, concentration camps, and especially, torture. A standard, and quite effective, technique for extracting information from a prisoner is to schedule interrogations at unusual and random times so that the entire everyday rhythm and timing of the prisoner is disrupted, thus forcing him or her into a condition of anomic time wherein all temporal structures are broken. This procedure has been known to crack even the most hardened criminals into unrestrained confessions.

8. One might speculate that part of the identity crisis experienced by many in our society is related to the (perceived) unavailability of self time. As such, the identity crisis may be seen as a reaction against the lack of meaning in one's life fostered by the intrusion of other social times into self time. It is during moments of self time that one's unitary identity is established and nurtured (Mead, b).

9. The lack of opportunities for primary, noninstrumental conversation is most severe for housewives. In a twelve-nation study, Robinson et al. observed, "The normal housewife in our samples spends almost twice as many of her waking hours alone as her employed husband; and if one discounts children in order to speak of adult-level social contact, then the differential factor moves closer to three" (140-1). There is reason to suspect that the higher incidence of depression and other mental disorders among housewives in comparison to single women and working married women (Bernard) may be related to their relative deprivation of time for meaningful adult interaction. Workers, for example, can build in time for conversations as a source of satisfaction while still "on the job" (Schrack).

10. Another possible way to reduce the perceived strain between family time and organizational time is suggested by Melbin's (b) conception of "night as frontier." The supply of social time is not a totally fixed pie to be divided among activities; rather, the shape of the pie may be expanded by pushing back the frontiers of social time into the early morning hours. It is a biological fact that some people would be healthier if they took less sleep, but sleep time is socially, not biologically, determined (except at some limit which few approach). Nevertheless, it is known that people differ in the period of the 24-hour cycle in which they are at peak physical and mental efficiency. For married couples who are a mismatch in their respective orientations to "morningness" or "nightness," the incongruity of their circadian rhythms may have important implications for the satisfaction they derive from patterns of family interaction (Cromwell et al.).

11. With the coming of the so-called now generation, we may be seeing something of a revolution in social time (cf. Gioscia). In earlier times, when the pace of social change was comparatively slow, people were more willing and prepared to accept waiting through social time without rebellion. Increasingly, moderns are saying "now" instead of waiting for "then." Instant credit buying, for example, eliminates the wait for money already earned. When children of junior high school age and younger are experimenting with drugs and sex in large numbers and the "eternal time" (Eliade; Poulet) perspective of traditional Christianity is being challenged by new creeds, one must ask whether the "now generation" is not ushering in the dawn of a new era of social time no less dramatic than the difference between the social time of industrialized Americans and the pastoral Nuer.

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# Wealth and Power: A Comparison of Men and Women in the Property Elite\*

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## ABSTRACT

*This paper discusses the relationship between wealth and power, and examines the limits of this relationship through an analysis of sex differences in the distribution of wealth and power. The general hypothesis is that stratification by sex crosscuts property stratification, so providing a counterexample to the general relationship. In particular, there will be fewer women than men in the property elite; their sources of wealth will differ from men's; and, compared to men, they will have less control over their wealth. Support for these hypotheses is found using various methods, including estate multiplier techniques and case studies of members of the property elite.*

A popular assertion maintains that women control most of the wealth in the United States. Were this the case, it would contrast sharply with the large number of studies documenting the lesser returns to women in economic, educational, occupational, and political sectors (Bernard; Hochschild; Lipman-Blumen and Tickamyer; Millman and Kantor). But in fact there have been few studies of women's representation among the property and power elite to document the actual situation.<sup>1</sup> In examining the position of these women, this paper raises issues which have implications for understanding both the status of women and the relationship between property and power under industrial capitalism. Two questions will be posed. First, are women as disproportionately underrepresented among the extremely wealthy as in other categories. Second, are women who do have access to great wealth able to translate these resources into positions of power and prominence to the same degree as men? After reviewing the literature on property stratification, various documentary, biographical, and case study materials will be examined to investigate sex differences in

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the distribution of wealth and resources. Answers to these questions may provide further understanding of the factors at work in the complex relationship between wealth, power, and stratification by sex.

### **Background Research: Property Stratification and Sex Inequality**

There is ample evidence for the existence of a property elite<sup>2</sup> in the United States, distinguished from the rest of the population by virtue of ownership of a disproportionate share of private property (Lampman, a, b; Lenski; Smith; Smith and Franklin; Soltow; Turner and Starnes). Despite fluctuations in the concentration and distribution of wealth<sup>3</sup> over the course of this century, the overall picture is one of relative stability, characterized by a very small fraction of the population controlling a very large amount of the wealth.<sup>4</sup>

Although theorists and students of politics disagree about the exact role played by holders of extreme wealth,<sup>5</sup> there is general consensus that such individuals and families have opportunities to exert power and influence unshared by other segments of society. The special advantages enjoyed by the extremely wealthy have been well documented to include everything from an opulent standard of living to a special system of justice (Lenski; Lundberg; Mills).

A prime example is found in the ability of the property elite to influence tax legislation to its own financial advantage, despite the ostensibly progressive nature of the tax structure. Specific examples are the loopholes which permit the wealthy to pay only a small fraction of the taxes their incomes otherwise would require—the distinctions made between sources of income which favor the wealthy such as the definition of capital gains as only partially taxable; and the frequency with which special bills or provisions in the tax laws are made favorable to particular interests or influential individuals (Lenski; Stern; Turner and Starnes).

Of more direct concern in this paper is the extent to which extreme wealth provides access to positions of power in this society. Power is defined as the "social capacity to make binding decisions that have far-reaching consequences for a society" (Orum, 27). It is generally agreed that the top positions in business, industry, and government are locations of great potential power and that persons in these positions have ample opportunity to exercise power. The connection between these positions and wealth is frequently assumed and increasingly well-documented. A number of investigators have demonstrated a more than coincidental connection between wealth and these positions. Domhoff, Dye, Matthews, and Zeitlin all demonstrate that persons in these positions are more often than not from upper class, wealthy families. Useem shows that the inner group of American capitalists are both very wealthy and influential as measured by

positions on governing boards of community organizations, business associations, etc. Furthermore his results imply that wealth frequently precedes position rather than being a consequence of it. Most of these studies, however, approach the problem by identifying the positions of power and then searching for the proportions who come from various types of social backgrounds. If the issue is whether wealth produces power, then another tack needs to be taken as well. To what extent do persons with great wealth translate these resources into positions of power? In other words, the issue is not only to document the number of high positions held by the wealthy but also the number of wealthy holding high positions.

In general, it can be maintained that ownership of great wealth creates access to powerful positions in the social structure. There are sufficient exceptions to this proposition, however, to make it difficult to define satisfactorily the boundaries and conditions of this relationship. One example is suggested by Baltzell in his study of the "Protestant Establishment." He relates how Jews from wealthy families who have attended Ivy League colleges are not permitted access to either the highest corporate positions or the locations where many important decisions are made, the private clubs. Despite access to financial resources, they are excluded from positions leading to the exercise of power, presumably on the basis of lack of another valued resource—ethnicity. Although there has been some easing of these restrictions, a more recent study of prominent Jews fundamentally substantiates these findings (Zweigenhaft).

Smigel illustrates the same process in the realm of the elite Wall Street law firms. In some cases, listing in the Social Register is one criterion for a position in the firm. Such evidence of a cross-cutting caste system or dual stratification by ethnicity leads one to expect yet another example of the conditional nature of the relationship: women in the property elite.

Many theories of sex stratification hypothesize the same relationship between resources and sex inequality as the more general theories of stratification discussed previously: increased access to resources by women diminishes inequality and male domination (Collins a, b; Lenski). Perhaps the best developed of such resource theories is Collins' conflict theory of sex stratification. Collins postulates that differences in strength and sexual aggression initially give men an advantage over women resulting in male domination. Changes in social organization and technology create changes in the relative importance of other types of resources and, therefore, the importance of strength and aggression may diminish. In advanced market societies, as Collins characterizes contemporary Western industrial nations, education, employment opportunities, and financial resources become important determinants of position. Women's bargaining power should rise as their access to these resources increases.

Other theories of sex stratification imply a more complex relationship between resources and inequality, stressing the importance of institu-

tionalized patriarchal norms and structures (Firestone; Jagger and Struhl) or the historically specific material conditions for women's inequality residing in their special relationship to production (Benston; Mitchell; Rowbotham). Both of these positions recognize women's continuing subordination despite changes in social structure and differences in class position.

If control of resources unilaterally provides access to power (for women as well as everyone else), then there should be little differentiation by sex in the source, distribution, form, and use of great wealth. On the other hand, if sex is an overriding attribute; if patriarchal norms and structures take precedence; or if other aspects of women's class position prevail, then women should show significantly different patterns on these factors than men. In that situation, one would expect women's position among the wealthy to differ from men's and their ability to utilize great wealth and resources to be limited. On this assumption, a series of hypotheses can be formulated, developing the implications of this argument. These are:

1. There will be fewer women among the top wealth-holders than men.
2. The source of wealth for female top wealth-holders will differ from men, with women more likely to inherit great wealth or receive gifts and settlements. They are less likely to increase significantly their wealth by their own efforts.
3. The forms of wealth held by women will differ from men. In particular, women's wealth is more likely to be tied up in ways that they are unable to control, as in trusts and annuities. Lundberg points out that trust funds, a favorite means of keeping fortunes intact and reducing inheritance taxes, concentrate great financial power in the hands of trustees and executors, which are frequently banks. The beneficiaries gain income but control is largely out of their hands. If, indeed, more of women's wealth is tied up in such arrangements, it probably places power over the funds in male hands, given the nature of these organizations. This suggests that there are institutionalized limits on women's ability to control their own wealth.
4. The uses to which women put their wealth will differ from men. In particular, it is expected that men will be much more active in the arenas of power—i.e., on corporation boards, in government and policy positions, and in general business dealings, while women will participate less in these areas, but are likely to be more active in civic affairs. As suggested by Veblen, women will be found to have honorific functions commensurate with their sex and class positions.

These hypotheses are stated in logical order rather than the order of investigation. The remainder of the paper will present data to examine them.

## Methods

A major problem in the study of property stratification is difficulty in obtaining data, because of the lack of direct information on wealth holdings. Survey research cannot provide such data, because the number of people who are great wealth-holders is so small and because responses to questions on wealth and income are notoriously unreliable. Statistics published from individual internal revenue returns, available in aggregate, do not necessarily represent actual earnings on holdings since there are many ways of avoiding and reducing taxes, the number of loopholes and the efficiency of their use increasing with the degree of wealth (Lundberg). Therefore, much of the evidence produced in support of this study will necessarily be circumstantial, indirect, and suggestive rather than conclusive.

Two main approaches are taken. The first is to examine primary and secondary sources of aggregate data including Internal Revenue Service reports and statistics and studies by economists such as Lampman and Smith. In this approach, the major source of information on wealth comes from statistics published by the IRS on estate taxes. For the years under consideration in this study, all estates of decedents with gross holdings larger than \$60,000 were required to file a tax return within fifteen months of death. This information is published by the IRS along with their calculations of wealth based on these returns. The technique used is the estate multiplier method which entails multiplying the number of decedents in each of a number of age, sex, and marital status categories by the inverse of the mortality rate for the particular group to arrive at estimates of wealth for the living population in each of the demographic categories and estate size groups. The technique is subject to some sources of error, particularly in choice of mortality rate and filing date for estate valuation, both of which may affect final estimates. These and other problems have concerned economists making use of this method (Lampman, a; Smith); however, it is probably the best available source of information on wealth distribution.

The second method is a case study of the very wealthy using biographical materials journalistic reports, and similar information. In all cases, interest is centered on the wealthiest examples in order to provide a conservative test of the hypotheses. If substantial sex differences remain despite access to resources—i.e., controlling for wealth—then sex clearly is an overriding factor. It is only in combination, using a large variety of sources and data, that answers to the questions raised above can be suggested.<sup>6</sup>

For both approaches, most of the data are collected for the late 1960s and early 1970s. The choice of this time period is determined by the availability and comparability of information from the various sources. While use of other time periods might result in variations in the findings, the

general stability in the composition of extreme wealth suggests that these results would also hold. In a few instances, older information supplements the case study material where this is necessary for complete records.

Findings

AGGREGATE STUDY

Studies using estate multiplier techniques show that there are fewer wealthy women than men; they have less wealth; and it is distributed differently. The differences are smaller than expected, however. Table 1 presents data on the proportion of top wealth-holders by sex and the percentage of the total wealth of such persons controlled by each sex for our time periods for which information is available. (Note that top wealth is traditionally defined as gross estate of decedents equal to \$60,000 or more; Lampman, a).

Using a slightly different definition of top wealth, Smith shows that of persons with net worth (gross assets less indebtedness, rather than gross estate) over \$60,000 for 1969, women are 43 percent of these persons with 44 percent of total net worth, and men are 57 percent with 56 percent of total net worth. The discrepancy between Smith's calculations and the data in Table 1 is accounted for by two items: the greater proportion of men with life insurance as their major source of wealth, and their tendency to greater indebtedness. Life insurance policies usually have large face values but much smaller surrender value, implying that the size of the asset is much greater for beneficiaries than for the policy holder. Therefore, these should not be used to estimate living wealth. Smith's technique adjusts for this fact, and this explains the decline in the proportion of men relative to women. Also, if adjustments are made for debt levels, a similar effect is evident since, among Smith's "super rich," women's debts are 6.7 percent of their total assets and men's are 11.8 percent. Difference in debt levels is

Table 1. CHANGE IN WOMEN'S SHARE OF THE TOP WEALTH, 1922-72

Year	Women		Men	
	% of Total Top Wealth-Holders	% of Total Top Wealth	% of Total Top Wealth-Holders	% of Total Top Wealth
1922	25.0		75.0	
1953	33.0	40.0	67.0	60.0
1969	37.0	44.0	63.0	56.0
1972	39.0	44.0	61.0	56.0

Source: Lampman (a, Table 2); IRS (b, Table C); IRS (d, Table C).

interesting for another reason: it suggests greater business activity on the part of men.

In both 1953 and 1969, women had a larger mean estate size, but men had a larger median estate. This suggests that there were a few women at the very top skewing the distribution, and perhaps also a large number of men with relatively small estates (defined by the \$60,000 limit). This is consistent with the tendency noted above for large numbers of men to have life insurance policies but little else (Lampman, a).

Lampman (a) suggests a large proportion of the increasing representation of women among the top wealth-holders is due to population growth in states with community property laws, which mandate equal division between husband and wife of all property acquired after marriage and consequent lowering of the amount required to be declared for estate tax purposes. After the death of a husband (usually before the wife's death) women with no *a priori* legal claim to their husband's wealth, may acquire enormous resources.

Age and marital status breakdowns by sex also show interesting differences. There was a positive association between age and size of estate for men but not for women in both 1953 and 1969. Women also tend to be older than men. In 1953, the median age for women top wealth-holders was 56, compared to 52 for men. In 1969, 10 percent of the women were younger than 40, and 22 percent older than 70, compared to 20 percent and 12 percent for men (IRS, b; Lampman, a).

Figures for marital status by sex also show large differences and provide an explanation for the age differences. Table 2 shows that women are much more likely to be widowed than men as well as having a somewhat higher proportion of divorced and single persons. Presumably women are more likely to inherit their wealth from their husbands given their longer life expectancies and the tendency for men to marry younger women.

Of special interest is the form and distribution of the assets of mar-

**Table 2.** PERCENT MARITAL DISTRIBUTION OF WOMEN AND MEN WITH NET WORTH GREATER THAN \$60,000, 1969

	Women	Men	Both
Married	52	88	72
Widowed	32	5	17
Divorced	5	1	3
Single	11	6	8

Source: Smith (Table 5).

ried men and women. It was suggested earlier that if the subordinate status of women prevails when they have access to enormous resources, then there may be institutionalized mechanisms to account for the constraint. One obvious way is if women's property is more often tied up in trusts and other relatively inaccessible forms. It is also expected that the forms and distribution of men's assets will more clearly indicate business activity than women's either because of the restrictions suggested above or because of other barriers to women's active participation.

Using estate tax data, the findings are somewhat equivocal. This is because some types of income do not have to be reported on estate tax returns, such as equity in a pension or income rights in annuities and trusts (IRS, d). Since it is precisely this sort of wealth which is expected to be disproportionately held by women, there is a serious gap in the available information. Other differences do emerge, however. As shown by Tables 3 and 4, women tend to have more of their wealth in the "other" category which covers personal effects, jewelry, automobiles, etc., whereas men tend to have a higher proportion in real estate, noncorporate assets, and notes and mortgages (IRS, b, d). Again, these are indicators of busi-

**Table 3.** ASSET COMPOSITION BY SEX—EACH ASSET AS A PERCENT OF TOTAL FOR PERSONS WITH NET WORTH OF \$1,000,000 OR MORE, 1969 AND 1972

Asset	1962		1972	
	Women	Men	Women	Men
Real estate	7.1	10.0	11.3	13.2
Corporate stock	59.7	52.7	45.6	50.0
Cash	4.0	4.0	3.9	4.7
Noncorporate assets	1.2	11.1	1.4	6.9
Life insurance	.1	.7	.1	.6
Corporate-foreign bonds	.7	1.4	.6	1.5
State and local bonds	4.6	3.9	4.3	5.4
Savings bonds	.4	.2	.3	.2
Other federal bonds	4.5	2.8	6.9	3.0
Notes and mortgages	.9	2.7	1.9	3.4
Other assets	16.7	3.0	23.7	11.1

Source: IRS (b, Chart 6; d, Chart 5).



**Table 4.** COMPOSITION OF ASSETS AND DEBTS AS PERCENT OF TOTAL ASSETS, BROKEN DOWN BY SEX AND MARITAL STATUS, 1969 AND 1972

Asset	Single		Married		Widowed	
	Women	Men	Women	Men	Women	Men
<u>1969 Net Worth Greater than \$500,000</u>						
Real estate	5.9	12.9	11.8	13.5	5.5	12.5
Corporate stock	68.1	49.9	58.8	49.9	32.2	51.3
Bonds	9.3	10.4	8.8	7.2	7.2	8.9
Cash	5.5	9.0	4.7	5.3	4.6	7.4
Noncorporate assets notes and mortgages	1.7	9.6	3.8	13.7	1.7	8.3
Other assets	9.5	8.2	12.1	10.4	32.4	11.6
Debts (negative)	2.9	8.6	8.7	10.9	2.8	8.2
<u>1972 Net Worth Greater than \$5,000,000</u>						
Real estate	3.5	11.7	5.8	9.0	9.1	9.0
Corporate stock	81.6	40.0	35.0	56.5	42.1	53.1
Bonds	10.9	12.6	13.0	11.3	12.5	6.7
Cash	2.1	5.3	1.7	4.4	2.4	1.9
Noncorporate assets	.2	5.3	4.0	8.1	1.5	21.9
Other assets	1.6	25.0	40.0	10.7	32.4	7.6
Debts (negative)	.7	4.4	7.3	8.1	6.0	6.8

Source: IRS (b, Chart 9; d, Charts 3-9).

ness activity on the men's part and as such represent an important difference in their holdings and the use they are able to make of them.

Estate tax data can be augmented with data on individual incomes. Recalling that taxable income does not by any means represent total wealth (or income), it can still be seen that the sources of income for men and women in excess of \$1,000,000 (the highest category available) are quite different. Women have a much greater share from dividends, estates, and trusts; men have a larger share from the sale of capital assets. The former

**Table 5.** SOURCES OF TAXABLE INCOME OF \$1,000,000 AND MORE FOR INDIVIDUAL RETURNS BY SEX, 1969

	Women			Men		
	Num- ber	Amount	%	Num- ber	Amount	%
Total adjusted gross income	105	\$230,524,000		106	\$263,906,000	
Wages and Salary	25	3,880,000	1.7	88	14,522,000	5.2
Business/prof.	4	170,000	.1	16	4,694,000	1.7
Farm	1	1,000		3	180,000	.1
Partnership	13	3,843,000	1.6	16	4,500,000	1.6
Small business corporation	3	230,000	1.0	14	7,451,000	2.7
Sale of capital assets	75	104,723,000	45.4	98	168,116,000	60.4
Sale of Depr. property	6	30,000		10	50,000	
Sale of other property				3	97,000	
Dividends	102	102,558,000	43.7	99	60,735,000	21.8
Interest	97	3,168,000	1.4	99	13,248	4.8
Pensions, annuities	9	52,000		11	127,000	.1
Rents	20	177,000	.1	28	800,000	.3
Royalties	18	4,530,000	1.9	14	2,203,000	.8
Estates and trusts	33	11,171,000	4.8	16	1,430,000	.5
Total*		234,533,000	100.1**		278,153,000	100.0

\*Total from all sources differs from total adjusted gross income by not including deductions and adjustments included in final assessment of taxable income.

\*\*Figure differs from 100% because of rounding error.

Source: IRS (a, Tables 1.21, 1.22).

sources all imply a more passive use of wealth than the receipt of capital gains. Investments can remain unchanged for years and still provide dividends. Estates and trusts are frequently untouchable. Capital gains (and losses), on the other hand, requires active manipulation of wealth. This in part substantiates the hypothesis that women's property is more likely to be tied up in ways they cannot or do not control, and men have greater business activity related to wealth production (see Table 5).

From the aggregate data it can be seen that while there are differences in the numbers of wealthy men and women, these are not as large as predicted. Much more important are the differences in the forms of their wealth and, to a lesser extent, its sources. There is sufficient evidence to suggest that men have greater control over their wealth and are more active in its use. It remains to be seen whether they are also more successful in translating this wealth into positions of power. More information on this may be obtained by examining a sample of extremely wealthy individuals.

#### CASE STUDY

It was considered advisable to get a sample of individuals who are among the wealthiest since it is assumed that the greater the wealth, the greater the access to power. The only reasonable source of such data is that which *Fortune* (Louis, a) magazine publishes. In 1968, it listed 66 of the wealthiest individuals in America. Of these, 12 were women, or less than 20 percent. To supplement this number, another 6 names were added which were not on this list but were included in a list published in 1957 (Smith). Depending on the accuracy of the *Fortune* lists, the women chosen for the study constitute a population rather than a sample.<sup>7</sup> To select a comparison group of men, the man directly below each woman was chosen, except where another woman was next on the list, in which case the man above was selected. Also included as additional cases for comparison purposes were men from the same family as a woman. The end result was a sample of 20 males. All subjects were worth a minimum of \$100,000,000 by *Fortune's* estimates, and some held assets valued a great deal higher. A list of these persons is attached in the Appendix.

The procedure followed was to trace these individuals to find out as much as possible about their social background, positions they hold, source of wealth, etc., using the following sources: *Who's Who in America*; *Who's Who of American Women*; Dunn and Bradstreet (*Million Dollar Directory and Reference Book of Corporate Management*); Standard and Poor (*Register of Corporations, Directors and Executives*); *Who's Who in Finance and Industry*; and the *Reader's Guide*. Information was classified into basic demographic data such as age, education, number of citations, whether they stated an occupation, and the kinds of positions they are associated with. Three basic categories of position were used. These were business and corporate posi-

tions, defined as top management or member of the board of directors;<sup>8</sup> government positions, defined as high ranking appointed (cabinet members, heads of agencies, etc.) or elected officials; and civic-public service activities, including leadership and participation in cultural, charitable, educational, and other voluntary service and organizations.

Unfortunately, the extremely wealthy appear to be fairly reticent about their personal lives, and with the exception of a few who have attracted media interest, they manage to stay out of the limelight. A number of people, mostly women, were not listed in any source other than the original *Fortune* lists. Lack of listing, especially in the biographical sources as opposed to the business reports, does not necessarily imply lack of activity. Even being listed is not necessarily a good indicator of activity since entries in such sources as *Who's Who* depend on self-report, and it is obvious that many contributors do not list all relevant information. It is also conceivable that some of the individuals on the list have lost part of their wealth.<sup>9</sup>

These problems should not be considered insurmountable, and in some cases may be a source of interesting information in and of themselves. For example, comparison of the number and sources of listings for men and women, even if not indicative of absolute activity level, do provide information on their relative prominence in national and business arenas. Furthermore, the possibility of subject attrition was compensated for by searching older editions of the sources listed as well as recent volumes.

The findings of this study show striking differences between men and women. As seen in Table 6, men are overwhelmingly listed in these sources, with almost as many in the business directories as in the biographical sources. Only 6 of the 18 women are listed at all, and only one is found in a financial directory, indicating their lack of participation in business. Men are also much more likely to have a stated occupation than women. Furthermore, it appears that the women are older than the men which is consistent with the estate tax data, although it must be cautioned that these results may be an artifact of the missing data on women and the fact that 6 of the 18 were drawn from a much older list.

Table 6 also shows a breakdown of the positions held by these individuals. Although once again the presence of missing data requires cautious interpretation of results, it is here, especially, that this lack of data can be used to infer women's lack of prominent participation at the national level.<sup>10</sup> As expected, women hold few positions in businesses and corporations. Interestingly, however, despite the fact that these women were not listed in the corporate references, in a large number of cases their husbands were. Comparison of siblings and other family members who were also on the list shows that whereas women do not appear, the men do (the Rockefeller family exemplifies this). Women have no positions in government compared to 7 (35 %) of the men.

**Table 6.** CHARACTERISTICS OF 38 OF THE WEALTHIEST PERSONS IN THE U.S

	Women	%	Men	%
<u>Total</u>	18	100.0	20	100.0
<u>Listed in at least 1 source other than Fortune or Lundberg</u>	6	33.3	20	100.0
<u>Listed in business directory</u>	1	5.6	16	80.0
<u>No stated occupation</u>	14	77.8	0	.0
<u>Education</u>				
Less than B.A.	3	16.7	3	13.0
B.A.	0	0.0	9	45.0
More than B.A.	1	5.6	3	15.0
No information	14	77.7	5	25.0
Total		100.0		100.0
<u>Age in 1968</u>				
50 or younger	2	11.1	5	25.0
50-70	6	33.3	10	50.0
70+	5	27.8	3	15.0
No information	5	27.8	2	10.0
Total		100.0		100.0
Mean age	65.8		61.2	
<u>Positions in businesses and corporations**</u>				
0/unknown	14	77.8	0	0.0
1	0	0.0	1	5.0
2	1	5.6	4	20.0
3	2	11.1	5	25.0
4	0	0.0	0	0.0
5	1	5.6	1	5.0
6	0	0.0	3	15.0
7	0	0.0	0	0.0
8	0	0.0	2	10.0
9	0	0.0	1	5.0
10+	0	0.0	3	15.0
Total	18	100.1*	20	100.0
Mean no. of positions	7		4.6	
Range	0-5		1-15	
<u>Positions in government</u>				
0	18	100.0	13	65.0
1+	0	0.0	7	35.0
Range	0		0-7	
<u>Public service, civic, charity, etc. positions</u>				
0/unknown	13	72.2	5	25.0
1	0	0.0	0	0.0
2	0	0.0	0	0.0
3	0	0.0	0	0.0
4	1	5.6	2	10.0
5	0	0.0	2	10.0
6	1	5.6	3	15.0

Table 6. (Continued)

	Women	%	Men	%
7	0	0.0	1	5.0
8	1	5.6	0	0.0
9	0	0.0	2	10.0
10+	2	11.1	7	35.0
Total	0	100.1	0	100.0
Mean no. of positions	2.3		7.05	
Range	0-14		0-15	

\*Rounded numbers may not add up to 100%.

\*\*Multiple positions in the same organization are counted only once.

The most startling result is the category of public service, civic, and charity positions. It was hypothesized that in this realm women would be more active than men. Ostrander's study of upper class women in one community found strong support for the active role played by upper class women in community, civic, and charitable activities. On the contrary, in this study, women appear to have no greater participation than in any other realm and men continue to take a commanding lead.

One reason for this difference may be in sampling procedures. Using a snowball technique, Ostrander selected subjects partly on the basis of their participation in such activities. Similarly, her definition of upper class women encompasses a much broader range of persons. Another explanation is that this study, unlike Ostrander's, focuses on activity at the national level. If more information at the local level were available, and more generally, data for these women, it is possible that the picture would be somewhat altered. An explanation for the prominence of men in this category may be that it is partly the result of the way this item was coded. Included are positions such as trustee of educational institutions on the assumption that despite the policy implications of such positions, they are still volunteer work. Men are presumably more likely to fill such positions. Nevertheless, the overall impact of these results is to stress women's relative lack of prominence at national or prestigious local levels.

## Discussion

If the results of these investigations are compared to the hypotheses developed earlier, it is found that while there is general support, some modifications must be made, and the evidence for others is far from conclusive. It is indeed the case that there are fewer women among the top wealth-holders although this difference is smaller than predicted. Furthermore, their num-

bers and proportions are rising. This may be considered a trend parallel to the general rise in status for women in American society.

It is not clear that the source of women's wealth differs from men's, as Hypothesis 2 suggested. Many large wealth-holders, both male and female, have initially inherited much property, although many have also increased their holdings in their lifetimes. There is circumstantial evidence from the estate tax data that many women gain wealth from inheritance of their husband's property or from marriage when residing in a community property state, indicating greater upward mobility for women. Comparison of proportions of men and women who are rentiers, wealth-holders with no income-producing employment, shows that women outdistance men. Mills found that of persons with wealth greater than \$30,000,000 in 1900, 1925, and in the 1950s, there was a general increase in rentiers, and 70 percent of them were women. Furthermore, journalists' accounts of the new rich (people who have not inherited their wealth) for this time period, include no women in the ranks even though it is known that the number of wealthy women was and still is increasing (Lamott; Louis, a). Therefore, although no firm conclusion can be drawn, it appears that women are more likely to inherit their wealth or a greater proportion of it, and generally tend to be more passive in their dealings with it.

The third hypothesis is closely related to the second. There is definite evidence that the forms of women's holdings differ from men's, but it is not possible to show conclusively that women's assets are more tied up than men's. There is, however, strong evidence that men's wealth is more active, i.e., such indicators as debt levels and profits from capital gains suggest that men are more involved in business activities entailing manipulation of their property.

The last hypothesis attempts to link the wealth with indicators of power. It was suggested that wealthy women are less likely to be involved in business, corporation, or government affairs than wealthy men, although they would be prominent in civic and charity functions compatible with sex-typed expectations. It was found, however, that men were much more prominent in all areas, and women were essentially too obscure to even find data on their activities.

Women's lack of access to powerful positions can be further documented by reference to two additional sources. Hunter, in a reputational study of the top leadership of the United States in the 1950s, gathered a list of 500 persons recognized as wielding a great deal of power. The number of women on this list is negligible; those who are mentioned are relatively obscure and are frequently identified as civic leaders or as involved in charitable organizations in contrast to men's more dominant roles in government and business.

The second source is an additional investigation undertaken to clarify

**Table 7.** PERCENT OF WOMEN ON CORPORATION BOARDS OF *FORTUNE*'S 1976 LIST OF THE TOP 25 CORPORATIONS AND A COMPARISON SAMPLE

	Total Posi- tions	Mean Number	Men	%	Women	%	Unknown	%
Top 25	1,070	42.8	995	93.0	11	1.0	64	6.0
Sample 48	568	11.8	418	73.6	28	4.9	122	21.5

the picture of women's role in the elite of the corporate structure. The case study data show that almost all of the men listed are active in business and corporate affairs, but rarely are the women. To discover how prevalent a pattern this is, the proportion of women who are officers or on the board of directors of the top 25 corporations in the country (*Fortune*) was determined. To see to what degree this is a function of size, an additional sample was taken, consisting of the first and last entry on any page listing one of the largest twenty-five corporations in Dunn and Bradstreet's *Reference Book of Corporate Management*. As seen in Table 7, although there is a problem because of the indeterminacy of sex of some individuals (because of the use of initials rather than first name), it is still overwhelmingly clear that men dominate the corporate structure at all levels, although slightly less so for smaller companies.

While this is not new information to anyone aware of current affairs, it nevertheless provides a measure of the degree of women's exclusion from the centers of economic power.

## Conclusions

Despite some increase in women's representation among the property elite, they remain a minority of top wealth-holders and frequently have more limited resources. Even when they do have great resources, they are not utilized to the same advantage as by men; controlling for asset level, women are unable to exploit their wealth in the same way that men do. Among men and women of equally great wealth, women do not have access to the same positions of power that men have, and furthermore, there is presumptive evidence of structural barriers which impede their access. In general, women members of the property elite play a very different role in society than their male counterparts. Exactly what this is, and how it is perpetuated remains to be determined. It is clear, however, that simple ownership of resources does not explain women's lack of power and prominence, and explanations must be sought for these limits. It appears that control of wealth, rather than ownership alone, is the key factor in



access to power. Since ownership is a more complex issue than generally conceded, wealth is clearly not a sufficient condition for power. To understand all the facets of the relationship between wealth, power, sex, and other stratification variables, it will be necessary to pay greater attention to the interaction between property and sex stratification.

## Notes

1. An occasional comment is found in the sex inequality literature about the desirability of studying women separately by class (cf. Gimenez; Rapp), but, in general, this has not been done. For one major exception, see Ostrander's study of upper class women.
2. The term "elite" is used as a general designation to denote the very small segment of the population involved and does not refer to elitist theories such as those associated with Pareto, Mosca, and their more recent followers. In the present use, elite refers to a small segment of what is more generally referred to as the upper class.
3. Wealth is defined following Lampman (a) as all assets with market value. A claim to wealth indicates the right to the use of income from this wealth and/or control over assets.
4. The percent of personal sector total equity owned by the wealthiest 1 percent of the adult population has fluctuated between 19 and 36 percent for the years 1922 to 1969 (Lampman, a; Smith and Franklin). A detailed study of wealth inequality in Wisconsin between 1860 and 1963 also found very little change over time and much similarity to the entire U.S. for selected years (Soltow).
5. Power elite, ruling class, and Marxist theorists (e.g., Domhoff, a, b; Miliband; Mills) all agree on the central part played by wealth in maintaining the primacy of the upper class among the power structure. Even pluralists would not deny that wealth is one means of gaining access to positions of power (cf., Keller) although they downplay its importance.
6. It should be noted that in this and subsequent sections, no single definition of top wealth is adopted, because of the flexibility required by dependence on a variety of sources for scarce data. In the best of all possible worlds, it would be possible to set a single standard for membership in this category and then systematically sample the cases. But for all the reasons outlined, an exploratory study cannot be so rigid.
7. For this reason, no significance tests are made, since they are not appropriately applied to a population (Morrison and Henkel).
8. Multiple positions in the same organization are counted only once to avoid comparability problems between different organizations and different career patterns. This procedure also lessens the probability of serious errors of omission in accounting for the subjects' business activities.
9. Note that a number of these persons are now deceased. This is irrelevant to this study, since data were collected for the appropriate time period and not for the present.
10. The failure of these women to show up in public service activities may well be an artifact of the large amount of missing data. What is argued here, however, is not that these women are necessarily completely inactive, but rather, their activities are not sufficiently influential to show up in national affairs. At local levels these women may be more prominent. Presumably the men would also be more prominent locally and therefore maintain the gap between them.

## Appendix: Fortune 1957 and 1968 Listings of Extremely Wealthy Americans

### WOMEN

Mrs. Mellon Bruce (Ailsa Mellon)  
 Mrs. W. Ford Buhl II (Josephine Ford)  
 Mrs. W. Van Allen Clark (Edna McConnell)  
 Mrs. Horace Dodge, Sr.  
 Doris Duke  
 Mrs. Alfred I. DuPont (Bessie Gardner)

### MEN

Charles Allen, Jr.  
 Clarence Dillon  
 John T. Dorrance, Jr.  
 Irené DuPont  
 William DuPont, Jr.  
 Benson Ford

Mrs. Edsel Ford	William Clay Ford
Helen Clay Frick	Arthur A. Houghton, Jr.
Matilda Geddings Grey	Roy Arthur Hunt
Mrs. Frederick Guest (Amy Phipps)	James S. McDonnell, Jr.
Mrs. Jean Mauze (Abby Rockefeller)	William McKnight
Cordelia Scaife May	Richard King Mellon
Mrs. Chauncey McCormick (Marion Deering)	Paul Mellon
Mrs. Lester J. Norris (Delora F. Angell)	Samuel I. Newhouse
Mrs. Charles Payson (Joan Whitney)	John D. Rockefeller III
Mrs. Marjorie Merriweather Post	David Rockefeller
Mrs. Alan M. Scaife (Sarah Mellon)	Richard Mellon Scaife
Mrs. Arthur Hays Sulzberger (Iphigenie Ochs)	W. Clement Stone
	John Hay Whitney

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# Managerial Power and Tenure in the Large Corporation\*

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## ABSTRACT

*This research examines the relationship of managerial power, in terms of the stockholdings of both the chief executive officer and other directors, to the length of managerial tenure among 218 large industrial corporations. Analysis of covariance is employed to assess the effects of different control configurations on the length of tenure, controlling for the simultaneous effects of corporate size and performance. On the basis of this analysis, it is evident that significant stock ownership by the chief executive officer is associated with longer tenure; but significant stock ownership by other directors is not associated with shorter tenure. Also, there are no consistent relationships between the control configuration variables and either the age of the chief executive officer or his years of service with the corporation. Finally, the relationship between managerial power and tenure obtains even when the chief executive officer controls only a relatively small block of stock.*

Sociologists and other social scientists have demonstrated an abiding interest in the issue of managerial succession and tenure in organizations. Some researchers have focused primarily on the impact of managerial succession on subsequent organization performance (Allen et al; Eitzen and Yetman; Gamson and Scotch; Grusky, b). Conversely, other researchers have concentrated on the organization characteristics which affect the rate of managerial succession (Allen et al.; Gordon and Becker; Grusky, a; James and Soref); or, alternatively, the length of managerial tenure (Crain et al.; Kriesberg; McEachern; Salancik and Pfeffer). Implicit in most of these studies is the assumption that a change of organization leadership often leads to more fundamental changes in organization goals and operations (Perrow; Zald, c). Whatever the eventual consequences of managerial succession for the organization, the process of succession is important in its own right because it provides evidence concerning the internal politics of the organi-

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zation. As Zald observes, "at the very least the choosing of a successor has to reflect the power balance of the organization" (a, 53). Managerial succession within the large business corporation, for example, enables the researcher to assess the empirical adequacy of various theories concerning the locus of corporate control (Allen, a; Useem; Zeitlin, a, b).

It can be argued that the prior research on this problem has often proceeded, albeit implicitly, from an essentially functional theory of managerial succession and tenure in organizations. In his pioneer study of the rate of succession among senior management officials of large corporations, for example, Grusky (a) concludes that there is a direct relationship between organization size and the frequency of managerial succession. Grusky attributes this pattern, at least in part, to the fact that the bureaucratic structure typical of most large corporations serves to reduce the potentially disruptive consequences of managerial succession. Moreover, he suggests that some minimal rate of succession is functional for the organization because "bureaucracies require periodic succession at the top if they are to adapt adequately to their environment" (269). A replication of this study conducted by Gordon and Becker calls into question the magnitude and, therefore, the significance of the relationship between size and succession. However, neither study can be accepted as being entirely conclusive because, as Gordon and Becker admit, "it is possible that the relationship between size and succession does not become manifest within a short period" (216).

A similar theoretical perspective is employed in a recent study of the relationship between the tenure of chief executive officers of large corporations and both corporate size and performance (Crain et al.). In comparing the relative effects of both size and performance on managerial tenure, these investigators conclude that "the tenure of corporate presidents is positively related in a quite responsive way to profit variations" (1374). Once again, it can be argued that it is functional for corporations to dismiss managers associated with low profits and to retain managers associated with high profits, even if different chief executive officers account for only a small proportion of the variation in corporate profitability over time. Although explanations of managerial succession and tenure in terms of corporate size and performance demonstrate the utility, however limited, of functional theory, they ignore completely the issue of managerial power. This is an egregious shortcoming, especially in light of recent discussions by organization theorists of the central importance of power in organizations (Perrow; Zald, c). These theorists have postulated that the power of a chief executive officer depends largely on the relative distribution of expertise and resources among the members of the board of directors (Zald, b). Certainly the most important source of power for any director, including the chief executive officer, is the control of a significant block of the voting stock in that corporation (Gordon; Zald, b). These theoretical formulations

are important because they locate the issue of managerial succession and tenure squarely within the context of the more general issue of corporate control and managerial autonomy (Allen, a; Useem; Zeitlin, a, b).

The first rigorous analysis of the effect of corporate control on managerial tenure was conducted by McEachern as part of his study of the relationship between managerial control and performance among 96 large industrial corporations. Using analysis of variance, he found that chief executive officers who are the only principal stockholders among the directors of their firms have considerably longer tenures than other chief executive officers. However, McEachern found no significant difference in the average length of managerial tenure between those firms where there are no principal stockholders among the directors and those firms where some of the directors, other than the chief executive officer, are principal stockholders. In an extension of this study which relied largely on the same sample of corporations and the same corporate control classifications, Salancik and Pfeffer employed analysis of covariance to examine the interactions between the different types of corporate control and various measures of corporate performance with respect to the length of managerial tenure. They found some evidence that tenure was directly related to performance only among those firms where some of the directors, other than the chief executive officer, are principal stockholders. In general, these two studies suggest that managerial power not only affects managerial tenure but that it also mediates the relationship between corporate performance and managerial tenure.

Although both the McEachern and the Salancik and Pfeffer studies break new ground, both theoretically and methodologically, they share two limitations. First, the sample of corporations examined in each analysis is relatively small: 96 firms in the McEachern study and 84 firms in the Salancik and Pfeffer study. Moreover, the exact sampling procedures employed by McEachern to obtain 32 industrial firms for each of his corporate control classifications is ambiguous. Second, on purely theoretical grounds, it seems appropriate to distinguish between three forms of family control rather than the two proposed originally by McEachern. In particular, it is possible to differentiate between three distinct situations: the chief executive officer as the only principal stockholder among the directors, both the chief executive officer and other directors as principal stockholders, and directors other than the chief executive officer as the only principal stockholders. Although each of these configurations assumes some degree of family control over the corporation, they imply substantial differences in the power of the chief executive officer. As a result of the stratified sampling procedures employed by both the McEachern and the Salancik and Pfeffer studies, they inadvertently ignore those firms in which both the chief executive officer and other directors are principal stockholders.

Another recent study, conducted by James and Soref, investigates

profit constraints on managerial autonomy among 286 large industrial corporations in 1965. They examine the effects of different types of corporate control, as well as corporate size and performance, on the probability of involuntary succession of the chief executive officer. In general, they found that the probability of involuntary succession during a given year is affected by both the size of the firm and its profitability but not by its type of corporate control. Although the focus on involuntary succession represents a major methodological innovation, this study suffers from two critical limitations. First, James and Soref employed extant classifications of corporate control which failed to distinguish between those family controlled firms where the chief executive officer is a member of the controlling family and those where he is not a member of this family. Second, the focus on succession in a given year ignores the fact that powerful chief executive officers may be able to resist succession for several years.

It has generally been presumed that managerial tenure, measured as the length of time that a person serves in a leadership position, is inversely related to managerial succession, measured as the frequency of changes of leadership during a given time period. Of course, in a purely statistical sense, these two approaches necessarily yield the same results over a prolonged period of time. Unfortunately, most researchers examine the frequency of succession over only a relatively short time period. This methodological limitation is especially problematic inasmuch as a chief executive officer who is also the only principal stockholder among the directors may be able to forestall his own succession. Indeed, the study of managerial succession and tenure in organizations represents a special case of the more general issue of decisions versus nondecisions in the study of societal power (Bachrach and Baratz), since a chief executive officer may possess sufficient power to prevent the emergence of succession as a legitimate political issue within the organization. In the absence of time series data, it is probably preferable to focus on managerial tenure rather than succession whenever managerial power is an issue.

The present analysis proceeds from the assumption that the chief executive officer is normally the most powerful individual within any corporation (Gordon). However, some chief executive officers are more so than others. To be exact, it is the relative distribution of stock ownership between the chief executive officer and the other members of the board of directors which determines, in large part, his power within the corporation and, consequently, his length of managerial tenure. In the absence of any principal stockholders among the directors, as in the case of the typical management controlled firm, the chief executive officer is usually the dominant member of the board of directors. Furthermore, the inherent power of the chief executive officer is reinforced whenever he is the only principal stockholder among the directors. Conversely, the power of the chief executive officer is somewhat curtailed whenever there are other principal stock-

holders among the other directors. The chief executive officer is perhaps the least powerful whenever he is not a principal stockholder but one or more of the other directors are principal stockholders.

### Sample and Measures

In order to achieve some theoretical closure on this issue, this research examines the effect of managerial power, as revealed by different configurations of corporate control, on length of managerial tenure in the large corporation, controlling for the effects of corporate size and performance. A preliminary sample was constructed which contained the 250 largest industrial corporations as ranked by either their assets or sales in 1975. This sample purposely excluded public utilities, retailing chains, transportation companies, commercial banks, insurance companies, and financial services firms. It also excluded privately owned companies, agricultural cooperatives, and subsidiaries of foreign-owned corporations. Although the preliminary sample contained 284 corporations, 66 corporations were eventually deleted for various reasons: 22 firms because of missing data on one or more variables, 9 firms because of minority control by another firm in the sample, and 35 firms because of succession of the chief executive officer between 1975 and 1976. As a result, the final sample is comprised of 218 large industrial corporations whose chief executive officers have served in that capacity for at least one full year as of 1976. An examination of those corporations deleted from the sample indicates that they do not differ significantly in size, profitability, or type of corporate control, from those corporations retained for the analysis.

The dependent variable in this analysis, managerial tenure, is measured by the number of years that an individual has served as the chief executive officer of a given corporation as of 1976. It must be noted that this does not represent the final length of managerial tenure for most of these chief executive officers. Consequently, this analysis employs a somewhat truncated measure of the ultimate length of managerial tenure. However, since these chief executive officers began their managerial tenures at different points in time, the use of this measure does not introduce any systematic bias into the analysis. Two intervening variables in this analysis are the total number of years that the chief executive officer has been employed by the corporation and the age of the chief executive officer. In addition, this research employs two control variables: the size of the firm, as measured by its sales, and the performance of the firm, as measured by its return on investment, expressed as the ratio of net income to assets. In order to correct for minor annual fluctuations in these two control variables, they are based on two year averages for 1975 and 1976.

It is inherently difficult to measure directly the independent variable



of primary theoretical interest, the power of the chief executive officer relation to the other members of the board of directors. In this research each corporation is classified into one of four possible control configurations, based on the distribution of stock ownership among the directors including the chief executive officer. The four configurations are: *management control* (no director, including the chief executive officer, is a principal stockholder); *direct family control* (the chief executive officer is the only principal stockholder among the directors); *joint family control* (both the chief executive officer and other directors are principal stockholders); and *indirect family control* (the chief executive officer is not a principal stockholder but there are principal stockholders among the other directors). In this classificatory scheme, any director who either controls or represents a significant block of stock is considered to be a principal stockholder. Nevertheless, it is the family, defined as the members of a particular descent group and their affines, which constitutes the basic unit of corporate control (Zeitlin, a). For example, a corporation where several directors, including the chief executive officer, are principal stockholders is considered to be a case of direct family control, rather than joint family control, whenever all of these directors are members or representatives of the same family.

In this study, a significant block of stock is defined, at least initially as 5 percent of the voting stock in a corporation, even though this stock may be distributed among the members of a family group. Recent studies of corporate control have adopted 5 percent stock ownership, in conjunction with representation on the board of directors, as a minimum condition for family control (Burch; McEachern; Pedersen and Tabb). However, in order to contrast the effects of different degrees of corporate control, this research differentiates between *family control*, requiring at least 5 percent stock ownership plus representation on the board of directors, and *family influence*, requiring only 1 percent stock ownership plus representation on the board of directors. Information on the principal stockholders of each corporation is derived from reports filed with the Securities and Exchange Commission, articles published in major business periodicals, and previous studies of corporate control (Burch; Chevalier; Larner; Lundberg; Pedersen and Tabb; Villarejo).

## Results

The simplest and most direct method for determining the effect of managerial power on tenure is to compare the average length of tenure of corporations with each type of control configuration using analysis of variance. However, this approach ignores the simultaneous effects of corporate size and performance on managerial tenure. Consequently, it is necessary to employ analysis of covariance in order to assess the effect of each control

configuration on length of managerial tenure, while controlling for the effects of corporate size and performance. This technique yields adjusted effects which represent, in this case, the expected length of managerial tenure for chief executive officers of corporations with each type of control configuration, if all of the corporations were equal in size and performance. These comparisons, involving the length of managerial tenure as well as the number of years the chief executive officer has been employed by the firm and his age, are presented in Table 1 in the case of family control. These effects, both unadjusted and adjusted, are expressed as deviations from the grand mean on each of the three variables. For example, the chief executive officers of management controlled firms have an average tenure of 6.88 years, 1.11 years less than the overall average of 7.99 years, whereas the chief executive officers of directly family controlled firms have an average tenure of 17.43 years, 7.06 years more than the overall average.

It can be pointed out that there is a close correspondence between the unadjusted and adjusted effects presented in this table. This correspondence can be attributed to the fact there is only a modest relationship between these variables and the covariates, corporate size and perfor-

**Table 1.** EFFECTS OF TYPE OF CONTROL CONFIGURATION ON TENURE, YEARS OF SERVICE, AND AGE IN THE CASE OF CONTROL BY FAMILY

Type of Control Configuration	Length of Tenure	Years of Service	Age
Management control (N=142)	-1.11 (-1.06)	-0.45 (-0.50)	-0.10 (-0.11)
Direct family control (N=23)	7.06 (7.06)	3.75 (4.14)	-0.72 (-0.62)
Joint family control (N=10)	9.51 (9.34)	1.33 (0.79)	4.81 (4.65)
Indirect family control (N=43)	-2.34 (-2.44)	-0.82 (-0.73)	-0.41 (-0.40)
Mean	7.99	24.77	56.59
$R^2_{x.c}$	0.212	0.015	0.036
$R^2_{x.sp}$	0.004	0.062	0.018
$R^2_{x(c.sp)}$	0.208	0.022	0.033
Prob.	0.000	0.257	0.068

Note: Adjusted effects are given in parentheses.

mance. Table 1 indicates the proportion of the variance in each of these variables explained by each set of explanatory variables. For example, the control configuration variables explain 21.2 percent of the variance in managerial tenure whereas corporate size and performance explain less than one percent of the variance in this variable. Finally, the control configuration variables explain 20.8 percent of the variance in managerial tenure which cannot be explained by corporate size and performance. This increment in explained variance is statistically significant beyond the 0.001 probability level.

The results presented in Table 1 provide considerable empirical confirmation for the power theory of managerial tenure. It is clear, for example, that those chief executive officers who are also principal stockholders in their corporations have significantly longer tenures than those chief executive officers who are not principal stockholders. Chief executive officers of directly or jointly family controlled firms have much longer tenures than chief executive officers of management controlled firms or those indirectly family controlled. However, these same results suggest important modifications of the initial theoretical scheme. It appears that the chief executive officers of those corporations where there are principal stockholders among the other directors do not have consistently shorter tenures than the chief executive officers of those corporations where no other director is a principal stockholder. Although chief executive officers of indirectly family controlled firms have somewhat shorter tenures than chief executive officers of management controlled firms, chief executive officers of jointly family controlled firms do not have shorter tenures than chief executive officers of directly family controlled firms. In other words, while significant stock ownership by a chief executive officer is associated with longer tenure, the presence of other principal stockholders among the directors is not associated with shorter tenure.

The comparisons presented in Table 1 also indicate that there are no consistent differences in either the number of years that the chief executive officer has been employed by the corporation or his age with respect to the control configuration variables. Chief executive officers of jointly family controlled firms are somewhat older and chief executive officers of directly family controlled firms have been employed by their corporations somewhat longer than other chief executive officers in general. Furthermore, it must be noted that these three variables are highly correlated with one another. The correlation between length of tenure and years of service is 0.248, while the correlation between length of tenure and age is 0.408. Years of service and age, in conjunction with corporate size and performance, explain 18.9 percent of the variance in length of managerial tenure. However, an analysis of covariance indicates that the control configuration variables explain another 17.4 percent of the variance in managerial tenure which cannot be explained by these four control variables. In short, the re-

relationship between the control configuration variables and length of managerial tenure is only partially attributable to either the age of the chief executive officer and his years of service with the corporation or the size and performance of that corporation.

Given the effects of both the control configuration variables and corporate size and performance on length of managerial tenure, it is necessary to determine the extent of any interaction between these explanatory variables with respect to managerial tenure. A separate regression analysis with interaction terms for both corporate size and performance for each type of control configuration indicates that these interaction terms explain an additional 9.3 percent of the variance in managerial tenure. The presence of such significant interaction implies that the adjusted effects presented in Table 1 pertain only to those corporations that are average in size and performance. In other words, the effects of the control configuration variables on managerial tenure are different at different levels of corporate size and performance. The pattern of this interaction can be demonstrated simply by comparing the correlations between both corporate size and performance and length of managerial tenure for corporations with each type of control configuration. These correlations are presented in Table 2. It is apparent from these correlations and from an inspection of the interaction terms in the regression analysis that there is a direct relationship between both corporate size and performance and length of managerial tenure among directly and jointly family controlled firms. Among management controlled and indirectly family controlled firms, there is either an inverse or a negligible relationship between these same variables.

This pattern of interaction between corporate size and performance and the control configuration variables with respect to length of mana-

**Table 2.** CORRELATIONS BETWEEN COVARIATES AND MANAGERIAL TENURE BY TYPE OF CONTROL CONFIGURATION IN THE CASE OF CONTROL BY FAMILY

Type of Control Configuration	Corporate Size	Corporate Performance
Management control (N=142)	-0.159	-0.106
Direct family control (N=23)	0.326	0.324
Joint family control (N=10)	0.405	0.174
Indirect family control (N=43)	-0.221	0.126

**Table 3.** EFFECTS OF TYPE OF CONTROL CONFIGURATION ON TENURE, YEARS OF SERVICE AND AGE IN THE CASE OF INFLUENCE BY FAMILY

Type of Control Configuration	Length of Tenure	Years of Service	Age
Management control (N=83)	-2.75 (-2.68)	-0.59 (-0.97)	-0.17 (-0.21)
Direct family influence (N=40)	7.11 (7.16)	2.68 (3.13)	1.33 (1.48)
Joint family influence (N=13)	7.63 (7.52)	-1.31 (-1.16)	1.79 (1.74)
Indirect family influence (N=82)	-1.90 (-1.97)	-0.50 (-0.36)	-0.76 (-0.78)
Mean	7.99	24.77	56.59
$R^2_{x.c}$	0.319	0.014	0.023
$R^2_{x.sp}$	0.004	0.062	0.018
$R^2_{x(c.sp)}$	0.318	0.019	0.025
Prob.	0.000	0.003	0.134

Note: Adjusted effects are given in parentheses.

gerial tenure is not consistent with the initial theoretical position. To the extent that any interaction is present, a direct relationship between corporate size and performance and managerial tenure would be anticipated only among those firms where there are principal stockholders among the directors other than the chief executive officer. In order to assess the importance of this interaction for the results presented in Table 1, it is necessary to employ the Johnson–Neyman technique for heterogeneity of regression between groups (Huitema). The objective of this technique, in the present case, is to determine the range of corporate size and performance over which the effects of the control configuration variables on length of managerial tenure are statistically significant. A comparison of the regression equation for directly and jointly family controlled firms, on the one hand, and that for management controlled and indirectly family controlled firms, on the other, indicates that the differences in length of managerial tenure are significant over the entire range of corporate size and performance encountered in this sample. In other words, chief executive officers who are also principal stockholders not only have longer tenures than those

who are not principal stockholders in their firms but this disparity becomes even more pronounced among the larger and more profitable firms.

The results presented in Table 1 are based on control configuration variables defined in terms of family control rather than family influence. Similar comparisons, using 1 percent stock ownership instead of 5 percent stock ownership by a director as the criterion for classification, are presented in Table 3. Once again, those chief executive officers who are principal stockholders of their corporations have much longer tenures than those chief executive officers who are not principal stockholders. Also, the chief executive officers of those corporations where some of the other directors are principal stockholders do not have shorter tenures than the chief executive officers of those corporations where there are no principal stockholders among the other directors. In general, the results obtained from an analysis based on family influence parallel those obtained from an analysis based on family control. In short, it seems that even one percent stock ownership by a chief executive officer is sufficient to affect the length of his tenure. It must be pointed out that the interaction terms for both corporate size and performance for each type of control configuration, in the case of family influence, explain an additional 6.9 percent of the variance in managerial tenure. Moreover, the pattern of interaction is very similar to that presented in Table 2 for the case of family control.

## Conclusions

This analysis sheds new light on the effect of managerial power on tenure in the large corporation. It reveals some of the inherent limitations of a strict functional theory of managerial succession and tenure. The analysis also demonstrates the utility of a power theory of managerial succession and tenure derived from recent research on the locus of corporate control. For example, it is evident that significant stock ownership by a chief executive officer increases the length of his managerial tenure. However, this same analysis also suggests important modifications of the initial theory. It is apparent, for example, that significant stock ownership by other directors does not decrease the length of the managerial tenure of the chief executive officer. The chief executive officer of a large corporation is often the dominant member of the board of directors by virtue of his position of authority within the corporation. Moreover, the inherent power of the chief executive officer is reinforced whenever he is also a principal stockholder in the corporation. As a result, the chief executive officer who controls a significant block of stock may be able to forestall any attempts to limit his managerial tenure. Conversely, it is often difficult for other directors, even those who are principal stockholders, to limit the managerial tenure of the chief executive officer. Finally, it seems that this relationship

between managerial power and tenure obtains even when the chief executive officer controls only a relatively small block of stock.

Although this analysis clarifies the relationship between managerial power and tenure, it leaves unresolved several important theoretical issues. Despite the fact that managerial succession and tenure are inextricably related to one another, an analysis of managerial tenure inevitably ignores some of the most important temporal factors affecting managerial succession. The succession of a chief executive officer is an event that represents the culmination of a political process within the organization. Moreover, in such hierarchical organizations as large corporations, this political process is often covert (Zald and Berger). Additional research is needed on the general conditions and specific events that affect the probability of managerial succession at any given time. The present analysis suggests that one of the major factors affecting the probability of succession is the power of the chief executive officer as a principal stockholder in relation to other directors as principal stockholders. Despite the limitations of this analysis, it has obvious implications for the extant theories of corporate control and managerial autonomy (Allen, a, b; Zeitlin, a, b). It also suggests a series of issues relating the locus of corporate control to the position of managers and directors within the overall class structure (Useem).

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# Sociology's "One Law"\*

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## ABSTRACT

*Though accepted widely enough for nomination as sociology's one law, there has never been an adequate empirical test of Durkheim's proposition that Catholics have lower suicide rates than Protestants. As a result, the empirical foundation for one important application of Durkheim's assertion that egoism and suicide vary proportionately remains uncertain. We use cross-national, longitudinal data from twelve nations to test Durkheim's proposition. Our comparisons (1) use national, female, and male suicide rates; (2) are made both with and without control for nation's level of development; and (3) are for four different time periods between 1919 and 1972. Results largely refute Durkheim's proposition. However, application of Durkheim's entire theory of integration (egoism and altruism) rather than just his theory of egoism shows that the data may nonetheless be consistent with his theory of variation in suicide rates.*

After noting sociology's lack of scientific laws, Merton identifies Durkheim's generalization that Catholics have a lower suicide rate than Protestants as an approximation to such a law. More recently, LaCapra calls Durkheim's theory of suicide "perhaps the only significant lawlike statement in sociology" (147; see also Johnson). Whether in books on theory construction and logic of inquiry (e.g., Hage; Stinchcombe; Wallace; Winton), or methods (e.g., Cole; Riley; Selltitz et al.; Smith), or statistics (e.g., Loether and McTavish), or in introductory texts (e.g., Broom and Selznick; Demerath and Marwell; Goodman and Marx; Levin and Spates; Light and Keller; Wilson), sociologists continue to discuss Durkheim's generalization relating to differences in Catholic/Protestant suicide rates as if it were a scientific law. Presumably because of its widespread acceptance Merton

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did not bother to cite any supporting evidence or studies other than *Suicide* (Durkheim, b).

Nevertheless, sociologists *do* disagree about whether Durkheim validated his theory of suicide. Nisbet refers to Durkheim's "demonstration of the unvarying correlation between high incidence of suicide and social and moral disorganization" (8; see also Bierstedt). Cole concludes that "the data on suicide rates for various religious groups lend support to Durkheim's theory of egoistic suicide" (7), and Wilson states that Durkheim's data reveal "a consistent and continuing difference between Catholic and Protestant suicide rates (emphatically higher for the latter)" (168). After noting the lack of individual level data on religious suicides available to Durkheim, Simon (213) maintains that nevertheless "Durkheim did as well as he could at this too high level of aggregation, showing that suicide is more common in places dominated by particular religions" (213).

But other sociologists are skeptical about the validity of Durkheim's demonstration. In his own book on suicide, Halbwachs, a leading disciple of Durkheim, questions whether Durkheim's forceful argument or the facts persuade the reader and he concludes that "sometimes . . . the dialectic was more persuasive than the facts" (4). Gibbs and Martin argue that *Suicide's* most important proposition (suicide varies inversely with the degree of social integration) "is supported, not by its predictive power, but by his (Durkheim's) forceful argumentation" (b, 8). Maris asserts that Durkheim's "generalizations do not account for his data" (18). Smelser offers many criticisms of Durkheim's method (a, 103; see also 104-11): (1) "In some cases the poor quality of the data raises doubts as to the possible validity of" Durkheim's comparisons; (2) "In other cases the degree of concomitant variation" found by Durkheim "is so slight as to resist any conclusions whatsoever"; (3) Durkheim's "method of dealing with . . . third, possibly causal variables in interpreting concomitant variation between two variables . . . was inconsistent and sometimes cavalier, and his interpretations are weakened accordingly"; (4) Sometimes Durkheim inappropriately held that "an established correlation at one analytic level (for example, the aggregate social level)" was also "valid or meaningful at another level (for example, the psychological level)." Both the reliability and validity of the data used by Durkheim (and, indeed, *any* official suicide data) are challenged by Douglas, who also suggests that Durkheim bent both theory and data to effect an artificially close alignment (see also Baechler).

In spite of continuing debate over and analyses of Durkheim's theory of suicide, (e.g., Douglas; Johnson; Lukes; Poggi; Pope; Smelser, b) no contemporary sociologist has tested Durkheim's proposition that Protestants have higher suicide rates than Catholics. The most comprehensive, empirically based test of the proposition is Halbwach's chapter on "Suicide and Religion" in *The Causes of Suicide*. Halbwachs grants "that Protestants commit suicide more than do Catholics" (168); however, he concludes that

these religious differences in suicide rates are due to "the cohesion of tradition rather than . . . religious cohesion" (190) *per se*. Except for Halbwachs, after more than three-quarters of a century *Suicide* itself remains the best evidence for, and the best test of, Durkheim's law-like statement on religious differences in suicide. Recent reanalysis of Durkheim's data shows:

If some results suggest that Catholics have higher suicide rates, and others that Protestants have higher rates, then taken together the results provide no warrant for asserting that either group has the higher rates . . . Modern sociology has long believed that the social suicide rate is higher for Protestants than for Catholics . . . Insofar as this idea derives from *Suicide*, it is not empirically well-founded (Pope, 76–7).

The apparent discrepancy between Durkheim's proposition and his own evidence and the widespread acceptance of this proposition make it essential to move from unsubstantiated rhetorical exchanges to an intersubjective development of empirically founded ideas about the relationship between religion and suicide. Accordingly, we propose to test Durkheim's proposition that Catholics have a lower suicide rate than Protestants.

## Theory and Hypotheses

### THEORY

The power of Durkheim's argument derives from his apparent success in demonstrating that suicide differentials in various types of societies—religious, familial, political, primitive, and military—can be explained in terms of level of integration (b). This paper addresses one important empirical test of Durkheim's hypothesis about the relationship between integration and suicide. Durkheim called low integration egoism, high integration altruism, and identified both as causes of suicide. His theory asserts a V- or U-shaped relationship between suicide and integration. Suicide rates are lowest toward the mid-point of the integration continuum and increase in proportion to lower (egoism) or to higher (altruism) integration.

Durkheim's theoretical analysis of religious society compared the strength of the collective factor with that of the individual factor, because, in his zero-sum imagery, a determinate statement about the strength of one is simultaneously a statement about the strength of the other. He specifically focused on the strength of collective beliefs relative to the strength of free inquiry. The various manifestations of free inquiry are an outgrowth of the weakening of common beliefs, as well as an indicator of the extent to which this weakening has occurred. *Suicide* is emphatic about one key relationship: free inquiry develops to fill the void created by a *prior*

weakening of collective beliefs. It is the *effect*; the weakening of collective beliefs is the *cause*. Normally free inquiry does not develop if strong collective beliefs exist, but once developed, free inquiry may weaken collective beliefs still further.

#### HYPOTHESES

Durkheim believed that free inquiry was more highly developed among Protestants than Catholics. "The Catholic accepts his faith ready made" whereas "the Protestant is far more the author of his faith" (b, 158). The more highly developed free inquiry among Protestants reflects the fact that Protestant religious society is less integrated than Catholic religious society. Consequently:

1.0 Catholics have lower suicide rates than Protestants. And as a corollary:

1.1 Catholic countries have lower suicide rates than Protestant countries. Furthermore, because the relationship between religion and suicide is genuine, and not spurious:

1.2 Catholic countries have lower suicide rates than Protestant countries when other variables are controlled for.

Finally, because the relationship between religion and suicide applies to both sexes:

1.3 The female suicide rate in Catholic countries is lower than the female suicide rate in Protestant countries.

1.4 The male suicide rate in Catholic countries is lower than the male suicide rate in Protestant countries.

Durkheim's analysis of the relationship between religious integration and suicide puts him on the horns of a dilemma. On the one hand, his initial comparison of Protestant with Catholic countries (b, 152-3) was frustrated by his inability to control for country's level of development. On the other hand, his solution, which was to compare Protestants and Catholics of the same country or region (b, 154), suffered from lack of any controls beyond the rates being in the same geographic unit: sex, age, education, type of job—to name some of the more important variables possibly influencing the relationship between religion and suicide—are not controlled. We have found only limited information on Protestant and Catholic suicide rates within countries and virtually no information on the sex, age, and socioeconomic status of the suicides represented by those rates; but we do have sex-specific suicide rates over time for five Protestant and seven Catholic countries, and we can control for level of economic development of those countries. Therefore, we use aggregate level data.

We define a country as Catholic or Protestant if at least 90 percent of

the religiously affiliated are one religion or the other. In addition to comparing Protestant and Catholic countries, we also consider what happens to the relationships between religious affiliation and suicide over time. Many observers, Durkheim included—"the old gods are growing old or already dead, and others are not yet born" (c, 475)—identify secularization as a pervasive trend in the modern world. As religion becomes less important and its impact diminishes, religious differences in integration and consequently in suicide should diminish concomitantly. Hence:

## 2.0 Catholic-Protestant suicide rate differences decrease over time.

Having cited Morselli's averages of 190 and 58 suicides per million for European Protestant and Catholic countries, respectively, Durkheim observed. "In spite of undeniable similarities, the social environments of the inhabitants of these different countries are not identical. The civilizations of Spain and Portugal are far below that of Germany and this inferiority may conceivably be the reason for the lesser development of suicide which we have just mentioned" (b, 152-3). Durkheim's solution was to compare the suicide rates of Protestants and Catholics of the same country, and he does so for a limited number of countries. As pointed out above, this method contains the advantage of comparing Protestant and Catholic rates directly, but the disadvantage of precluding the use of important controls. In the spirit of Durkheim's initial country by country comparison, we control the contaminating effect of "inferiority" of civilization by combining three indicators into a development index. The indicators are: percent of population residing in cities of 50,000 or more—a measure of urbanization; telephones per capita—a measure of economic development; and secondary school enrollment per capita—a measure of social development.<sup>1</sup> We use this index to test the following corollary of 1.2.

### 1.2a Controlling for level of development, Catholic countries have lower suicide rates than Protestant countries.

In analyzing religious differences in suicide, Durkheim did not control for sex. We introduce this variable to test the following corollaries of 1.3 and 1.4:

### 1.3a Controlling for level of development, the female suicide rate in Catholic countries is lower than the female suicide rate in Protestant countries.

### 1.4a Controlling for level of development, the male suicide rate in Catholic countries is lower than the male suicide rate in Protestant countries.

Although we analyze national female and male rates (all are yearly rates per 100,000), we know of no empirical evidence that would allow us to measure female-male differences in religious integration throughout the period (1919-72). The available evidence goes back to 1936 and indi-

cates (Argyle and Beit-Hallahmi) that women are more integrated into the religious community than men. There is every reason to believe that this relationship holds back through 1919. Therefore:

3.0 The correlation between Catholic-Protestant religious affiliation and suicide rates is stronger for women than for men.

## Data and Method

### DATA

Besnard analyzes official suicide data to refute the arguments of Douglas and Baechler (see also Atkinson), who criticize official suicide data. We agree with Sainsbury and Barraclough's conclusion, again based on empirical assessment of official suicide data, that "national and international suicide statistics are sufficiently reliable to be of scientific value, although they need to be interpreted critically" (1252; see also Gibb's comparison of coroners' reports and official suicide statistics). As long as suicide statistics are reasonably reliable they should be used; the alternative is to forgo research based on official suicide rates which would preclude testing Durkheim's proposition that Catholics have lower suicide rates than Protestants.

Our analysis uses part of a larger cross-national data set on twenty-four industrialized nations for the period 1900-75. Data analysis on religion, suicide, and development for seven Catholic countries (Austria, Belgium, Republic of Ireland, Italy, Luxembourg, Portugal, and Spain) and five Protestant countries (Denmark, Finland, Iceland, Norway, and Sweden) covers the years 1919-39 and 1946-72. During this period the twelve countries have been religiously homogeneous as measured by the 90 percent criterion identified above. There is extensive overlap between these countries and the Western European nations on which *Suicide* (152-70) focuses.

Religious affiliation data are from the *Statesman's Year-Book* (Keltie and Epstein); suicide rates are from published sources (e.g., national statistical yearbooks; *World Health Organization*) and reports received through personal correspondence with national statistics offices; and development data are from Banks and from computer tapes updating these data. (Where Banks' telephone data were incomplete we supplemented them with data from the annual *The World's Telephones*, American Telephone and Telegraph.) (Ask authors for data sources not listed in references.)

## METHOD

We test our hypotheses (hypotheses 1.1 through 3.0) with analyses of covariance by comparison of unstandardized and standardized differences of suicide rate means.

Unavailability of data limits analysis to 1919–39 and 1946–72. Before 1919 suicide data are missing for both Iceland and Luxembourg. Also, for this period, development indicator data are sparse for many of the remaining countries. Furthermore, much necessary information is missing for the World War II period (1939–45) for all countries.

Our data set contains 576 cases (48 years  $\times$  12 countries) where national suicide rate is the dependent variable. This number triples to 1,728 when female and male suicide rate cases are included ( $576 \times 3$ ).<sup>2</sup> Missing data for both the dependent and independent variables create the need to (1) estimate missing data where there is sufficient information to make a reasonable estimate, or (2) reduce the number of observations. We used estimation procedures in 367 of the 400 cases which were missing data for at least one variable. 1,728 minus 33 (missing cases in which we could not use estimation procedures) gives 1,695 as our total number of cases. Appendices A and B identify all missing cases, describe estimation procedures, and specify when estimation procedures were used.

## Analysis

### UNADJUSTED DIFFERENCES IN SUICIDE RATES

Hypothesis 1.1 states that, on the average, Catholic countries will have lower suicide rates than Protestant countries. The country-specific suicide rate averages for the five Protestant and seven Catholic countries (Table 1) show that the Catholic rates are generally lower than the Protestant rates. For the national as well as female and male rate comparisons, both the Catholic means and medians are lower than the respective Protestant averages. This comparison of averages is instructive, but only additional analysis will enable adequate assessment of hypothesis 1.1 and determination of (1) the magnitude of the Protestant–Catholic differences; (2) the impact of development on the religion–suicide rate correlation (1.2a); (3) what the correlation between religion and suicide rate looks like over time (2.0); and (4) whether the correlation between Catholic–Protestant religious affiliation and suicide rates is stronger for women than for men (3.0).

To further assess hypothesis 1.1 we used all country- and year-specific rates to derive weighted Catholic and Protestant means (Table 2, Part A). Comparison of Catholic and Protestant weighted averages for all years of national as well as female and male suicide rates (Part A, Col-

**Table 1.** AVERAGE SUICIDE RATES BY COUNTRY AND TYPE OF SUICIDE RATE, 1919-72

	Rates (Per 100,000)						
	National		Females		Males		Sample Size
	Avg.	(St. Dev.)	Avg.	(St. Dev.)	Avg.	(St. Dev.)	
<b>Protestant</b>							
Denmark	19.5	(3.6)	12.6	(3.8)	26.7	(3.9)	48
Finland	18.6	(3.7)	7.1	(1.8)	31.1	(6.0)	48
Iceland	9.3	(3.3)	4.3	(1.8)	14.2	(5.3)	48
Norway	6.9	(0.9)	2.9	(0.7)	10.9	(1.3)	48
Sweden	17.0	(2.4)	8.0	(2.2)	26.3	(2.8)	48
Mean*	14.3	--	7.0	--	21.8	--	--
Median	17.0	--	7.1	--	26.3	--	--
<b>Catholic</b>							
Austria	28.0	(7.5)	17.5	(4.8)	39.6	(10.2)	47
Belgium	15.0	(1.5)	7.8	(1.1)	22.4	(2.8)	48
Ireland	2.7	(0.5)	1.2	(0.3)	4.2	(0.8)	48
Italy	7.1	(1.6)	3.9	(0.6)	10.5	(2.6)	48
Luxembourg	12.7	(3.6)	5.6	(2.2)	19.8	(6.6)	43
Portugal	9.0	(1.7)	4.2	(0.8)	14.2	(2.9)	44
Spain	5.3	(0.7)	2.6	(0.3)	8.2	(1.3)	47
Mean*	11.4	--	6.1	--	17.0	--	--
Median	9.0	--	4.2	--	14.2	--	--

\*Unweighted average of countries' averages.

umn 1) continues to support hypothesis 1.1: in each instance mean Catholic rates are lower than mean Protestant rates. The absolute differences are smallest for female rates, as would be expected given the smaller magnitude of female rates. However, contrary to hypothesis 3.0, the standardized correlation ( $\eta$ ) between religion and suicide rate for women (.09) is only half that for men (.22).

The religious affiliation-suicide rate correlation by time period (Part A, Columns 2-5) produces some unexpected results. Contrary to hypothesis 2.0, the religion-suicide rate correlations show an overall pattern of *increase* for all three suicide categories. Before World War II, the national and male religion-suicide correlations are negligible ( $\eta$ s are between  $-.02$  and  $+.05$ ). In contrast, after World War II the national and male correlations are moderate,  $+.36$  and  $+.40$  for the national and  $+.39$  and  $+.42$  for the male, showing that Protestant nations typically have higher national and male suicide rates than Catholic countries. Before World War II, Protestant female rates are *lower* than Catholic female rates ( $\eta$  a weak  $-.11$  for each time period) but, again, after the war Protestant rates are higher ( $\eta$ s  $+.24$  and  $+.29$ ). Pre-World War II data fail to support, and post-World War II data do support, hypothesis 1.1.



**Table 2.** UNADJUSTED AND ADJUSTED DIFFERENCES IN AVERAGE SUICIDE RATES BETWEEN PROTESTANT AND CATHOLIC COUNTRIES BY TYPE OF SUICIDE RATE, 1919-72

Categories Compared, Slope and Sample Size*	A: Unadjusted Rates (per 100,000)									
	All Years		1919-29		1930-39		1946-59		1960-72	
	Avg.	St.Dev.	Avg.	St.Dev.	Avg.	St.Dev.	Avg.	St.Dev.	Avg.	St.Dev.
<b>National</b>										
Prot. (P)	14.3	6.0	11.2	4.5	14.3	5.8	15.2	6.2	15.8	6.1
(Sample Size)	(240)		(55)		(50)		(70)		(65)	
Cath. (C)	11.4	8.5	11.2	8.7	14.6	11.7	10.3	6.5	10.2	6.7
(Sample Size)	(325)		(71)		(70)		(94)		(90)	
$\bar{P}-\bar{C}$	+2.9		+0.0		-0.3		+4.9		+5.6	
Eta	+.19		+.00		-.02		+.36		+.40	
<b>Females</b>										
Prot. (P)	7.0	4.1	4.8	2.0	6.2	3.0	7.9	4.8	8.5	4.3
(Sample Size)	(240)		(55)		(50)		(70)		(65)	
Cath. (C)	6.1	5.5	5.7	5.4	7.6	7.8	5.7	4.4	5.8	4.3
(Sample Size)	(325)		(71)		(70)		(94)		(90)	
$\bar{P}-\bar{C}$	+0.9		-0.9		-1.4		+2.2		+2.6	
Eta	+.09		-.11		-.11		+.24		+.29	
<b>Males</b>										
Prot. (P)	21.8	8.9	18.0	7.4	22.6	9.7	22.8	8.6	23.5	9.0
(Sample Size)	(240)		(55)		(50)		(70)		(65)	
Cath. (C)	16.9	12.1	16.9	12.6	21.9	16.1	15.2	9.2	14.9	9.4
(Sample Size)	(325)		(71)		(70)		(94)		(90)	
$\bar{P}-\bar{C}$	+4.9		+1.1		+0.7		+7.6		+8.6	
Eta	+.22		+.05		+.03		+.39		+.42	
<b>B: Adjusted Rates (per 100,000)**</b>										
			1919-29		1930-39		1946-59		1960-72	
<b>National</b>										
$\bar{P}-\bar{C}$			-2.2		-3.3		+2.3		+6.6	
Beta			-.15		-.17		+.17		+.46	
(Sample Size)			(126)		(120)		(164)		(155)	
<b>Females</b>										
$\bar{P}-\bar{C}$			-2.4		-4.1		-0.8		+3.2	
Beta			-.29		-.33		-.09		+.35	
(Sample Size)			(126)		(120)		(164)		(155)	
<b>Males</b>										
$\bar{P}-\bar{C}$			-1.7		-2.4		+5.6		+10.1	
Beta			-.08		-.08		+.29		+.49	
(Sample Size)			(126)		(120)		(164)		(155)	

\*Denotes the number of cases by country and year as described in the text. Each set of figures includes five Protestant and seven Catholic countries.

\*\*Each adjusted difference is based on Protestant and Catholic deviations from the grand mean, with development as covariate.

## ADJUSTED DIFFERENCES IN SUICIDE RATES

Table 3 (Column 1) provides mixed support for hypothesis 1.2a. The zero-order correlation between religion and suicide for the national comparisons in Table 2 (Part A, Column 1;  $\eta = +.19$ ) becomes a smaller but still non-negligible partial correlation ( $\beta = +.13$ ), whereas the male zero-order correlation ( $\eta = +.22$ ) is reduced less ( $\beta = +.19$ ). On the other hand, the female zero-order correlation which was positive and negligible ( $\eta = +.09$ ) becomes a negative, negligible partial ( $\beta = -.01$ ). National and male rates support hypothesis 1.2a; female rates do not. Furthermore, as was true of Table 2 (Part A, Column 1) results, Table 3 results remain inconsistent with hypothesis 3.0.

Controlling for the effect of time as well as development (Table 3, Column 2), the adjusted Protestant-Catholic differences differ even more from the unadjusted differences found in Table 2. The nearly moderate  $\eta$  of  $+.19$  becomes a negligible  $\beta$  of  $+.04$  for national rate comparisons; the female comparison changes from  $+.09$  to  $-.11$ ; and the male comparison is halved ( $\eta = +.22$ ,  $\beta = +.11$ ). Taken as a whole, the results in Column 2 (Table 3) do not support hypothesis 1.2a. The national rate com-

**Table 3.** ADJUSTED DIFFERENCES IN AVERAGE SUICIDE RATES BETWEEN PROTESTANT AND CATHOLIC COUNTRIES BY TYPE OF RATE, WITHOUT AND WITH TIME CONTROLS

Rates Per 100,000	No Time Control*	Time Control
<u>National</u>		
$\bar{P}-\bar{C}^{**}$	+2.0	+0.7
Beta	+.13	+.04
<u>Females</u>		
$\bar{P}-\bar{C}^{**}$	-0.1	-1.0
Beta	-.01	-.11
<u>Males</u>		
$\bar{P}-\bar{C}^{**}$	+4.3	+2.6
Beta	+.19	+.11

\*The time categories are: 1919-29, 1930-39, 1946-59, and 1960-72. In the second column of figures, time is entered as a main effect with the above four categories.

\*\*See Table 2, note\*\*.

Note: N=565 for each comparison (see Table 2, note\*).

parison produces a negligible beta (+.04) whereas the female rates show a negative beta equal in magnitude to the positive male beta.

Overall results from Table 3 do not sustain hypothesis 1.2a. When controlling for time and development only one (male rates) of the three Catholic-Protestant comparisons shows any appreciable positive correlation between Protestantism and higher suicide rates.

We specify time period in deriving adjusted average suicide rate differences between Catholic and Protestant countries in order to compare these averages to the corresponding unadjusted averages (Table 2). The general pattern of results may be summarized as follows:

	<i>Unadjusted</i>	<i>Adjusted</i>
Positive (non-negligible)	6	5
Negligible	4	3*
Negative (non-negligible)	2	4

\*Two of these betas are  $-.08$ , one  $-.09$ ; hence all three approach being negative non-negligible.

Hypothesis 1.1 receives less support from the adjusted than from the unadjusted averages which suggests that, indeed, development explains some of the relationship between religion and suicide.

The coefficients in Part B of Table 2 show that if level of development is held constant, the findings are modified with regard to the effects of time and sex. While the unadjusted national and male rate comparisons show negligible differences before World War II and moderate positive differences after (Table 2, Part A), the adjusted comparisons show negative differences before and positive differences after (Table 2, Part B). Although the unadjusted weak negative female correlations before World War II became moderately positive after the war, the adjusted prewar comparisons are moderately negative and only the 1960-72 comparison is moderately positive.

The results, particularly those in Table 2, generally contradict our hypotheses. First, contrary to Hypothesis 1.1, Catholic suicide rates are lower than Protestant rates only in the comparisons unadjusted for time or development (Table 1; Table 2, Part A, Column 1); for example, adjusted averages in Table 2 show more (7) comparisons where Protestant rates are *lower* than Catholic rates than the reverse (5). Part B of Table 2 directly contradicts hypothesis 2.0. Not only is there no trend toward weaker positive (or increasing negative) Protestant-Catholic differences but, in fact, the opposite occurs. For unadjusted averages (Table 2) there are no negative differences before World War II, whereas there are positive differences after. For adjusted averages the differences before World War II are all negative (two being negligible), whereas in the periods afterward five of six comparisons are positive. Hypothesis 1.2a receives mixed support from the findings. Table 1 and Part A of Table 2 (Column 1) show that Catholic countries have lower suicide rates than Protestant countries. Controlling

for level of development (and time) produces two distinct patterns, a pre- and post-World War II pattern. Before World War II all betas (Table 2, Part B) are negative showing that, controlling for level of development, Catholic countries have higher, not lower, suicide rates than Protestant countries. In contrast, for the post-World War II period (Table 2, Part B) with one exception (females from 1946–59) all betas are positive. Pre-World War II results contradict hypothesis 1.2a; post-World War II results sustain it. Ironically, the time period closest to that analyzed by Durkheim himself contradicts his hypothesis, that furthest from it supports it. Finally, contrary to hypothesis 3.0, results for women consistently show either less support for or greater evidence against Durkheim's general hypothesis about the relationship between religion and suicide.

## Discussion

Although our results seem to disconfirm Durkheim's theory, perhaps it would be premature to reject it. After all, Durkheim's theory of integration as developed in *Suicide* and throughout his work has long been regarded as one of sociology's most valuable theoretical resources. Additionally, post-World War II results sustain Durkheim's theory (with the exception of the negligible  $-.09$  beta for female suicide rates for the 1946–59 period in Table 2). Third, and overlapping with the immediately preceding reason, analysis of religious differences in suicide reveals a striking pattern that calls for explanation. Results for 1919–29 differ little from those for 1930–39, but when we compare the three most recent periods, 1930–39, 1946–59, and 1960–72 a pattern emerges (Table 2, Part B). For the national, female, and male groups, a comparison of correlations in a period with those immediately following it shows a change of at least  $.20$  in the direction of a greater tendency for Catholic countries to have lower suicide rates. This discovery prompts reconsideration of the implications of our findings.

The key to such reassessment lies in recalling that Durkheim's theory of integration postulates a V- or U-shaped relationship between integration and suicide. When applying his theory to modern society, Durkheim typically used only the egoism portion. Consequently, Durkheim (b, 152–216) argued that suicide and integration vary inversely in religious, familial, and political society. However, Durkheim's analysis of contemporary military society (b, 228–39) applies the theory of altruism that asserts a positive relationship between integration and suicide.

Durkheim's tendency to apply only the theory of egoism to suicide in modern society is problematic. For example, it may produce a discrepancy between his theoretical characterization of a group and his placement of that group on the integration scale. Without using the word altruistic, Durkheim (b, 160, 376) characterizes the Jews as an altruistic group; none-

theless he placed them on the egoism portion of the integration continuum. Let us assume, as *Suicide* (b, 155) perhaps implicitly acknowledges, declining levels of integration in Jewish religious society. Had Durkheim found declining Jewish rates, consistent with his portrayal of them as altruistic, he could have argued that these rates were falling precisely due to declining levels of altruism and that, therefore, Jewish suicide rates clearly confirmed his theory.

Durkheim's usual assumption that the theory of egoism is applicable to groups in modern society, though problematic, is highly convenient, because it means that he does not have to decide on which side of the egoism-altruism boundary a given group falls. While for groups falling clearly on one side or the other (toward the tops of the "V"), such placement is not difficult, placement of groups at the border (the bottom of the "V") is complex; yet such placement is crucial. For groups on the altruism side, falling integration should produce lower suicide rates (until the group crosses the line to the egoistic side, when one would predict higher rates). If the same group is on the egoism side, then falling integration should produce higher rates. Depending on where a group falls on the integration continuum, *opposite* empirical outcomes from the *same* cause (weakened integration) should occur.

Finally, if in practice Durkheim simply seemed to assume the applicability of the theory of egoism to groups in modern society, there is another possibility. Since Durkheim obviously knew the rates cited in *Suicide* before he wrote his explanations of them, it may be that he used his knowledge of the rates themselves to make his placements. For instance, Durkheim's theoretical accounts do not portray the modern military (b, 234, 236-38) as more altruistic than the Jews (b, 159-60, 167-68, 376). Among other things, he notes (b, 160, 234, 238, 376) the "primitive" social structure of both groups. Yet in a chapter on altruism (b, 217-40) he discussed the modern military (b, 228-40) and asserted a positive relationship between integration and suicide in military society. In contrast, he discussed the Jews (b, 155-57, 159-60, 167-70) in a chapter (b, 152-70) on egoistic society and asserted an inverse relationship between integration and suicide among them. If Durkheim did, in fact, use rates to place groups, this would explain both the different treatment accorded Jews and the modern military, and the related discrepancy between his theoretical description of the Jews as altruistic and his use of the theory of egoism to explain their suicide rates.

If we do not follow Durkheim in assuming that both Catholics and Protestants are more or less egoistic, but instead, consider use of both the theories of egoism and altruism, we immediately face attractive explanatory possibilities as well as the difficult task of placing Protestants and Catholics on the integration continuum. If available, an indicator of integration could be used to place each group on the integration continuum.

Unfortunately, we lack indicators that would permit precise placements.<sup>3</sup> Furthermore, such an indicator would not itself indicate the point at which egoism becomes altruism. That is, an integration indicator would not determine the precise location of the point of the "V" on the integration continuum. In diagramming Durkheim's theory the point of the "V" may simply be placed at the mid-point of the integration continuum. But only the most precise indicator of integration would make it possible to locate this midpoint and position groups with respect to it. An integration indicator is most useful for placing groups on the integration continuum relative to each other. However, relative placement of Protestants and Catholics is not the immediate problem.

Our placement of Protestants and Catholics along the integration continuum is made in accordance with the following assumptions. First, we accept Durkheim's argument that Catholic religious society is more integrated than Protestant. Second, we do not accept Durkheim's assumption that Catholics fall on the egoism portion of the integration continuum, but we agree with him that Catholics should fall toward the center of this continuum. Third, we agree with Durkheim that Protestants fall on the egoism side of the integration continuum, but we assume that initially (before World War II) they are near the center of the continuum. Finally, we assume a declining level of integration in both religious communities during this time period. While we have no direct measure of this change, correlations between time and suicide rate for each religion (Table 4) are consistent with this and the previous assumptions. For egoistic Protestant countries decreasing integration should produce increasing suicide rates and, in fact, does for national, female, and male rates, whether or not adjustment is made for level of development: all correlations between time and suicide rates are positive. For altruistic Catholic countries, however, decreasing integration should produce decreasing suicide rates and, in fact, does for all rates regardless of adjustment for development: all correlations between time and suicide rates are negative.

We use the adjusted correlations (betas) in Table 2 for the relative placement of Protestant and Catholic nations. For the pre-World War II period Catholics appear on the altruism portion of the integration continuum, somewhat higher than Protestants are on the egoism side (Figure 1, Part A). This is in accordance with the evidence that Catholics have higher suicide rates even though they are more integrated than Protestants (national betas are  $-.15$  and  $-.17$ ). Our hypothesis is that by 1946-59 both Catholic and Protestant religious societies became less integrated (Figure 1, Part B) to the point where Catholicism is closer to the center of the integration continuum than Protestantism.

In 1960-72 (Figure 1, Part C), the continued decreasing levels of integration produce greater discrepancies as the Catholic countries move toward and Protestant countries away from the center. These placements

**Table 4.** UNADJUSTED AND ADJUSTED EFFECTS OF TIME ON SUICIDE RATE BY RELIGION AND TYPE OF SUICIDE RATE, 1919-72

Rates (Per 100,000)	Unadjusted*	Adjusted
<u>Protestant Countries</u> (N=325)		
National	+.28	+.37
Females	+.36	+.22
Males	+.21	+.43
<u>Catholic Countries</u> (N=240)		
National	-.10	-.43
Females	-.03	-.39
Males	-.13	-.43

\*Multiple regression was used to produce the time coefficients listed. The unadjusted effect is the zero-order correlation between time and suicide rate as measured by  $r$ . The adjusted effect is the standardized coefficient for time (with development controlled) as measured by  $\beta$ .

explain why the negative  $\beta$  ( $-.17$ ) for national rates in Table 2 for 1930-39 becomes a positive  $\beta$  ( $+.17$ ) for 1946-59 and why this  $+.17$   $\beta$  becomes a larger  $+.46$   $\beta$  for 1960-72.

We argued that females are more integrated into religious society than males; consequently, the tendency for Catholics to have lower suicide rates should be stronger for women than for men (hypothesis 3.0). The  $\beta$ s for all periods in Table 2 disconfirm this hypothesis. The lines of interpretation just developed with respect to national rates, however, can be applied to female rates as well if we make the following two assumptions. First, we hypothesize that overall female and male suicide rates reflect a combination of egoistic and altruistic forces bearing on the various "societies" into which women and men are integrated. The net effect is for female rates to be lower than male rates. Second, we continue to assume that females are more integrated than males into religious society, producing a tendency for Catholic females to have higher suicide rates than Catholic males, all other things being equal (Figure 1).

On the basis of these assumptions, for the pre-World War II period, we place Protestant and Catholic females further toward the high end of the integration continuum than Protestants and Catholics generally (Fig. 1, Part A). As integration in both churches decreases over time, Protestant female rates increase and Catholic female rates decrease in line with the rest of the population. Consequently, the 1946-59  $\beta$  ( $-.09$ ) in Table 2, although still negative, is weaker than the pre-World War II  $\beta$ s (Figure 1,

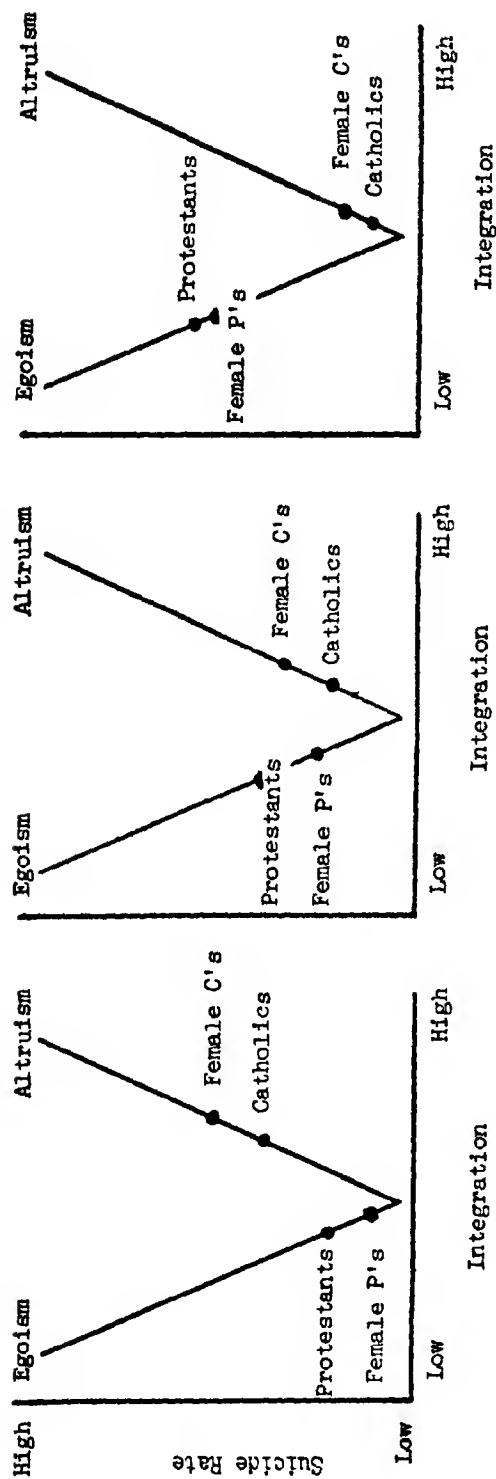


Figure 1. PROJECTED PROTESTANT AND CATHOLIC SUICIDE RATES BASED ON LEVELS OF RELIGIOUS INTEGRATION ALONE



Part B). And in the period 1960–72, the same integration trend produces relatively higher female rates ( $\beta = +.35$ ) in Protestant countries than in Catholic (Figure 1, Part C). And in each postwar period, females in both churches continue to be more integrated than males (Figure 1, Parts B and C). Therefore, as the level of integration in each church decreases over time, several things happen: the Protestant nations move toward increased egoism and the Catholic nations move toward decreased altruism. Within each religious group females follow the general pattern but remain relatively more integrated into religious society than males.

## Conclusions

*Suicide's* data are for the nineteenth century, particularly from about 1850–91. Recent assessment of the analysis and data indicate that these data do not demonstrate that Catholics have lower suicide rates than Protestants (Pope). Our analysis of twentieth-century data again fails to support the assertion that Catholics have lower suicide rates than Protestants. Specifically, pre-World War II data fail to confirm Durkheim's theory. Sociology's "one law" has never had adequate empirical grounding and this lack coupled with our pre-World War II results now permit us to reject this "law." Using Durkheim's entire theory of integration (and not just the theory of egoism), however, permits us to offer an interpretation consistent both with our results and with Durkheim's general theory of integration as applied to suicide.

If such interpretation expands the theory's explanatory possibilities, it simultaneously makes the theory harder to falsify and protects it from disconfirmatory evidence. Nevertheless, as represented in Figure 1, the statements about suicide rates derived from Durkheim's entire theory of integration are just as determinate as those derived from just one part of his theory, e.g., the theory of egoism or altruism. The problem, of course, lies in determining the boundary between altruism and egoism in order to state when integration varies proportionately (theory of altruism) and when inversely (theory of egoism) with suicide. Lacking means to determine this boundary, we have relied partly on the data themselves to place groups along the integration continuum. Durkheim placed Catholics on the egoism side of the egoism–altruism boundary. We placed them on the altruism side. But since these different placements imply no great difference in level of Catholic integration, and since no objective measure exists that would permit precise placement, we feel that our placement is as justifiable as Durkheim's, especially in light of the decreasing suicide rates in Catholic countries over time.

In principle, Durkheim's theory leads to clear hypotheses about the relationship between integration and suicide at any level of integration. In

practice, the difficulty of placing groups near it to one side or the other of the egoism–altruism boundary introduces great theoretical flexibility in post factum applications. Using this flexibility we have suggested that results which contradict one of the theory's hypotheses may in fact be consistent with the general theory. Our interpretation of Durkheim's theory enlarges the scope of the data needed to test adequately his theory of integration as applied to religious differences in suicide. Additional research is needed to test, not sociology's "one law" which never received adequate empirical support and which must now be rejected, but Durkheim's theory of integration as applied to suicide.

## Notes

1. Durkheim (b, 162ff ) identified learning through education as another manifestation of free inquiry. Because we wish to control for education and because percent secondary school enrollment conceptually and statistically correlates well with other measures of development, we combine these measures into an index. In the construction of the development index, principal factor analysis without iteration was used to gauge the strength of indicator correlation and to produce weighting for each variable. All indicator correlations were moderate (at least .45) and their weightings similar to one another (percent urban .36, telephones per capita .41, and secondary school enrollment per capita .42).
2. Were the data available for all or most of the "cases" we are analyzing, we would like to have used not only sex- but also age-specific suicide rates. Given the presumed high incidence of suicide among older people, it is possible that any suicide rate differences between Protestant and Catholic countries might be attributable to differences in the countries' age structures. While we cannot prove this hypothesis directly, some fragmentary evidence suggests that our findings probably would not be very different if we had controlled for age. For one thing recent evidence on a sample of Western industrialized countries shows that age contributes very little by itself to suicide rate variation (Danigelis and Pope). Additionally, on the one hand, evidence on the age structure of the twelve European countries in our study shows that the Catholic countries, on average, tend to have a higher percentage of high suicide risk populations (over-60 males and over-60 females) between 1919 and 1972 with the discrepancy increasing somewhat over time (Keyfitz and Fiegler; Mitchell). On the other hand, our comparison of Protestant and Catholic suicide rates will show that the unadjusted Protestant rates are generally higher in each of the four time periods examined (Table 2, Part A) and that the trend over time is for the Catholic rates to decrease (Table 4). Working without an age control is problematic, but we feel that the evidence in hand, incomplete and based on aggregate data as it is, nevertheless suggests the problem may not be a serious one.
3. Ever since its introduction in 1958, Gibbs and Martin's (a) measure of status integration has been the subject of lively debate (see Schalkwyk et al; and the references cited therein). Schalkwyk et al. identify methodological, practical, and theoretical problems in its use, and until the problems they identify are demonstrated to be less serious than they appear to be or until ways to overcome them are identified, we prefer not to base our test of Durkheim on this measure. In any case, its use would not specify the precise point along the integration continuum at which altruism becomes egoism (see discussion above).

## Appendix A. Estimation of Missing Data

### DEPENDENT VARIABLES

National suicide rates are available for all 48 years in eight countries, 47 years in two countries, 44 years in one country, and 43 years in the last country for a total of 565

rates. Unfortunately, there are many more gaps for male and female suicide rates, so that for each sex only 484 rates are available (see Appendix B).

Because of the linear interdependence of total female and male suicides, we estimated the missing female and male rates by using the national rates from the missing years and the ratios of female to national and male to national rates from those years where all three rates are known. First, for each of the 484 years where all three rates are known we calculated female suicide rate/national suicide rate and male suicide rate/national suicide rate. Second, for each country we averaged all the female/national ratios and all the male/national ratios in order to compare them with the country averages. Also for each country we then averaged the female/national and male/national ratios which immediately precede and follow each missing data year or sets of years. We decided to use the averages calculated by the latter method to estimate female and male rates, because, in many countries, there are definite patterned increases and decreases of ratios over time, suggesting that the closest temporal estimates (i.e., those based on the latter method's averages) would be more accurate. In any event, differences between the two sets of averages are not substantial. Finally, we multiplied national suicide rates for the missing years by the average female rate/national rate and male rate/national rate ratios from the nearest years to estimate the appropriate female and male suicide rates respectively. In this way, we have increased to 565 each the number of female suicide rates and the number of male suicide rates we analyze.

#### INDEPENDENT VARIABLES

There also are some gaps in the urbanization, telephone, and secondary school enrollment figures (61, 31, and 77 respectively) (Appendix B). In the case of the first and last variables, missing figures were estimated by using information available on percent population in cities over 100,000 and primary school enrollment respectively. Following the same procedure used to estimate missing female and male suicide rates, we calculated ratios between percent living in cities over 50,000 and percent living in cities over 100,000 and between secondary school enrollment and primary school enrollment. We then used these ratios to estimate percent living in cities over 50,000 and secondary school enrollment respectively where there are gaps for these variables but not for the cities over 100,000 and primary school indicators. In cases where information is missing for both pairs of indicators we estimated missing data with the nearest cities over 50,000 or secondary school enrollment figures from that country (the specific method is that used for telephone data problems and follows immediately). Missing telephone data are less frequent because we were able to supplement Banks with *The World's Telephones* (American Telephone and Telegraph) figures. In each case we used the nearest year's telephone figures to estimate the gaps. In any country where two years for which phone figures are known flank a year which contains no telephone information, we used the average to estimate the missing figures. When only the figure preceding or following a missing data year or set of years is known, that figure was used as the estimate for the missing year(s).

Altogether we were able to estimate variable values for all cases except those in which the national suicide rate was missing (11 years), making the total missing cases 33 ( $11 \times 3$ ) out of 1,728 and the number of valid cases 1,695 (1728-33).

#### A FINAL NOTE

For only two countries were there substantial amounts of missing data for both independent and dependent variables. For Iceland we had to estimate sex-specific

suicide rates for 30 years and development indicator rates for 21 years. Estimates were also made for 17 years of sex-specific suicide rates and 21 years of secondary school enrollment rates for Luxembourg (see Appendix B). We analyzed the data set both with and without the Iceland and Luxembourg data. In the text, the "with" results are presented. The results without Iceland and Luxembourg are quite consistent with the reported findings in Tables 1 through 3, the unadjusted comparisons in Table 4, and the adjusted comparisons in Table 4 for the Catholic countries. While the adjusted time effects for Protestant countries are positive with Iceland included and negative without Iceland, the magnitude of both the standardized and unstandardized time effects without Iceland are smaller for Protestant than for Catholic countries, suggesting that, even with Iceland excluded, Catholic and Protestant countries may be on different parts of the integration curve.

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Appendix B. MISSING DATA BY COUNTRY AND VARIABLE BEFORE ESTIMATION\*

	Variable									
	Suicide Rate							Total Missing Data**		
	For		For		Urbanization	Telephones	Secondary School Enrollment			
	Nat'l	Females	Males					By Nation	By Sex	
Austria	1	13	13	11	3	11	11	11	13	
Belgium	0	2	2	0	0	0	0	0	2	
Denmark	0	0	0	0	0	0	21	21	21	
Finland	0	3	3	0	0	0	0	0	3	
Iceland	0	30	30	21	21	21	21	21	30	
Ireland	0	3	3	3	3	3	3	3	3	
Italy	0	4	4	21	0	0	21	21	21	
Luxembourg	5	17	17	5	4	21	22	22	35	
Norway	0	3	3	0	0	0	0	0	3	
Portugal	4	6	6	0	0	0	4	4	6	
Spain	1	10	10	0	0	0	1	1	10	
Sweden	0	1	1	0	0	0	0	0	1	
Totals										
Missing	11	92	92	61	31	77	104	148		
Available	565	484	484	515	545	499	472	428		
(576 - Missing)										

\*For the nonwar years (1919-39, 1946-72) each country should have information for 48 years for each variable; the numbers in the table represent the number of years for which the information is unavailable.

\*\*The "By Nation" column lists the number of cases in which information is lacking either for national rate or for one of the development indicators; the "By Sex" column lists the number of cases in which information is lacking for the female (or male) rate or one of the development indicators.

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# Religious Trends Among College Students, 1948-79\*

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## ABSTRACT

*Identical surveys of Williams College men in 1948, 1967, 1974, and 1979 asked about religious beliefs and behavior. The trend from 1948 to 1967 was one of liberalization and disengagement from church life; from 1967 to 1974 there was little change; from 1974 to 1979 there was a slight shift toward conservative beliefs and greater church participation. Apparently a lowpoint in traditional religiosity occurred on many campuses in about 1972-75. The percentage of students reporting that they had once reacted against the beliefs taught them increased from 1948 to 1974 but not thereafter; the median age of the reaction fell two years from 1948 to 1974, but it did not change to 1979. The hypothesis that church rejection by some students is a part of a general rejection of institutions found little support.*

Of all population groups, none is more sensitive to shifts in the cultural climate than college students. Past research shows that college students shift in attitudes from decade to decade or from half-decade to half-decade much more than does the total adult population (Hoge, b). This greater volatility of students seems to result from both their greater independence from institutional commitments and their greater proximity to intellectual life. Yankelovich argues that college students are a kind of forerunner group for cultural innovation generally, and that the basic pattern of cultural change in America is diffusion from college students to other young people, then to the total population.

Thus college students are important to anyone attempting to understand religious change in America. A number of trend studies have been done on college students, spanning the past half-century. They have found a fall-rise-fall pattern in traditional religious commitment, with a high level

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in the middle 1920s, then a decline to the middle 1930s, then a rise to a peak in 1952–55, than a long decline through the 1960s (see Hoge, b). The trends in religion are one component in broader trends in values. Traditional religious commitment has been strongest in times of conservative values on political, social, and moral questions; it has been weakest during times of liberal attitudes and high levels of politicization.

Observers of the college scene have often been surprised by shifts in cultural climate. No one in the late 1950s predicted the campus unrest and cultural revolution of the 1960s. And no one in the 1960s predicted the swing to privatism and conservatism during the middle 1970s. The religious changes, especially, surprised student-watchers. Beginning in 1970 and 1971, new religious movements, both non-Western and fundamentalist Christian, appeared on college campuses (see Glock and Bellah; Wuthnow, b).

Attempts to construct general theories of changing cultural climate among college students have generally been unsuccessful (see Hoge, b; Wuthnow, a). The phenomenon is subject to short-range change, and it is as elusive for sociologists as political climate is for political scientists. Much research effort goes into measuring and interpreting decade-by-decade shifts.

Careful replication research is important, partly because the intense public attention given to particular movements such as the Unification Church or the People's Temple produces false impressions about the religiosity of the vast majority of youth today. Replication data depicting trends are available from several sources.

The American Council on Education has collected valuable data in its annual freshman survey, done in over 300 colleges since 1967. Most of the items are replications, producing good data on attitude changes over 12 years (see Astin et al., and earlier years). The trends can be summarized in three general statements: (1) Politically, the students shifted slightly to conservative views since 1970. They identify themselves as being more conservative in orientation, and they are less inclined to voice social criticism on such questions as taxing wealthy people or preserving the environment. (2) On moral questions such as sexual behavior there has been little change from the early 1970s until 1979. However, there is a decrease in students agreeing that "parents should be discouraged from having large families." (3) Personal commitments have become privatistic, with more concern for making money and obtaining personal recognition for one's work.

The A.C.E. surveys have only one item on religion—asking about religious preference. The percentage saying "none" declined from 14 in 1971 to 8 in 1979.

Brown and Condon surveyed undergraduates in a northwestern university annually from 1971 to 1975. They found a slight trend away



from formal church participation and toward greater acceptance of anti-religious activities during that time. They also found a decline in political activism and an increase in respect for authority. They concluded that "the year 1972 appeared to be the high point of students' disregard for authority" (332).

Strommen compared large samples of high school youth surveyed between 1970 and 1977. He found a decline in concern over such matters as injustice, pollution of land and water, and world hunger. Also he found an increase in religious participation and a heightened awareness of a personal God. (On other earlier studies see Hastings and Hoge, b.)

To summarize, available trend data indicate a shift toward conservative values and privatism since the early 1970s. Shifts in religious attitudes have in the past conformed to such overall changes in value orientation. The remainder of this paper reports on a series of replication studies between 1948 and 1979 on religious attitudes. They were done at Williams College, a high quality independent eastern liberal arts college of about 1,850 undergraduates. It was a men's college until 1970, when women were admitted. Its entrance standards are among the highest in the nation, and its students come from all regions. It draws its students from the same general pool as many other quality private colleges, and in surveys done in the late 1960s its students reported values and attitudes similar to those at Harvard and Dartmouth. Students in this type of college have been found to be more liberal in religion than students in state universities or church-related colleges (Goldsan et al.; Hoge, a).

## Methods

In April 1948 the senior author surveyed Williams College students, using a questionnaire on religion developed at Harvard (Allport et al.). From a random sample of undergraduates 205 questionnaires were returned for a completion rate of 96 percent (Hastings). In April 1967 the same questionnaire was pretested and found to be reliable for a replication study. It was used with a random sample stratified by college class. A total of 206 were returned for a completion rate of 93 percent (Hastings and Hoge, a). In March 1974 the questionnaire was again used with a random sample of undergraduate men, stratified by class, with 210 completions (91 %). Then in March 1979 the same questionnaire was again used with the same kind of sample, achieving 159 completions for an 88 percent rate.

In a replication study such as this, using identical items with equivalent samples of college students, changes in the results may be the result of (a) changes in the meanings and understandings of the questionnaire items, (b) changes in the effect of college experience on the students, (c) changes in the composition of the student body at the college, or (d) broader changes

in attitudes in the pool of middle class youth from which the college draws its students. The first of these we sought to avoid by eliminating items found troublesome in pretesting and by using multiple items with various wordings on the same topic. The second was assessed by comparing freshman-to-senior differences in each survey. In 1967, 1974, and 1979 we found no significant association between college class and a series of religious attitudes. Among these students, religious attitudes and commitments are brought to college, not produced or measurably altered by the college experience (on this also see Astin).

With these two sources of change eliminated, there remain possibilities (c) and (d) above. The strength of each must be estimated. For measuring (c), the impact of changes in the composition of the student body at Williams, we calculated adjusted responses using weighted 1979 data, weighted to reinstate the crucial background characteristics of the 1974 sample. The adjusted responses thus eliminate most of the effect of changing admissions patterns at Williams College and depict religious change as it seems to exist more generally among well-educated middle class youth. Adjusted responses were similarly computed in 1967 and 1974.

Earlier reports have depicted the changing characteristics of Williams undergraduates from 1948 to 1967 and from 1967 to 1974. The main change from 1948 to 1967 was a broadening of regional background, so that Williams students in 1967 came from all parts of the nation, not just predominantly New England and New York. The main change from 1967 to 1974 was a large increase in the number of black students enrolled.

The Director of Admissions summarized changes in recruitment and admissions between 1974 and 1979. In general, there was little change except for an increase in Hispanic students; by 1979 about 12 were in the undergraduate student body. The number of black students in the undergraduate student body was constant at about 120–130 from 1974 to 1979. The number of women increased, from 31 percent of the students in 1974 to 43 percent in 1979. SAT scores declined gradually between 1974 and 1979, as they have at all leading colleges. The socioeconomic status and regional origin of the student body remained quite constant.

Data on religious backgrounds of students are not tabulated by the Admissions Office, but our surveys indicated that the percentage from Catholic homes rose from 24 percent in 1974 to 28 percent in 1979, the percentage from Protestant and Jewish homes was stable, and the percentage giving their home religious influence as "ethical but not theological Christianity (e.g., humanism, ethical culture)" fell from 6 percent in 1974 to 1 percent in 1979.

The 1979 survey also found that the median age of the respondents was 20.3 years, compared with 20.1 in 1974. In 1979, 56 percent said their fathers had graduate or professional degrees, compared with 44 percent in 1974. Reported median family income was about \$35,000, up about 27 per-

cent since 1974. Sixty-three percent had attended public high school, compared with 55 percent in 1974. Fifty-two percent described their hometowns during their high school years as suburban (51% in 1974); 14 percent described them as cities (not suburbs) over 50,000 (19% in 1974); 34 percent described them as smaller towns or rural settings (31% in 1974).

In 1967 the adjusted responses indicated slightly less change in religious beliefs from 1948 to 1967 than depicted by the raw responses—about two percentage points less on the average. The adjusted responses in 1974 differed slightly from the raw responses, but the direction of the differences was not consistent, indicating that the raw responses were little influenced by changes in the student body (Hastings and Hoge, b).

In 1979 we computed weighted responses to reinstate the 1974 background characteristics, and hence control for changes in the composition of the student body from 1974 to 1979. The only noteworthy change we found was in the religious background of the students—especially the increase in those reared as Catholics and liberal Protestants, and the decrease in those reared in “ethical but not theological Christianity.” Hence we down-weighted the first two groups and upweighted the third.<sup>1</sup> The 1979 adjusted responses came out a bit less traditionally religious than the 1979 raw responses, though the pattern is erratic. We will discuss the 1979 adjusted responses alongside the 1979 raw responses.

The number of veterans in the student body in 1967, 1974, and 1979 was near zero, but in 1948 over half were veterans. For sake of comparability the veterans are excluded from our analysis (reducing the N to 92 in 1948). As noted, Williams College was a men’s college until 1970: after that, the percentage of women steadily increased. We doubt if this had much impact on the men’s religious attitudes, since recent research indicates that the impact of *all* college experience on religious attitudes is slight. For sake of comparability, the replication studies are of men only.

## Analyses and Results

### RELIGIOUS BACKGROUND AND PRACTICES

An item asked “To what degree has religion been an influence in your upbringing?” The percent saying “very marked” changed little from 1948 to 1974, but increased 4 percentage points (from 20 to 24%) between 1974 and 1979. The percent saying “none” was 0 in 1948, 2 in 1967, 5 in 1974, and 4 in 1979. Then three items asked about the character of this influence (if there was any), about present feelings of need for religious orientation or belief, and about present preference of traditions.

Table 1 summarizes the responses to the item on present choice of religious tradition and change between home religious upbringing and

**Table 1. PRESENT CHOICE OF RELIGIOUS TRADITION AND NET CHANGE FROM BACKGROUND TO PRESENT CHOICE (IN PERCENT)\***

	1948 (N=92)		1967 (N=205)		1974 (N=210)		1979 (N=159)	
	Choice	Change	Choice	Change	Choice	Change	Choice	Change
Roman	14	-1	9	-3	7	-17	16	-12
Anglo-Catholic, Eastern Orthodox	4	-3	**	-4	1	-1	1	-1
Protestant Christianity	38	-27	19	-38	10	-34	18	-25
Liberalized Protestantism	6	+1	6	+3	1	-1	3	-1
Ethical but not theological Christ'y	14	+10	19	+15	17	+11	10	+10
Some form of Judaism	1	-3	8	-7	8	-5	8	-6
Other	2	+2	10	+7	12	+8	8	+4
New type of religion needed	7		12		11		15	
None needed, doubtful need, or multiple responses***	14		16		33		22	
	100		99		100		101	

\*The percentage whose background is in each group is not shown here; it can be calculated by subtracting the amount of change from the percentage choosing each group. For example, in 1979, 28 percent reported a Catholic background;  $16 - (-12) = 28$ . Changes in present choice from 1948 to 1967, from 1967 to 1974, and from 1974 to 1979 are all significant at .05 chi-square. Due to rounding error, columns may not sum to 100%.

\*\*Less than 1/2%.

\*\*\*Multiple responses were coded separately only in 1967; the figures in 1948, 1974, and 1979 represent none needed or doubtful need.

present choice. The figure under "choice" is the percent saying that the tradition "satisfactorily meets your own religious needs." The figure under "change" is the difference between those reared in the tradition and those seeing it as satisfactory now. Most notable in Table 1 is the large decline in persons reared as Protestants who find that tradition satisfactory. In 1967, 1974, and 1979 over half of the students reared as Protestants no longer find Protestant Christianity satisfactory. The losses among Catholics and Jews are also great. The students tend to shift to "ethical but not theological Christianity" or "none," or they say that "a new type of religion is needed." The trend was away from Protestantism and Catholicism from 1948 to 1974, but it reversed by 1979, so that more students in 1979 found the main Christian churches satisfactory than did in 1974.

The figures in Table 1 show the net changes from background to preference. They include both the students who changed *away from* any home tradition and also other students who changed *to* that tradition. To separate these two kinds of movement we tabled home religious tradition with present preference, producing a measure of holding power (the percent of students reared in a tradition who choose it when in college). The holding power percentages for the Catholics were 73 in 1948, 65 in 1967, 30 in 1974, and 67 in 1979. For the Protestants they were 58 in 1948, 34 in 1967, 22 in 1974, and 49 in 1979. For the Jews they were 22 in 1948, 50 in 1967, 48 in 1974, and 71 in 1979 (in 1979, Catholics had N=26, Protestants had N=55, and Jews had N=17).

The overall pattern from home upbringing to college-age preference is a liberal shift. But it is less marked in 1979 than in 1967 and 1974: there is an indication that college students in 1979 were more receptive to traditional religions than they were in the late 1960s and early 1970s.

More information on present preferences is found in a later item asking "With what religious group are you most sympathetic intellectually?" The responses were Catholic, 20 percent; Protestant, 34 percent; Jewish, 15 percent; "other," 9 percent; and "none," 22 percent. By adding the last two categories we find that about 31 percent are not sympathetic with any group in the Judeo-Christian tradition. The majority of these persons have turned not to non-Western religions but to individualistic religious quests.

### *Religious Beliefs*

Table 2 shows four items about religious beliefs. On the first item, the percent saying that they feel a need for some religious orientation or belief dropped from 1948 to 1979. (The 1979 adjusted responses were 54, 32, and 14 percent, indicating an even greater drop since 1974.) Apparently a traditional religious orientation is seen as something optional by more and more college students.

The second item asks about the nature of the Deity. From 1948 to

Table 2. RELIGIOUS BELIEFS (IN PERCENT)

	1948 (N=92)	1967 (N=206)	1974 (N=207)	1979 (N=159)
<i>"Do you feel that you require some form of religious orientation or belief in order to achieve a fully mature philosophy of life?"</i>				
		*		
Yes	85	65	60	57
No	5	22	24	28
Doubtful	10	13	17	15
<i>"The nature of the Deity"***</i>				
		*		
Infinitely wise omnipotent Creator	29	11	12	15
Infinitely intelligent and friendly Being	27	24	23	27
Vast, impersonal spiritual source	13	13	12	14
I neither believe nor disbelieve in God	18	13	15	17
The only power is natural law	1	8	13	8
The universe is merely a machine	1	1	2	2
None of these alternatives	11	28	23	18
<i>"Immortality"***</i>				
		*		
Personal immortality	38	17	22	29
Reincarnation	3	2	1	3
Continued existence as part of a spiritual principle	9	11	15	15
Influence upon children and social institutions	23	39	30	27
I disbelieve in any of these senses	2	7	7	8
None of these alternatives	25	24	25	18
<i>"Organized religion"***</i>				
		*	*	
The church is the one sure and infallible foundation of civilized life	8	1	1	1
On the whole the church stands for the best in human life	56	41	30	41
There is certain doubt. Possibly the church may do some harm	12	19	20	20
The total influence may be on the whole harmful	3	11	20	11
Stronghold of much that is unwholesome and dangerous to human welfare	2	4	5	8
Insufficient familiarity	5	7	11	6
A different attitude	14	18	14	13

\*Differences between responses on this item were significant at .05  $\chi^2$  when comparing the two surveys on each side of the asterisk.

\*\*The responses are abbreviated here; for the full responses see Allport et al.

1967 there was a definite shift away from orthodox Christian views, but since 1967 the trend has slightly reversed. (The 1979 adjusted responses were 1 percentage point lower on each of the first three responses.) Both the raw and adjusted 1979 responses suggest a slight trend toward more orthodox beliefs, but the trend is too small to be statistically significant.

The third item asks about immortality, and again we see a slight trend toward more conservative views in 1979 (though not significant at .05). (The 1979 adjusted responses were 27, 3, 15, 25, 8, and 22%—slightly less conservative than the raw responses.)

The fourth item asks about organized religion. Attitudes became more unfavorable between 1948 and 1974, but since 1974 they have become a bit more favorable (though not significant at .05). (The 1979 adjusted responses were 2 percentage points lower on the combined first two responses.)

Two additional items provide further information. One asked about views of the Person of Christ. In 1948, 38 percent believed that Christ is the human incarnation of God; in 1967 it was 20 percent ( $p < .05$ ), in 1974 it was 19 percent, and in 1979, 30 percent ( $p < .05$ ). (The 1979 adjusted figure was 29%— $p < .05$  from 1974.) Again we see a trend toward more conservative beliefs from 1974 to 1979.

The other item was about the church. It stated that denominational distinctions, at least within Protestant Christianity, are out of date and may as well be eliminated quickly. In 1948, 60 percent agreed; in 1967, 49 percent agreed; in 1974, 38 percent agreed; and in 1979, 42 percent agreed (46% when adjusted; not significant at .05). The percent disagreeing changed little. Perhaps more important is the increase in "no opinion" responses. It increased from 17 percent in 1948 to 24 in 1967, to 38 in 1974, and to 39 in 1979 (37 when adjusted) ( $p < .05$  from 1948 to 1979). Students seem to be disengaging more and more from ecclesiastical issues.

Some observers have suggested that among youth, religion is becoming very much individualized, so that church participation is seen as less important. Since 1967 our questionnaire asked, "Do you feel that church attendance and other types of religious behavior are an integral part of your religious orientation?" The percent saying yes changed little between 1967 and 1979—it was 24 in 1967, 19 in 1974, and 25 in 1979 (23 when adjusted). Apparently church participation is still seen as being as important as it was a decade ago.

We conclude that the decline in orthodox beliefs which took place during the 1960s has stopped, and some first indications are visible that a conservative trend began in the middle 1970s.

### *Religion and Science*

An item asked about the much-discussed conflict between religion and science (see Table 3). The overall trend was a shift toward seeing greater conflict between 1948 and 1967, but little change from 1967 to 1979. (The 1979 adjusted responses were 4 percentage points higher in the first two responses.)

Those students who checked the fourth or fifth responses were asked to indicate whether scientific or religious formulations must give way to the other. In 1967, 91 percent said that religion must give way; in 1974, again 91 percent said this; in 1979, 97 percent said so.

In 1974 and 1979 we looked at the correlates of views on this item and found that the persons with the most orthodox responses on other

**Table 3.** "HOW DO YOU FEEL ABOUT THE FREQUENTLY MENTIONED CONFLICT BETWEEN THE FINDINGS OF SCIENCE AND THE PRINCIPAL (BASIC) CONTENTIONS OF RELIGION?" (IN PERCENT)\*

	1948 (N=87)	1967 (N=198)	1974 (N=205)	1979 (N=158)
Religion and science clearly support one another	38	16	12	15
The conflict is negligible (more apparent than real)	32	35	40	36
The conflict is considerable, but probably not irreconcilable	21	21	19	20
The conflict is very considerable, perhaps irreconcilable	7	16	23	19
The conflict is definitely irreconcilable	2	12	7	10
	100	100	100	100

\*The difference between 1948 and 1967 was significant at .05 by  $\chi^2$ .

The difference between 1967 and 1974 and between 1974 and 1979 was not significant at .05.

belief items tended to see no conflict between religion and science. It is interesting that today the persons seeing no conflict between religion and science are the most orthodox, whereas in the nineteenth century, the most orthodox tended to be those who saw a conflict and wished to discredit modern science. Today the religious liberals argue that science must prevail over religious views, and the religious conservatives argue that science and religion do not conflict when properly understood.

### *Devotional Practices*

Table 4 shows two items asking about religious devotional practices. Frequency of prayer declined from 1948 to 1974, then increased slightly (but not significantly) to 1979. (The 1979 adjusted responses were 3 percentage points lower on the first two responses.) Feelings of reverence and devotion decreased from 1948 to 1967, then changed little until 1974 and increased slightly to 1979. (The 1979 adjusted responses were 2 percentage points lower on the first two responses.)<sup>2</sup>

### *Religious Development*

Three items on religious development are shown in Table 5. The percent of students who have at some time reacted against the religious beliefs taught them increased from 1948 to 1974, then changed little to 1979. (The 1979 adjusted figures on the first item were 73, 17, and 10%.) The median age when the doubts started took a similar pattern—it fell from 1948 to 1974, then changed little. (The adjusted 1979 median age was 14.5.) It seems that the more students who reacted at any point in time, the younger was the median age of reaction; apparently cultural influences in this regard have more impact on younger adolescents than older adolescents.



**Table 4.** TWO ITEMS ON DEVOTIONAL PRACTICES DURING THE PAST SIX MONTHS (IN PERCENT)\*

	1948 (N=92)	1967 (N=206)	1974 (N=210)	1979 (N=159)
Prayed				
Daily	19	9	9	13
Fairly frequently	20	15	7	10
Occasionally	23	18	17	17
Rarely	23	23	31	25
Never	15	35	36	36
Experienced feeling of reverence, devotion, or dependence upon a Supreme Being				
Daily	12	3	7	10
Frequently	15	16	11	17
Occasionally	26	23	27	20
Rarely	22	23	24	13
Never	24	35	32	41

\*On both items the differences between 1948 and 1967 were significant at .05 by  $\chi^2$ , and the differences between 1967 and 1974 were not significant. On the second item the difference between 1974 and 1979 was significant at .05 by  $\chi^2$ .

The last item in Table 5 shows that of the students who reacted against their religious training, most are again in partial agreement with the beliefs taught. (The 1979 adjusted figures were 18, 66, and 16%.) The more who have reacted, at any point in time, the fewer say they are now in substantial agreement again with the beliefs taught them.

After the 1974 survey we speculated that accelerated maturation during adolescence might be causing the movement to younger median

**Table 5.** REACTION TO HOME RELIGIOUS TEACHINGS (IN PERCENT)\*

	1948 (N=88)	1967 (N=199)	1974 (N=195)	1979 (N=150)
<i>"If you were brought up under some religious influence, has there been a period in which you have reacted either partially or wholly against the beliefs taught?"</i>				
Yes	57	72	79	71
No	27	19	14	17
Doubtful	16	9	7	11
(If so:) <i>"When did the doubts start?"</i>				
Median age	16.4	15.5	14.4	14.8
(If so:) <i>"Would you say that at the present time you:"</i>				
Are in substantial agreement with the beliefs taught.	24	15	10	20
Are in partial agreement with them.	70	67	70	63
Wholly disagree with them	6	18	21	17

\*On the first and third items the 1948-67 difference was significant at .05 by  $\chi^2$ . No 1967-74 or 1974-79 differences were significant at .05.

ages of religious rebellion. However, in 1979 the median age rose, greatly weakening such an argument and causing us to look mainly at shifts in the cultural climate to understand changes in age of rebellion.

#### TEST OF THE INSTITUTIONAL REJECTION HYPOTHESIS

Several observers of religion among college students have suggested that the decline in institutional religious participation during the 1960s and early 1970s was a part of a more general rejection of institutions. In this view, many youth rejecting the church were doing so as part of a more general rejection of establishment institutions, including government, politics, business, and others. For example, Wuthnow and Glock, in a study of Berkeley students, concluded that persons who have rejected the organized church

are least likely to value conventional career rewards, such as job security, a high income, and a chance to advance; are least likely to think that a businessman's career would give what they want in life; and are most likely to adhere to such countercultural goals as changing society, not having a long-range career, and living in a rural area (177).

The 1974 and 1979 surveys provide an opportunity to test the hypothesis among Williams' students. The questionnaire included a series of items asking opinions about the country's top political leaders, business leaders, education leaders, and religious leaders. Respondents were asked if a list of descriptive phrases was true of each. As a measure of general affirmation, we constructed an *Affirmation Index* for each set of leaders, using two of the descriptive phrases: "high moral calibre" and "sincerely interested in solving social problems." In both 1974 and 1979, religious leaders were rated highest, education leaders second, political leaders third, and business leaders lowest.

To test the institutional rejection hypothesis we correlated each of the three affirmation indices (for political leaders, business leaders, and education leaders) with three measures of church commitment. The first measure was the item shown at the bottom of Table 2, asking for a global evaluation, either positive or negative, of organized religion; the first five responses were scored from 5 to 1, and the other responses were scored as missing data. The second measure was an item asking about church (or synagogue) attendance during the past six months; the five responses were scored "about once a week" = "about every other week" = 2; "on an average once a month" = 3; "once or twice only" = 4; and "not at all" = 5. The third measure was the Affirmation Index for Religious Leaders; each of the two responses was scored 1 if marked, 0 if not, and the index score is the sum, ranging from 0 to 2. The correlations are shown in Table 6.

Correlations between views of other institutional leaders and (a)

Table 6. CORRELATIONS OF CHURCH COMMITMENT WITH AFFIRMATION OF OTHER LEADERS

	Affirmation of Political Leaders	Affirmation of Business Leaders	Affirmation of Education Leaders
Attitude toward organized religion (5=positive, 1=negative)			
1974	-.08	-.08	-.01
1979	.13	.11	.02
Church attendance			
1974	.06	.05	.07
1979	.14*	-.01	-.03
Affirmation of religious leaders			
1974	.25*	.11	.52*
1979	.32*	.05	.38*

\*Significant at .05. In 1974, N varies from 156 to 207; in 1979, N varies from 127 to 159.

attitude toward organized religion, and (b) church attendance, are very weak in both 1974 and 1979. The only noteworthy correlations are those between affirmation of religious leaders and affirmation of political leaders, and those between affirmation of religious leaders and affirmation of education leaders. If the institutional rejection hypothesis is tested using direct measures of affirmation of organized religion or church participation, no support is found. Only in the less direct test, that of relating affirmation of religious leaders with affirmation of other institutional leaders, is there support. Most strongly associated with views about religious leaders are views about education leaders. In general, these data offer little support for the institutional rejection hypothesis. Acceptance or rejection of the institutional church seems but weakly associated with views about other established institutions. Possibly the cultural polarization found at Berkeley was not as intense in eastern colleges such as Williams.<sup>3</sup>

Discussion

The trend in religious attitudes and behavior of Williams College students was one of liberalization and disengagement between 1948 and 1967, little change between 1967 and 1974, and a slight return between 1974 and 1979. We must be tentative about depicting the changes from 1974 to 1979, since changes on many items were not statistically significant. This overall trend since the 1940s agrees with other research.

The adjusted responses in 1979 are the best estimate of attitude changes since 1974 in other colleges similar to Williams. All research shows a conservatizing trend on college campuses in the late 1970s; new 1979

replication studies done by the second author at Dartmouth College and the University of Michigan agree with the Williams replication in finding more conservative religious attitudes than in 1974. Other high quality colleges in the East and Midwest undoubtedly have the same trend.

Based on all available evidence, we would guess that the lowpoint in traditional religiosity at many colleges was about 1972–75. Before that, religious trends were away from orthodoxy for a long time, ever since a highpoint in about 1952–55 (Hoge, b, 188). Observers who speak of a "return of the 1950s" on campus are greatly overstating the situation, since social, political, and religious attitudes at the end of the 1970s, though moving in the direction of the 1950s, still have a long way to go. Also, attitudes about personal freedoms do not seem to be moving toward the conformity and other-direction of the 1950s.

As evidence accumulates about college students' religious orientations and about adolescents' rebellion against religious training, researchers should begin to construct theories explaining the patterns of these life experiences. Already we can see that they change from decade to decade depending on the broader cultural climate. More research should be directed to analysis of changes in cultural climate.

## Notes

1. A technical discussion of the weighting of 1967 and 1974 is given in Hastings and Hoge (b). In 1967 we weighted the data to reinstate the characteristics of the 1948 sample, and in 1974 we weighted the data to reinstate the characteristics of the 1967 sample. In 1967 we weighted only on home religious tradition; in 1974 we weighted on both home religious tradition and major course. In 1974 we weighted only on home religious tradition, since changes in major course were not great between 1974 and 1979. The weights used were .85 for those reared as Catholics, .45 for those reared as liberal Protestants, and 9.5 for those reared as ethical but not theological Christians. These weights reinstate the home religious traditions of the 1974 sample without changing the overall N. The strong weighting for the last group unfortunately produces a slightly erratic pattern of differences between raw and adjusted responses. Yet the overall picture shows that changes in home religious tradition from the 1974 sample to the 1979 sample caused a small portion of the apparent shift toward conservative religious beliefs.
2. We would like to have trend data on church attendance, and our questionnaire had an item asking about it. However, in 1948 Williams College had required chapel attendance which inflated the figures, and in 1967 a typing error destroyed the item. Between 1974 and 1979, when we do have good data, the percent of students who reported church attendance monthly or oftener rose 8 percentage points (6 when adjusted).
3. A similar test of the institutional rejection hypothesis was done using 1975 NORC data (nationwide sample of adults 18 and over). Three measures of religious attitudes (belief in life after death, frequency of church attendance, and an item asking "Would you call yourself a strong \_\_\_\_\_ (whatever the preference)?") were correlated with questions about confidence in business leaders, leaders of banks, education leaders, the federal government, labor leaders, the press, medicine, television, the Supreme Court, the scientific community, Congress, and military leaders. All correlations were weaker than  $\pm .15$  (see Hoge, c, 101).

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# *Explaining Fertility Differences Among* **U.S. Protestants\***

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## ABSTRACT

*The hypothesis that Protestant fertility patterns result from differences in social characteristics among denominations is set against the hypothesis that doctrinal differences lead members of conservative denominations to have both higher wanted and unwanted fertility. Data for white couples from the 1965 National Fertility Survey support the doctrine hypothesis for wanted fertility among conservative Protestants low in formal participation. It is suggested that couples who follow conservative doctrine and have larger families participate less frequently because of conflicting demands associated with larger families. Further analysis with a measure of participation less sensitive to differences in family size produces results consistent with this argument.*

The preoccupation of American demography with comparisons of Catholic, Jewish, and Protestant fertility has overshadowed the study of variation within these groups. Probably because Protestants are the largest group and their fertility levels fall between those of Catholics and Jews (e.g., Westoff and Ryder), within-group investigations have principally described fertility variations within the latter two groups in order to explain why they deviate from the apparent Protestant norm (see Goldscheider; Potvin et al.). In many ways, however, Protestants are the most heterogeneous of the three (Glock and Stark). Not surprisingly, variations in fertility among Protestant denominations are as great or greater than those between Catholics and Protestants (Mueller and Lane; Ryder and Westoff, a).

Arguments predicting fertility differences among Protestant groups have rarely been articulated. Petersen's view that "the effect of religion on the reproductive behavior of most persons in the West is now probably

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close to nil" (536) seems particularly descriptive of the usual approach to intra-Protestant variation (see DeJong). Because major Protestant denominations are similar in their reproductive doctrines, and because those doctrines—in particular, that of responsible parenthood (see Dyck)—leave much of the decision-making to the individual couple, other factors have been invoked to explain fertility patterns among Protestant groups. The implicit assumption has been that, like fertility differences between Jews and Protestants (Freedman et al.), those among Protestant groups result from compositional differences in the distribution of residence, socioeconomic status, and other social characteristics: "What may seem to be a religious influence often reflects the fact that members of any denomination are typically concentrated in a very few places in the social structure as defined by occupation, education, income or any other of the usual indices" (Petersen, 536). I shall refer to this view as the characteristics hypothesis, borrowing a term used to describe a similar explanation for differential racial and ethnic group fertility (see Goldscheider and Uhlenberg).

DeJong has developed a contrasting explanation which holds that some Protestant beliefs may affect reproduction. I shall refer to this view as the doctrine hypothesis, because it argues that differences in doctrine among Protestants along the liberal-conservative continuum may lead members of more conservative or fundamentalist denominations to have larger families. First, because more conservative Protestant denominations emphasize the ultimate and infallible authority of the Bible, their members tend to take literally such passages as Genesis 1:28 (*Good News Bible*): "Have many children, so that your descendants will live all over the earth and bring it under their control." Second, because more conservative Protestants emphasize that life is under God's will and plan, they may see contraception and abortion as interfering with God's design for mankind (DeJong).

The two parts of DeJong's argument suggest quite different but complementary mechanisms for enhancing fertility among members of conservative denominations. The explanation based on literal interpretation of pronatalist Scriptures suggests that members of more conservative Protestant groups would have more children because they *wanted* those children. The explanation based on ambivalence toward birth control suggests that members of more conservative Protestant groups would have higher fertility *not* because they *wanted* the children, but because of less efficient use of contraceptives and a lesser willingness to abort. Put another way, DeJong's arguments focus, respectively, on the demand for, and the supply of, births.

Using data from the 1965 *National Fertility Survey*, this paper reports an investigation of fertility differences between members of liberal and conservative Protestant denominations to test the characteristics hypothesis and the doctrine hypothesis. To distinguish between the supply and

demand parts of the doctrine hypothesis, the analysis is conducted separately for the wanted and unwanted components of fertility. (See Bean and Swicegood; Bean et al.; Westoff and Ryder, for further discussion of wanted and unwanted fertility as measures of the supply of and demand for births.)

There is little previous evidence to suggest which hypothesis might expect greater support. DeJong investigated fertility values, not actual child-bearing. Only a handful of studies have examined fertility variations among Protestant denominations. In their report on the 1965 *National Fertility Survey*, Ryder and Westoff (a) found that more moderate and liberal denominations had very similar average family sizes among their members (Episcopalians, 2.1 children born per woman; Methodists, Presbyterians, Congregationalists, and Evangelical United Brethren, each 2.2), but traditionally more conservative groups were slightly higher (Baptists, 2.3; Lutherans, 2.5; fundamentalist sects, 2.8). Mueller and Lane, using the March 1957 *Current Population Survey*, found the range in children ever born per 1000 ever-married women aged 15-44 to vary from 2,381 for Baptists to 1,922 for Presbyterians. Neither study controlled for potentially confounding variables, but Whelpton et al. present results consistent with the doctrine hypothesis after controlling for socioeconomic status and residence.

## Data and Methods

The data are taken from the 1965 *National Fertility Survey* (see Ryder and Westoff (a) for greater detail on the sample). This survey is the most recent national sample of women that contains detailed information on both fertility and religion. (Neither the 1970 *National Fertility Survey* nor the 1973 or 1976 *National Survey of Family Growth* contains as much detail on Protestant affiliation.) From the original sample of 5,617 currently married women were drawn all white, Protestant women who wanted at least two children, who had borne all the children they wanted, and who had consistent reproductive histories, that is, who had had no wanted births after an unwanted birth. A further exclusion of women in a residual "other Protestant" category (see explanation below) left a sample of 1,359. The restriction to women who wanted at least two children is intended to exclude women whose wanted fertility is below the minimum family size considered normative in American society (see Griffith), because the accuracy of such women's records may be questioned (Ryder and Westoff, b). The restriction to women who have had all the children they want is intended to include only women who have been subject to the risk of an unwanted birth. The exclusion of women with inconsistent reproductive histories reflects the possibility that such women may have misreported their unwanted fertility (Ryder and Westoff, b). Black women were excluded to remove the possible confounding influence of race, particularly because



blacks tend to be more concentrated than whites in more conservative denominations.

The dependent variable, fertility, is measured as the number of children born at the time of the interview, and is divided into two components, wanted and unwanted. Both components are determined by a series of retrospective questions designed to see if a woman had more children than she actually preferred (see Ryder and Westoff, for a detailed discussion of the measurement problem). In general, wanted fertility designates those births wanted at time of conception or some future date; unwanted fertility, births not wanted at conception or in the future.

Doctrine is measured by denominational liberalism-conservatism, using a modified version of a typology devised by Glock and Stark (120-1). The liberal category includes members of those denominations they classified as either liberal or moderate: Episcopal, Methodist, Presbyterian, Congregational, and Evangelical United Brethren (who merged with the Methodists in 1968). The conservative category includes denominations labeled by Glock and Stark as fundamentalist or conservative, in particular American Baptist, Southern Baptist, American Lutheran, Missouri Synod Lutheran, as well as several smaller groups (already aggregated by the *National Fertility Survey* under the heading "fundamentalist sects"), including Christian Missionary Alliance, Church of Christ, Church of the Nazarene, Church of God, Assembly of God, Jehovah's Witnesses, Seventh Day Adventist, Salvation Army, and other Baptist. Several denominations, notably Disciples of Christ, had to be excluded because their members had been placed in a residual category, "other Protestant," on the *National Fertility Survey* data file, a category too heterogeneous to fit either the liberal or conservative classification. To represent denominational liberalism-conservatism, a dummy variable is used in the analyses: 1, if conservative; 0, if liberal.

Because members of a particular denomination are not identical in their involvement in its religious subculture, however, membership per se may be too broad an indicator of doctrine. It seems reasonable to expect that the sorts of relationships predicted by the doctrine hypothesis would be more likely to appear among persons with greater participation in the common life of the group to which they belong, since association with other members is likely to reinforce distinctive beliefs. This further possibility is tested by examining the impact of denominational liberalism-conservatism within categories of formal religious participation. Formal participation is measured by church attendance (see Mueller and Johnson): 1, once a week or more; 0, less than once a week.

Four socioeconomic status variables (wife's education; husband's education, income, and occupation) and two measures of residence are included in the analyses. Education is measured as years of schooling completed. Income is measured in dollars; because the 1965 *National Fer-*

*tility Survey* coded income in categories (most were \$1,000 in width), the dollar amounts represent the midpoint of the income interval to which the couple had been assigned. (The midpoint of the open-ended upper interval (\$15,000 and over) was calculated at \$16,300 by sketching a curve to the income distribution and calculating the point which divided in half the area under the curve above \$15,000.) Occupation is measured by a decile scale, in which the highest status occupations are ranked at 9, the lowest at 0. Size of place of residence is also measured by a rank order, with the largest category, large cities, coded 1, and the smallest category, rural farm, coded 8. Region is measured by a set of four dummy variables, representing the Northeast, Midwest, South, and Far West; each couple was assigned a 1 for the variable representing the region in which they lived, a 0 for the other three.

To adjust for possible liberal-conservative differences in the length of the childbearing period, age and age at marriage are controlled. Exposure to risk of an unwanted pregnancy is included in analyses of unwanted fertility to control for possible differences in time subject to the risk of an unwanted pregnancy. If either denominational category displayed higher wanted fertility, its members might have lower unwanted fertility only because they had fewer years after completing their wanted families to become pregnant accidentally. Age and age at marriage are measured in years, exposure to risk in months since last wanted birth.

Dummy variable multiple regression analysis is used to test the hypotheses. In parallel analyses, each of the fertility measures is regressed on the independent and control variables. Both unadjusted and adjusted outcomes are presented. In the regressions one dummy variable from each set was deleted to avoid linear dependency (see Suits). The coefficients for the remaining variable or variables in each set can be interpreted as deviations from this omitted category. The means and standard deviations for variables in the regressions are presented in Table 1.

## Results

The results of the unadjusted regressions are presented in the first column of Table 2 (panel A for wanted fertility, panel B for unwanted fertility). In general, the coefficients are in the directions expected and similar for both measures of fertility. Members of more conservative denominations reveal both higher wanted ( $b = .234$ ) and unwanted ( $b = .081$ ) fertility, although only the former coefficient is significant (based on an F-test). The coefficients for socioeconomic status, residence, and control variables also reveal expected effects, and all are significantly related to at least one of the fertility components (most at  $p < .001$ ) except for region of residence. Only

**Table 1.** MEANS AND STANDARD DEVIATIONS FOR VARIABLES IN THE REGRESSIONS

Variables	Mean	Standard Deviation
Denomination		
Conservative	.561	.496
Liberal	.439	.496
Socioeconomic status		
Husband's education	11.295	3.081
Husband's income	7.555	3.559
Husband's occupation	5.608	2.668
Wife's education	11.336	2.414
Residence		
Size of place	5.213	1.848
Region		
Northeast	.170	.376
Midwest	.319	.466
South	.365	.482
West	.146	.357
Religious participation		
High	.491	.500
Low	.509	.500
Wife's age	37.515	8.217
Wife's age at marriage	19.890	3.201
Risk	114.931	82.763
Wanted fertility	2.972	1.259
Unwanted fertility	.425	.969

(N = 1,359)

religious participation reveals unexpected results: less frequent attenders have higher fertility.

Each fertility component was next regressed on all of the independent and control variables simultaneously in parallel analyses. The outcomes of these adjusted regressions are presented in the second column of Table 2. Most variables reveal effects similar to those in the unadjusted runs, although some no longer attain statistically significant relationships with fertility. But the shift to nonsignificance of the socioeconomic status variables (except for wife's education) may understate their impact on fertility because of a property of multiple regression: Gordon has demonstrated that, in a subset of independent variables, the one most highly correlated with the dependent variable (here, wife's education) may be the only one to reveal a significant effect.

For wanted fertility, the effect is in the direction predicted ( $b = .116$ ),

Table 2. REGRESSIONS OF WANTED AND UNWANTED FERTILITY ON INDEPENDENT VARIABLES (UNSTANDARDIZED COEFFICIENTS)

Independent Variables	A. Wanted Births			B. Unwanted Births		
	Unadjusted	Adjusted Additive	Adjusted Interactive	Unadjusted	Adjusted Additive	Adjusted Interactive
Denomination						
Conservative	.234**	.116 *	.248**	.081 *	-.024 *	.046 *
Liberal						
Socioeconomic status						
Husband's education	-.067**	.009	.009	-.057**	-.003	-.003
Husband's income	-.032**	.006	.006	-.033**	-.008	-.009
Husband's occupation	-.065**	-.018	-.020	-.045**	-.008	-.009
Wife's education	-.121**	-.079**	-.079**	-.096**	-.072**	-.072**
Residence						
Size of place	-.087**	.056***	.056***	.018	-.005	-.005
Region						
Northeast	.088	.043	.044	.053	-.033	-.033
Midwest	.282	.169	.170	.028	-.026	-.025
South	.162	-.014	-.012	.049	-.064	-.063
West	*	*	*	*	*	*
Religious participation						
High	-.131 *	-.078 *	.084 *	-.183**	-.065 *	.020 *
Low						
Wife's age	.022**	.027**	.027**	.006	-.032**	-.032**
Wife's age at marriage	-.048**	-.043**	-.045**	-.043**	.010	.009
Risk						
Interaction						
Denomination by religious participation						
Conservative by high	*	*	-.283 <sup>†</sup>	*	*	-.149 *
Conservative by low	*	*	*	*	*	*
Liberal by high	*	*	*	*	*	*
Liberal by low	*	*	*	*	*	*
Constant		3.303	3.262		1.960	1.940
R <sup>2</sup>		.094	.097		.124	.126
F		11.661**	11.141**		14.693**	13.811**
F (increment to R <sup>2</sup> of adding interaction)		4.456 <sup>†</sup>			2.169	

\*Deleted from the regression    \*p < .001.    \*\*p < .01.    †p < .05.

but for unwanted fertility it becomes negative ( $b = -.024$ ). One can place little confidence in either finding, however, for both fail to attain statistical significance (based on an F-test,  $p < .05$ ). In other words, when the effects of social characteristics and control variables have been taken into account, the association between denominational liberalism-conservatism and both measures of fertility has a high probability of resulting from chance. This outcome supports the characteristics hypothesis. Variations in wanted and unwanted fertility between members of conservative and liberal denominations reflect variations in socioeconomic status, residence patterns, and the control variables, not doctrine.

Somewhat different results emerge, however, when the possibility is examined that the doctrine hypothesis might hold only among those high in religious participation. First-order multiplicative interaction terms between denominational liberalism-conservatism and religious participation were created and the adjusted regressions repeated with these additional terms. The results, presented in column 3 of Table 2, differ for the two fertility measures. For wanted fertility, a negative effect ( $b = -.283$ ) is found, statistically significant at  $p < .05$ . At the same time, the coefficient for denominational liberalism-conservatism increases and becomes statistically significant ( $b = .248$ ,  $p < .01$ ). This finding indicates that denominational membership and religious participation interact in their effects on fertility, but in an unanticipated direction. Instead of higher religious participation being associated with higher fertility among members of conservative denominations, as predicted, the opposite result occurs: higher fertility is found among conservative members low in participation. This finding indicates an effect of Protestant liberalism-conservatism on wanted fertility, but is not consistent with the expectations of the doctrine hypothesis. This outcome is explored further in the next section. For unwanted fertility, the introduction of denomination by religious participation interaction terms does not add significantly to the explained variance of the additive model (based on an F-test of the increment to the multiple correlation coefficient of the additive model from adding interaction), indicating that level of religious participation does not affect the unwanted fertility of members within the conservative and liberal denominational categories.

## Discussion and Conclusions

This study has undertaken a first test of two hypotheses which argue alternative explanations for fertility differences between members of conservative and liberal Protestant denominations. The results for unwanted fertility support the characteristics hypothesis, but the findings for wanted fertility support neither hypothesis. As the doctrine hypothesis predicted, after controlling for the effects of social characteristics and residence, mem-

bers of more conservative denominations revealed higher wanted fertility than members of more liberal denominations. Unexpectedly, however, further analysis indicated that this effect operates only among those conservative members who have lower levels of religious participation.

One cannot eliminate the possibility that the results reflect differential misreporting of the status of births. If members of conservative denominations experience greater doctrinal pressure for larger families, they might be less willing to admit that a particular birth is unwanted, and thus inflate their actual demand for births. Still, it is not obvious why misreporting would lead to an effect only among conservative Protestants low in participation.

If one assumes that the results indicate a genuine fertility-enhancing effect of conservative membership, this unanticipated outcome remains hard to explain. Why is the effect not greater among those who participate more in the common religious life, i.e., those with a higher frequency of church attendance? Perhaps those aspects of belief which enhance fertility are more likely to operate among persons less involved in the life of the church. In some unknown fashion, teachings which affect fertility are reinforced more effectively through informal channels than through formal ones.

A more plausible explanation reverses the direction of effect. It may be that those with larger families attend church less frequently because of time demands associated with several children. Glock et al. provide suggestive evidence for this view in a study of Episcopal parishioners. Lowest church involvement scores were obtained by married persons with children, lower than scores for married persons without children or not currently married persons regardless of child presence. No attempt was made to sort out the effect of number of children—their main concern was the differing involvement of intact and nonintact families—but these results are consistent with the view that children can interfere with church participation. Hence, the low religious participation category in the present study might be expected to include both couples who chose to participate infrequently as well as those who, because of family obligations, did not participate as often as they would have preferred.

Further analysis was undertaken to explore this possible explanation. Women who belonged to conservative denominations and were low in religious participation were selected and the regressions recomputed for this subsample ( $N=361$ ) with the addition of a new independent variable, daily religious activities (coded as 1, yes, or 0, no, based on responses to the question, "Are there religious activities in the daily life of your family, such as having family prayers, reading the Bible, saying grace at meals, and so forth?"). If large families keep women from regular formal participation, daily activities in the home would provide an alternative indicator of devotion. Women low in church attendance who still participate at home

would be expected to have larger families than those who do not participate. Because this argument would apply whether the births were wanted or unwanted, the analysis is undertaken for both fertility components.

The results, presented in Table 3, are consistent with expectations. Daily religious activities are positively associated with wanted fertility ( $b = .302$ ,  $p < .1$ ). For unwanted fertility, the results are similar ( $b = .225$ ,  $p < .05$ ). Hence, conservative members low in participation have higher fertility if they participate in religious activities at home. Under certain conditions, then, conservative membership can enhance both the supply of and demand for births. This finding provides qualified support for the doctrine hypothesis. Of course, further analysis is needed to confirm that these results indicate a fertility-enhancing effect of conservative denominational membership. A longitudinal approach would be useful to untangle the relations among denominational liberalism-conservatism, religious participation, and fertility.

The finding of a small effect on fertility for a subset of conservative Protestants should not obscure the fact that much of the fertility differential between members of conservative and liberal denominations is due to compositional differences in socioeconomic status, residence, and other

**Table 3.** REGRESSIONS OF WANTED AND UNWANTED FERTILITY ON INDEPENDENT VARIABLES FOR MEMBERS OF CONSERVATIVE DENOMINATIONS LOW IN RELIGIOUS PARTICIPATION (N = 361) (UNSTANDARDIZED COEFFICIENTS)

Independent Variables	Wanted Fertility Adjusted Additive	Unwanted Fertility Adjusted Additive
Socioeconomic status		
Husband's education	-.044	-.004
Husband's income	-.004	-.027
Husband's occupation	-.031	-.034
Wife's education	-.119***	-.075†
Residence		
Size of place	.067	.008
Region		
Northeast	.186	.052
Midwest	.098	.011
South	-.148	-.163
West	*	*
Daily religious activities		
Yes	.302†	.255†
No	*	*
Wife's age	.046**	-.039***
Wife's age at marriage	-.046	.021
Risk	*	.006**
Constant	3.738	1.912
R <sup>2</sup>	.172	.148
F	6.592**	5.035**

\*Deleted from the regression. \*\* $p < .001$ . \*\*\* $p < .01$ . † $p < .05$ . ‡ $p < .1$ .

variables controlled in this analysis. The study of Protestant fertility is not likely to yield the large differences, typical in the past, of Catholic-Protestant comparisons; but the findings of this study still suggest that Protestant patterns merit closer inspection. Recent surveys have found an increase in religious interest in the United States (Gallup), probably due in part to the so-called evangelical movement. Yet almost nothing is known about the fertility consequences of this phenomenon. As Catholic and Protestant fertility levels continue to converge (Westoff and Jones), this and other dimensions of Protestant life seem promising choices as new arenas for the study of American fertility behavior.

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# The New Mobility Ratio\*

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## ABSTRACT

*A new mobility ratio has been proposed as a way of dealing with the confounding between main and interaction terms which is inherent in the old mobility ratio. It is shown that the new ratio does not in fact succeed in this aim, that values of the ratio are highly dependent on the model fitted to account for interaction, and that criticisms of Blau and Duncan's conclusions about the mobility of American society, which derive from application of the new ratio, cannot be sustained. The old mobility ratio, unlike the new, always has a determinate value, and its alleged defects are formally equivalent to properties which are not usually regarded as defects in a multivariate analysis of discriminance.*

In Chapter 4 of their monograph *Opportunity and Change*, Featherman and Hauser present a new mobility ratio for the quantification of the individual cells of a social mobility table. They make two claims about this ratio, one substantive and the other methodological. The methodological claim is that the new mobility ratio is equivalent to a coefficient suggested by Goodman, the only difference being that Goodman presents values of the coefficient for only the diagonal cells of a mobility table. The substantive claim is that the new ratio gives quite different results from other measures. In particular, it is held that the new ratio shows that American society is a good deal less mobile at the top and bottom than was suggested by Blau and Duncan on the basis of the old mobility ratio (that is the ratio of observed to expected frequency in a cell, where expectations are computed on the model of independence of rows and columns, a model which students of mobility commonly call the model of "perfect mobility"). The difference, it should be noted, is not a difference in the data; it is a difference in the method of presenting and analyzing the data. Indeed, Featherman and Hauser show that there was very little change in the mobility of American men between Blau and Duncan's inquiry in 1962 and their own inquiry in 1973. They write:

\*Paper presented to a seminar at the University of Wisconsin-Madison, 1980. I am indebted to Professor Halliman H. Winsborough for comments and suggestions.

Why was the Blau–Duncan interpretation of the mobility table so far off the mark? The problem lay, again, in the confounding of row and column effects with interactions. They measured the permeability of class boundaries with respect to directional movements by comparing (old) mobility ratios above and below the main diagonal of each mobility table (Blau and Duncan, 58 f). They attributed the larger number of entries greater than unity below than above the main diagonal to 'one-way screens that permit a proportionate flow only in the upward and not in the downward direction' (59). These asymmetries primarily reflected the net upward shift in occupations between origin and destination distributions. It is instructive that their interpretation was not supported in the 1962 table of mobility from father's occupation to son's first occupation because it did not show a net upward shift in the occupational distribution. Blau and Duncan (59) appear to have recognized this, but did not draw from it the appropriate methodological inference. (178).

Featherman and Hauser go on:

As an alternative to the Blau–Duncan interpretation, we think our multiplicative models yield a cogent and parsimonious description of occupational mobility among American men. Unlike its precursor, our description does not reflect the shape of occupational distributions of origin or destination, but only the underlying patterns of immobility and exchange between occupational strata (179).

The old mobility ratio was put forward by Rogoff and Glass. Criticisms of it have been expressed by Billewicz, Blau and Duncan, and Tyree. Featherman and Hauser say:

'that there is an undesirable part-whole relationship between the internal frequencies and marginal (row and column) effects under the independence model.' And they claim that 'in the revised model that part-whole relationship is eliminated, and the structure of the data becomes clear' (144).

In what follows we examine and cast doubt on the claim that the new mobility ratio is a general way of disentangling interaction and row-and-column effects in a contingency table. The crux of our argument is that Goodman developed a coefficient which was peculiarly adapted to his model of "quasi-independence," and that attempts to apply it to models with quite different properties are conceptually flawed and give highly unstable results. In contrast to the new mobility ratio, the old mobility ratio always yields determinate values, and its alleged defects are not considered to be defects when they (or their formal analogues) manifest themselves in other areas of multivariate analysis.

#### GOODMAN'S "NEW INDEX OF STATUS IMMOBILITY"

In considering the relations between Goodman's "new index of status immobility" and Hauser's "new mobility ratio" (Featherman and Hauser; Hauser) we must distinguish between the coefficient itself and the model

from which particular values of it happen to be derived. In order to effect this distinction we repeat Goodman's exemplary analysis using a different, but entirely equivalent method.

The technique which we employ is that of regression (Nelder). In this technique the logarithms of the frequencies in a table are arranged in a column, and this vector is regressed, by weighted least squares, on a matrix of independent variables. For the purpose of reproducing Goodman's example we employ the design matrix shown in Table 1. Goodman's full model (which he calls a model of "quasi-independence") involves fitting the grand mean, two terms for the three rows, two terms for the three columns, and a separate term for each diagonal cell. The parameters for this model are given in the table. By multiplying each element of a row in the design matrix by the corresponding parameter, and summing across all columns, we obtain the logarithm of the expectation generated by the model ( $e_{full}$ ). The denominator of Goodman's index is simply a truncated version of this equation in which the parameters for status inheritance are omitted (or set to zero). The truncated equation gives the logarithm of the "expectation"  $e_{part}$ . These latter expectations satisfy the perfect mobility criterion (statistical independence of rows and columns) but they do not sum to the same grand total as the observed frequencies 0.

It is well known (Snedecor) that simply truncating a regression equation does not, in general, produce an optimal or theoretically significant model. Goodman's purpose, clearly, is to fit a two-stage model in which certain cells are fitted and then left alone while others are fitted. His truncated model yields a best fit to the off-diagonal cells, and the expectations for these cells are not altered when terms are added for the remaining cells. In fact, the model has obvious affinities with the McNemar-Bowker test (Bowker) in that it allows the diagonal cells to take any values they please without affecting the fit to the non-diagonal cells. (The nature of the McNemar-Bowker test has been expounded elsewhere [Hope d], with the aid of the sort of design matrix shown in Table 1.)

Goodman called his index an "index of status immobility," and it has to be asked whether it is a general index, or one peculiarly adapted to the two-stage character of the quasi-independence model.

#### THE NEW MOBILITY RATIO

In *Opportunity and Change* Featherman and Hauser present a  $5 \times 5$  table of male intergenerational mobility which is reproduced in Table 2. Beneath each observed frequency is the expectation  $e_{part}$  which is generated by application of Goodman's quasi-independence model. Clearly, the model does not fit the off-diagonal cells and therefore it does not give us a license to say (for example) that 372 men would stay in category one if the diagonal cells conformed to the pattern of the off-diagonal cells. What Featherman



Table 4. EXPECTATION IN EACH CELL,  $e_{\text{FATHER}}$ , COMPUTED (a) FROM PARAMETERS  $b$ , IN TABLE 3, AND (b) FROM EITHER OF THE OTHER TWO SETS OF PARAMETERS ( $b$ , AND  $b_A$ )\*

Father's Occupation	Son's Occupation				Farm
	Upper Nonmanual	Lower Nonmanual	Upper Manual	Lower Manual	
Upper nonmanual	412 288	404 283	425 297	926 649	63 44
Lower nonmanual	418 293	410 287	431 302	940 659	64 45
Upper manual	1,077 754	1,056 740	1,110 778	2,423 1,698	166 116
Lower manual	1,159 812	1,137 797	1,196 838	2,609 1,828	178 125
Farm	566 396	555 389	583 409	1,273 892	87 61

\*Featherman and Hauser (Table 4.7) employ the former as denominators of their new mobility ratio.

**Table 5.** EXPECTATIONS ( $e_{sat}$ ) AND NEW MOBILITY RATIOS FOR THE FEATHERMAN-HAUSER (FH) MODEL ( $\chi^2 = 67$ ,  $df = 12$ ) AND FOR TWO COMPLETELY SATURATED MODELS ( $\chi^2 = 0$   $df = 0$ )

Cell	Observed Frequency	FH Model		First Saturated Model		Second Saturated Model	
		$e_{Part}$	Mobility Ratio	$e_{Part}$	Mobility Ratio	$e_{Part}$	Mobility Ratio
11	1,414	417	3.39	31	45.61	3,325	0.43
12	521	409	1.27	27	19.30	1,225	0.43
13	302	430	0.70	33	9.15	710	0.43
14	643	938	0.69	122	5.27	1,512	0.43
15	40	64	0.63	139	0.29	94	0.43
21	724	423	1.71	37	19.57	1,702	0.43
22	524	415	1.26	32	16.38	627	0.84
23	254	436	0.58	40	6.35	364	0.68
24	703	952	0.74	146	4.82	774	0.91
25	48	65	0.74	166	0.29	48	1.00
31	798	1,090	0.73	84	9.50	1,876	0.43
32	648	1,069	0.61	73	8.88	691	0.94
33	856	1,124	0.76	90	9.51	401	2.13
34	1,676	2,453	0.68	329	5.09	853	1.96
35	108	168	0.64	374	0.29	53	2.04
41	756	1,174	0.64	183	4.13	1,778	0.43
42	914	1,151	0.79	160	5.71	655	1.40
43	771	1,210	0.64	198	3.89	380	2.03
44	3,325	2,641	1.26	722	4.61	808	4.12
45	237	181	1.31	821	0.29	50	4.74
51	409	573	0.71	1,417	0.29	962	0.43
52	357	562	0.64	1,236	0.29	354	1.01
53	441	591	0.75	1,527	0.29	205	2.15
54	1,611	1,289	1.25	5,580	0.29	437	3.69
55	1,832	88	20.82	6,345	0.29	27	67.85
Sum	19,912	19,913		19,912		19,911	

that the three sets of coefficients do not all yield the same expectations  $e_{part}$ , if by  $e_{part}$  we mean the expectations computed from the first eleven parameters only. Nevertheless  $e_{part}$  for the various equations is determined up to a coefficient of proportionality (Table 4). What Featherman and Hauser do in order to fix the absolute value of these expectations is to set the mean of the "level" coefficients, over the twenty-five cells of the table, to zero by adjusting the parameter for the grand mean. A more intuitively appealing standardization might be to set the sum of the  $e_{part}$  to the observed total in the table, and this has been done in Table 5. This standardization is analogous to the standardization implicit in the Goodman index, in that there the sum of the  $e_{part}$  over the off-diagonal cells is equal to the sum of the observed values for those cells (Tables 1 and 2). The new mobility ratio is the ratio of the observed frequency for a cell to  $e_{part}$  for that cell. Because of the slight difference in our standardization procedure, the values of the ratio given in Table 5 are slightly different (by a constant factor) from those given by Featherman and Hauser (1959). (The difference in standardization is only a minor change and it does not constitute part of our criticism of the new mobility ratio.)

### Criticism of the New Mobility Ratio

The criticism which has been advanced against the old mobility ratio is that "the confounding of the interaction effects and marginal effects under the independence model (when it does not fit the data) prevents us from seeing the pattern of the interaction effects when we look at the mobility ratios" (Featherman and Hauser, 144). The claim made for the new mobility ratio is that it takes appropriate steps to deal with this confounding. But does it?

The point which is being made is fairly straightforward: in fitting loglinear models to a mobility table we ordinarily begin by fitting terms for rows and columns (though it has been argued elsewhere [Hope, e] that this is not where we should in fact begin), and then we go on to fit terms for the interaction of rows and columns. The parameters for the row and column terms when no interactions are fitted are, of course, different from the parameters for those terms when interactions *are* fitted. The claim for the new mobility ratio is that the latter parameters (which generate  $e_{part}$ ) are more appropriate than the former parameters (which generate the expectations for the perfect mobility model) as denominators of mobility ratios. But why should they be more appropriate? It is well known that if a regression model is split into two subsets of terms, *A* and *B*, then an alteration in the composition of *B* will in general induce a change in the parameters for *A*. To illustrate this for a loglinear model let us replace the "level" parameters of Table 3 with sixteen terms, each being the product of



a term for one of the first four rows and a term for one of the first four columns. This yields a "fully saturated model" which fits the interactions perfectly, and it gives the expectations  $e_{part}$ , and the mobility ratios, shown in Table 5 under the heading "First saturated model."

The obvious inference to be drawn is that the values of mobility ratios are highly model-dependent. There is, however, a further inference, which is illustrated by the "Second saturated model." This is, logically, entirely equivalent to the first. Like its companion it fits all the remaining degrees of freedom and reduces chi square to zero (each model, in fact, consists of sixteen contrasts for sixteen of the diagonal cells). The second saturated model is constructed exactly like the first except that instead of dropping terms for the last row and column we drop terms for the first row and column.

Once again, the inference from the column of mobility ratios is obvious: not only is the new mobility ratio model-dependent, it is also dependent on the precise set of axes chosen to map a given area (in this case the whole) of the data space. We might represent the argument by imagining a simple, two-circle Venn diagram which shows the specific row-and-column space, the specific interaction space, and the shared space. The two saturated models of Table 5 account for the same specific interaction space, but they take different bites out of the row-and-column space.

The reason for the arbitrary nature of the mobility ratio is not far to seek, since it resides in just that "confounding" between row-and-column terms and interaction terms which the ratio was intended to make allowance for. The parameters of the truncated row-and-column model (which generate  $e_{part}$ ) are highly dependent on the precise form of the terms representing interactions. It might be possible to suggest an all-purpose set of interaction terms with which to generate standard denominators for the mobility ratio. For example, we might define  $r - 1$  orthogonal polynomials on the rows, and  $c - 1$  orthogonal polynomials on the columns, and take every possible product of a row polynomial and a column polynomial. But it should be evident that the mobility ratios reported by Featherman and Hauser are model-specific and cannot be held to disprove the inferences which Blau and Duncan drew from the old mobility ratios.

It should be noted that what has been demonstrated is that the new mobility ratio is dependent on the particular set of terms which is chosen to represent interaction. It is *not* the case that the ratio depends on the set of terms used to present row-and-column effects. On the contrary, we may represent the row-and-column effects by several different design matrices without altering the model, or the new mobility ratios, in any way at all. For example, instead of dropping terms for the last row and column (signalled by values of zero for  $b_k$  in table 3) we might drop the first row and column. Or we might substitute two sets of orthogonal polynomials, as already described. Or we might employ the disaggregation of perfect mo-

bility into two complementary models,  $H$  and  $D$ , which form part of the structured diamond model of mobility analysis (Hope, e; see below). Whichever of these we choose to employ, we find that the model does not alter a whit, so long as the set of interaction terms is held constant. In other words, different but formally equivalent representations of the row-and-column space have no effect on the new mobility ratio. But Table 5 shows that different but formally equivalent representations of the *interaction* space may greatly alter the new mobility ratio.

Whatever axes we choose to express the row-and-column (perfect mobility) variance, these always generate the same expectations, namely the expectations on a model of statistical independence of rows and columns. Hence the old mobility ratio always has a determinate value. It is true, as Tyree has elegantly shown, that the old mobility ratio does not, as was once supposed, partial out differences in the size of marginal categories. But this is not so much a failing as a fact. If we consider a log-linear analysis as a canonical analysis (Hope, b,c), then the error variance-covariance matrix is a function of the marginal frequencies. Two mobility tables which have different marginal frequencies are analogous to two samples which have different within-sample variance-covariance matrices. The "structured diamond model" of mobility analysis which has been set out elsewhere (Hope, e) has the merit that it explicitly looks at differences in within-sample covariance matrices, and quantifies them on the same scale of percentage importance as the interaction effects.

Consideration of the structured model shows that the true base model of mobility analysis is, not the ordinary model of statistical independence, but a subspace of that model which represents statistical independence within constant margins (model  $H$ ). Considered as a regression model, what  $H$  does is to constrain the parameter for father's category  $i$  to be the same as the parameter for son's category  $i$ , for all  $i$ . And because  $H + D$  is a way of writing the perfect mobility model (which fits the marginal distributions perfectly), it is easy to see that  $D$  quantifies the extent to which the equality constraint fails to fit the data. The variance accounted for by  $D$  is a matter of substantive sociological interest, and not something to be partialled out as if it were an artifact. It tells us the extent to which mobility is due to intergenerational structural change. This implies that the appropriate denominators for a mobility ratio are the expectations generated by  $H$  (which stands for "halfway model"; Hope, a,e).

However, the structured model also shows that the numerator of the ordinary (new or old) mobility ratio, that is the observed frequency in a cell, can be broken down into several sociologically distinct, and statistically distinguishable, components. For purposes of exposition, there may occasionally be some advantage in expressing one or more of these components in ratio, or in logarithmic form, perhaps employing the halfway model as base. But in general, computed ratios will be less perspicuous

than the disaggregated components of expectations from which they are derived.

There can be no general rule for presenting mobility data, once we grant the compound nature of every observed frequency. It may, however, be observed that loglinear regression differs from ordinary regression in that the latter explains variance in the dependent variable by reference to an additive combination of the independent variables, whereas the former explains frequencies (not squared frequencies) by reference to a multiplicative combination of the independent variables. The inference which I draw from this is that there is a lot to be said for deemphasizing the parameters of a loglinear analysis (which are, in any case, often highly dependent on the particular set of axes chosen to represent a model) in order to concentrate on the decomposition of frequencies. It is a simple matter to compute, for each cell of a mobility table, the expectation generated by the halfway model, followed by increments or decrements due to each successive term of the structured model (Hope d, e).

The last element in a cell is the residual, that is the element which ensures that the values in the cell sum to the observed frequency. If the residuals are small then there is no need to report mobility ratios or other functions of parameters: all the further information we require is given in the table which records the percentage of mobility variance accounted for by the various pieces of the model. The main exception to this generalization relates to any aspect of the model which is identified with a single degree of freedom. For this, it makes sense to report the value of the parameter which accounts for that degree of freedom.

### Concluding Remarks

The new mobility ratio is model-dependent. It is even dependent on the particular set of axes chosen to represent a given model (Table 5). So unless we are quite sure that we have somehow arrived at *the correct* model, we can place little credence in our new mobility ratios.

Clearly, one could test the probability of arriving at a correct model by postulating a true, generative, model, synthesizing a number of mobility tables from it by the addition of random disturbances, and then inviting analysts to infer the generative model from the synthetic tables. And one could vary the generative model in a number of directions to see how this affected the analysts' chances of lighting on it. However, it is not clear how one would arrive at a realistic form for the generative model in the first place, or what would be the credible limits on its variations.

Traditionally, the degree of openness of a society has been assessed by fitting a model of perfect mobility to a mobility table and defining exchange mobility as deviation from the model. The logic of the new mo-

bility ratio is closer to that of econometric analysis, in which the parameters are regarded as quantifying aspects of a theory-based model. In the latter case it makes sense to regard all parameters as being on the same footing, and to construct an index from some subset of them. What we have shown in this paper, however, is that the perfect mobility space has a determinate character which the interaction space lacks. Indeed, the interaction space is *defined* as that which is left over after perfect mobility has been fitted, and therefore it is *not* appropriate to treat the parameters of the two spaces as being on the same footing. The traditional, sequential, logic of social mobility analysis is basically sound. The "structured diamond" model shows how it can be elaborated and given greater precision and sociological significance.

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# On the Formulation of a Structural Model of the Mobility Table\*

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## ABSTRACT

*The note explores the implications of alternative formulations of the Hauser structural model of the mobility table.*

The structural model of the mobility table proposed by Hauser (a, b; Featherman and Hauser) has been recognized as a fruitful analysis device. This note discusses the formulation of the model, and suggests that a description in terms of odds-ratios may have advantages over the notion of levels or densities. We have no quarrel with the model in itself (though the argument will imply a certain skepticism of the sharp divide Hauser sees between his and traditional models) but our discussion may restrict the kinds of things that should be said about the model.

Since addition is, by tradition, more transparent than multiplication we present the arithmetic in logarithms, not raw values, though nothing hangs on this. The structural model then can be written:

$$F_{ij} = m + a_i + b_j + v_k \quad (1)$$

where  $F_{ij}$  are fitted values, and the settings of  $k$  are determined by the particular values of  $i$  and  $j$  (and are most conveniently presented in tabular form). For the mobility table considered in Featherman and Hauser (147–59) we are given the suggested arrangement of levels in Table 1A, yielding the parameter estimates in Table 1B. The authors treat the grouping of cells by density “levels” as interpretable categorizations. For example, they remark: “it is worth noting that four of the five density levels recognized in the model occur along the main diagonal” (151), and state: “Differences between level parameters may readily be interpreted as differences in logs of frequencies, net of row and column effects . . . the estimates say that immobility in farm occupations is 3.40 . . . greater . . . than the estimated mobility or immobility in cells assigned to Level 5” (156). Consider now the

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Table 1.\*

A. Asymmetric 5-Level Model of Mobility from Father's Occupation to First Full-Time Civilian Occupation					
Father's Occupation	Son's Occupation				
	(1)	(2)	(3)	(4)	(5)
1. Upper nonmanual	2	4	5	5	5
2. Lower nonmanual	3	4	5	5	5
3. Upper manual	5	5	5	5	5
4. Lower manual	5	5	5	4	4
5. Farm	5	5	5	4	1

B. Additive Parameters					
Design Factor	Category of row, column, or level				
	(1)	(2)	(3)	(4)	(5)
Rows (father's occupation)	-.466	-.451	.495	.570	-.148
Columns (son's occupation)	.209	.190	.240	1.020	-1.660
Levels (density)	3.044	1.234	.549	.243	-.356
Grand mean = 6.277					

\*From Featherman and Hauser (151, 155).

alternative level specification in Table 2A (which yields the parameters reported in Table 2B). Here only two of the levels reside on the diagonal, and the distance between the "farm" cell (5,5) and the base Level 5 is approximately one and a third times the value Featherman and Hauser report. The two models, on this formulation, appear different. Yet, as some addition will demonstrate, they yield identical sets of fitted values.

There would seem some merit in a formulation to render this equivalence explicit. Interaction effects in additive tabular models can be modified by adding constants to row or column effects whilst retaining fitted values, so we require an expression invariant under such transformation. The expression will be relational between cells, not an attribute of one cell. (Indeed, levels which do not meet the transformation criterion, and appear to be attributes, have themselves to be interpreted through dyadic relations—as in the quotation above—since their zero point is arbitrary.) An obvious tetradic candidate would be a measure of the disparity between consistent and inconsistent responses; where the four cells are named in the usual sequence,  $a$  to  $d$ , this would be:

$$(a + d) - (b + c)$$

Table 2.

A. "Level" Specification					
	2	2	2	3	3
	4	2	2	3	3
	5	3	2	3	3
	5	3	2	2	2
	5	3	2	2	1
B. Additive Parameters					
	(1)	(2)	(3)	(4)	(5)
Rows	-.466	-.451	.495	.570	-.148
Columns	1.122	.112	-.437	.942	-1.738
Levels	3.122	.321	-.278	-.364	-1.267
Grand mean = 6.277					

which is the logarithmic form of the familiar cross-product ratio or odds-ratio; that ratio is one of a family of such measures (Fienberg, 18) but has the attraction of formal simplicity in both raw and logarithmic modes. Following Goodman, and using the notation of (1) above, we define:

$C_{ij} = F_{ij} + F_{i+1, j+1} - F_{i, j+1} - F_{i+1, j}$  (2)

where the  $F_{ij}$  are (logarithmic) fitted values; by its logic,  $C_{ij}$  can equivalently be defined on the  $v_k$  of equation (1). We then express both Table 1A and Table 2A in the model formulation of Table 3A. From the parameters in Table 3B we can, again following Goodman (816), calculate the fitted values from:

$F_{ij} = m + a_i + b_j + \sum_{s=1}^{i-1} \sum_{t=1}^{j-1} C_{st}$  (3)

where we define  $C_{ij}=0$  for  $i=1$  or  $j=1$ . Table 3A, besides being unique, does reveal the structure of the model, and we can contrast this with the pattern in Table 4, which is the  $C^*_{ij}$  array calculated on the observed data.

Goldthorpe (96-7) was right to stress for mobility tables the interpretive utility of odds-ratios, but underutilized this presentation because misled by their apparent multiplicity. The full set, which is large, of  $C_{ij}$  values is exactly entailed by the  $(R-1)(C-1)$  table of interstitial  $C_{ij}$  (simply

Table 3.

A. $C_{ij}$ Array Implied by Tables 1 and 2					
	y	0	0	0	
	w	x	0	0	
	0	0	x	0	
	0	0	0	z	
B. Parameters					
	(1)	(2)	(3)	(4)	(5)
Rows	.625	-.045	-.004	.071	-.647
Columns	1.361	.351	-.198	.582	-2.098
$C_{ij}$	.306	.599	.685	2.801	
	(w)	(x)	(y)	(z)	
Constant	5.268				

Table 4.\*

7	-2	3	1
1	10	-3	-1
4	-4	8	1
-3	4	-2	28

\* $C_{ij}^*$  array calculated from observed data in Featherman and Hauser (150) (values are multiplied by 10 and rounded).

add the  $C_{ij}$  values between cells to obtain the  $C_{ij}$  for those cells), so presentation of the model can be reasonably compact. Duncan discusses an association between respondents' color and religious participation and presents his  $C_{ij}$  as "estimates of the two parameters displayed in the following 2 by 3 (color by response) matrix" (61):

$$\begin{matrix} x & y & 0 \\ 0 & 0 & 0 \end{matrix}$$

(where the estimates are  $x=.10$ ,  $y=-.34$ ) and notes that "there appears to be little effect of color where the contrast of the two extreme score groups is



concerned." The information could be more compactly given in an ordered pair of  $C_{ij}$  values, thus:

.44    -.34.

from which all three contrasts are readily seen. And whilst choice in a particular  $C_{ij}$  array involves an ordinality decision it is not one fraught with substantive import, for no claims about groupings of cells rest upon it; on changing it we are not led to unsay things previously said, and the alternatives are self-evidently equivalent. So Table 3 is claimed as more transparent than Tables 1 and 2.

We can readily see why the "levels" formulation in Table 1 is not unique. Trivially, the level to which the middle column is assigned is arbitrary; the column can be assigned to any of the five levels, and the corresponding column effect appropriately modified, without altering the estimated values. Further, consider column 1. We have a column 2 which reads (descending):

4   4   5   5   5

and (in Table 3) a set of  $C_{ij}$  between it and column 1 which reads:

$y$     $w$    0   0

where the 0 are literal zeros, and  $y$  and  $w$  are two particular parameters. On the formulation of Table 1, levels 2 and 3 do not appear more than once, so we can set the lower three cells of column 1 *any* other level, set the value of level 3 in (2,1) as required to recover  $w$ , and then set level 2 in (1,1) to recover  $y$ . None of these changes affect the estimated values, nor the model as seen in terms of the  $C_{ij}$  in Table 3.

The two formulations, in Tables 1 and 2, logically and of necessity, yield identical cell-by-cell fitted values. They do not yield approximately the same fit, which would return us to the familiar situation of competing models with different structures (where typically the "same fit" measure does not require tight correspondence between fitted values, observation by observation, but an equivalence of the *aggregated* disparities). It is not that they *happen* to come out at the same place; they *must* come out at the same place. This inferential equivalence is not a ground for rejecting either formulation, but it may attenuate our allegiance. Reading the account of the behavior of the residuals in cells grouped by levels (Featherman and Hauser, 154-55; Hauser, b, 425), the naive reader would be forgiven for assuming that the cell-by-cell pattern of residuals is consequent on that grouping by levels. The realization that other patterns of levels can yield these self-same residuals should make it clear the "levels" are here our imposition, not adjudicated by the data. What the data can speak to, be-

cause the representation formulates the structure of the model we are fitting, is the representation in Figure 3.

The claim is that "levels" talk reifies features which are not a necessary component of the model, leading the naive reader to misperceive their ontological status. Now, it could well be argued, authors are not to be blamed for the (assumed) behavior of (hypothetical) naive readers. Yet precisely in the context of the table we have been discussing Hauser introduces his "New Mobility Ratio" (Featherman and Hauser, 159; Hauser, b, 429). The New Mobility Ratio, in logarithmic form, is an additive function of the residual and level parameters for each cell. The level parameters are altered by choice of Table 1 or Table 2; the residuals are unaffected. So the New Mobility Ratio is sensitive to choice of Table 1 or Table 2, though both are in fact the same model (Table 3). It would seem that Hauser himself has here been misled, and whilst he clearly recognizes that the New Mobility Ratios are defined "under a given statistical model" (Hauser, a, 939), Tables 1 and 2 are surely not in this sense different statistical models. Further, in attacking the old mobility ratio, Hauser presents an illustrative 3 by 3 table, in which cell (1,1) contains 500 observations and each of the others 100, and remarks: "It is evident by inspection that all of the association in the table is due to the large entry in the (1,1) cell" (b, 925). But this, insofar as it is presented as an argument for seeing the number as being generated by some feature of that cell, is a stipulative definition, not an entailment from the data. Certainly we can see (1,1) as the odd cell; equally we could claim that its observed size is large precisely because of the oddity of the alternative outcomes. Hauser is, in effect, claiming that the pattern of levels in Table 5A is self-evident, but we could obtain identical fitted values from Table 5B, as the implied pattern of  $C_{ij}$  in both (Table 5C) reveals; it is not obvious by inspection that either pattern of levels has primacy.<sup>1</sup> Admittedly it is possible to work with levels without being misled. Goodman (814) who uses the odds-ratio formulation to demonstrate the equivalence of various models does not see that as an argument against the "levels" formulation; but the models he considers are highly regular and, once noted, it is straightforward to spot the equivalent arrangements. The relatively disordered designs which the Hauser approach invites are (when expressed as levels) not similarly transparent.

Hauser having reproduced much of his earlier discussion, and additionally specified his iterative respecification strategy,<sup>2</sup> ends with a brief note of caution:

Any number of models may imply the same set of odds ratios, and in this sense they will be equivalent. One such model may be transformed into another by multiplicative rescaling (Goodman 1979). Thus any model that fits a given classification is going to be equivalent in this sense (or nearly equivalent) to any other model that fits the same classification - for close fit means that they will imply a particular set of odds ratios. In this sense a saturated model of the 5 by 5 British

Table 5.

A. One Pattern of Levels		
1	2	2
2	2	2
2	2	2
B. Another Pattern of Levels		
1	2	2
1	1	1
1	1	1
C. The Corresponding $C_{ij}$ Array		
x	0	
0	0	

mobility table will be equivalent to the rather different specification of Hauser (1978), within the limits of sampling variability (b, 453).

The first two sentences in that passage contain the point of this present note, so I am saying nothing unknown; but, as the preceding discussion has tried to show, it is not clear that the force of the known has been attended to. The second two sentences of the passage then blur the issue by moving from logical equivalence to *de facto* similarity. The match between the saturated model and the Hauser model of the classic British table is not theoretically worrying; we can conceive of data sets which would discriminate between these two models (the saturated model always fits, the Hauser model not always) in a way that we *cannot* conceive of data sets which would discriminate between the models of Table 1 and Table 2. And it is this in principle impossibility which is worrying. Hauser further proceeds to make a recommendation that "it may be necessary to use multiplicative rescaling" (b, 454) to see the equivalence between a pair of models, which seems perverse when the  $C_{ij}$  presentation would instantly reveal the equivalence (and in any case these rescaling algorithms presuppose that we have accepted the  $C_{ij}$  as the pertinent structure; for the transformation makes sense if and only if we can point to some aspect of the table which remains constant).

That having been said, there is one major attenuation of the case against "levels" (which attenuation does not, however, become a case

against the odds-ratio formulation). For models of reasonable complexity, the levels specification may well be unique. Hauser (a, 933) applies the levels specification in Table 6A to the 1949 British mobility table;<sup>3</sup> this yields the  $C_{ij}$  array in Table 6B. Here we have 6 independent contrasts, so we shall need a minimum of 6 levels. Consider first the lower right-hand corner of the  $C_{ij}$  array. The four elements display a diagonal pattern which, considered in isolation, can be generated by any of the arrangements of levels in Table 7 (and note that 7B is a rotation of 7A, as 7D is of 7C, and 7F is of 7E). But to this we must add the constraints imposed by the  $y \ y$  pattern in row 4 and column 4 of Table 6B; these require the levels arrangements in Table 8A and 8B respectively: where the  $c$  levels are unspecified (by the  $y \ y$  pair) and we assume that we wish to use only two levels to map one  $C_{ij}$  contrast value. The two Table 8 constraints are compatible only with Table 7D. We have thus fixed the major portion of the "levels" array, and can proceed in similar vein to demonstrate that the description in Table 6A entails and is entailed by Table 6B.

Another way to ascertain uniqueness of level arrangements would be to ask under what conditions constants can be added to levels and at the same time subtracted from rows or columns to leave the fitted values unchanged. There are at least three such conditions:

- 1. A (row or column) vector carrying only one level;
- 2. A set of vectors carrying  $N$  levels all of which are unique to that set, and

Table 6.\*

A. Levels Formulation				
1	2	4	5	6
2	3	4	5	6
4	4	4	5	5
5	5	5	6	5
6	6	5	5	4
B. $C_{ij}$ Formulation				
v	w	0	0	
w	x	0	y	
0	0	z	y	
0	y	y	z	

\*Model from Hauser (a, 933).

Table 7.

A.			B.			C.			D.		
a	b	c	c	b	a	a	a	c	c	a	a
b	a	b	b	a	b	a	b	a	a	b	a
c	b	a	a	b	c	c	a	a	a	a	c

E.			F.			G.		
a	a	c	c	b	a	a	c	b
b	a	b	a	a	a	c	c	c
c	a	a	a	b	c	b	c	a

Table 8.

A.		
a	c	b
b	c	a

B.	
a	b
c	c
b	a

in which cells (if any) common to two vectors lie at levels not more than one of which occurs elsewhere in the set;  
3. A set of vectors carrying  $N$  levels of which  $N - 1$  are unique to that set, and in which cells (if any) common to two vectors lie at levels which do not occur elsewhere in the set.

While a set may have one member it may also have many; and since the relevant vectors need not be adjacent, the scanning task (to determine that an arrangement fails to satisfy the conditions and so is unique) is not straightforward, and models can be complex and yet fall foul of the conditions. But by (repeated) inspection we can satisfy ourselves that Table 6A is a unique formulation, and it may well be that most models of reasonable complexity avoid the three non-uniqueness conditions.

Thus Goldthorpe's (106-7) eight-level formulation is unique. But, interestingly, his (100) initial seven-level formulation is not. Again, suppose that in Table 6A we had, not unreasonably, assigned the (4,4) cell to level 7. This appears on casual inspection at least as complex as Table 6A; but the resulting  $C_{ij}$  are compatible with several arrangements of levels (for we have, in effect, unlocked the strong  $y = y$  constraint!). And while our fully articulated "levels" models may be determinate, we often do, for reasons of parsimony, consider reduced specifications. For example, Hauser (a,950) notes that "an even simpler model fits almost as well" as that in Table 6A; this model combines level 2 with level 3, and level 5 with level 6 (thus removing the remaining non-zero off-diagonal  $C_{ij}$ ). The two equal  $C_{ij}$  at the lower end of the diagonal still exercise a strong constraint (restricting us to Table 7D or equivalently 7E), but nevertheless the design array can have any level assigned to the fourth row (or alternatively to the fourth column) without affecting the estimated  $C_{ij}$ .

The converse of this would suggest that iterative respecification procedures (starting from simple models and building upwards, attending to patterns of residuals within levels) may be suspect. For simple starting configurations may not be unique representations of their model, and different starting versions of the same model may lead to different destinations. Though notice also that the notion of "simplicity" is not determinate. We have seen that sequences (either diagonal or rectangular) of identical  $C_{ij}$  values exercise strong constraints on "levels" layouts. A simple diagonal effects array larger than 4 by 4 (with levels lying parallel to the main diagonal) is for this reason determinate. (Consider a 5 by 5 levels array, consider the two 3 by 3 diagonal arrays that can be seen within the resulting 4 by 4 array of  $C_{ij}$  values; only the pattern of levels in Table 7A can be used without contradiction to generate them.) This may well be an argument in favor of "diagonal" models; though note that the diagonal pattern is seen as clearly in the  $C_{ij}$  array as it is in the larger "levels" array. As a further footnote on simplicity, notice that a 4 by 4 array of identical (non-zero)  $C_{ij}$  values requires a 5 by 5 array containing 5 separate (though interdependent) sets of levels to capture it.

Indeed, the straightforward unoriginal observation that there are several alternative formulations of the structural model (with perhaps a suggestion that the odds-ratio formulation might be more attended to), is not unproblematic; particularly if there is held to be some virtue in parsimony. We have already seen that the apparent compactness of a "levels" formulation as in equation (1) depends on a complex tabular specification of the location of the levels (which in fully articulated models is often not summarizable—there is no simpler rule to generate the table). Consider further the 1972 British mobility table presented by Goldthorpe (105-8). Table 9A gives the  $C^*_{ij}$  calculated on the observed data, Table 9B gives in schematic form the  $C_{ij}$  array implied by 8 level model. Examining 9B in

conjunction with 9A, it is tempting to suggest that a more compact and equally accurate formulation could be obtained by equivalencing  $e$  and  $f$ ; and why not abolish both  $b$  and  $d$  and replace them by  $2f$ , which, besides being apparently neater, also better fits the observed values? There are other modifications of kindred nature which would make a more compact, closer to the data, pattern of  $C_{ij}$  values (remembering to consider the full set). But these simplifications, if worked through, require many extra levels to represent them. Simplicity in one formulation is lack of simplicity in another. Or again consider the top right-hand corner of Table 9A, which is poorly represented by the model as seen in Table 9B. In particular, it seems a reasonable and straightforward demand that the equivalence of  $C^*_{ij}$  val-

Table 9.

A. $C^*_{ij}$ Calculated on the Raw Goldthorpe (105) Data*					
6	-2	-1	8	0	-4
1	5	0	2	1	3
1	-2	16	-15	3	3
1	1	-14	17	-2	-5
2	2	0	-1	6	0
1	1	0	0	-3	5

B. The $C_{ij}$ Array Implied by Goldthorpe (105-8)					
b	.	-e	c+e	g	.
.	d	.	e	c-e	.
.	.	a+f	-(a+e)	e	.
f	.	-(a+f)	a+e+f	-f	-e
c-f	e	f	-f	2f	e-f
.	.	.	.	-f	2f

The '.' Is Used for  $C_{ij}=0$ , and the Other Parameters Are as Follows:

	(a)	(b)	(c)	(d)	(e)	(f)	(g)
$C_{ij}$	1.30	.58	.45	.40	.31	.30	.04

\*Values are multiplied by 10 and rounded.

ues seen in the (2,6), (3,5), (3,6) cells be reproduced by the model; but such a triangular arrangement of identical  $C_{ij}$  values requires more than a pair of "levels" to specify it. So again a request for a simple model on one representation yields a complex model on another; a further illustration of the philosophical commonplace that the concepts of parsimony, simplicity and their kin are seldom well defined.

The discussion in this note connects to three others. First, Holt has pointed out in the context of loglinear models what is (or should be) obvious to any user of dummy variables in a regression context, namely, that the parameterization of the fully saturated model is indeterminate. If we allow each cell of the table its peculiar interaction effect,  $w_{ij}$ , there are many different patterns of  $w_{ij}$  values which, with appropriate  $a_i$  and  $b_j$ , yield the observed values: and since these patterns need bear no apparent patterned relations to each other, there is no way of noting, by simple inspection of the  $w_{ij}$ , their formal resemblance. Hope makes a similar point and uses it to emphasize, in the context of a model which exactly fits the data, the arbitrary nature of the "New Mobility Ratio." The argument can be extended by treating the fitted values,  $F_{ij}$ , from the structural model as if observed values, fitting them exactly by:

$$F_{ij} = m + a_i + b_j + w_{ij} \quad (4)$$

and noting that the pattern of  $w_{ij}$  (and hence of the "New Mobility Ratio") need bear no systematic resemblance to the pattern of  $v_k$  which generated the  $F_{ij}$  in equation (1). The obvious defense of the specific levels of the structural model against charges made by way of equation (4), is that there are fewer of them than of the  $w_{ij}$ ; the parameters of the more compact model have some claim to substantive acceptance. It was to avoid this parsimony argument that this present note focussed on instances where parsimony was no aid.

Second, on the matter of indeterminacy of scaling in the saturated model, it has been argued against Holt that: "Use of a particular way of coding the categories in fact reflects the researcher's ideas about the nature of the underlying process and the mechanisms by which the variables affect one another" (Wilson, 236). This would be a claim that choice of coefficients was not arbitrary. A similar ground would underly the old attempts (Boyle; Lyons) to attach disparate imports to equivalent (Macdonald) dummy variable representations of categorical variables, and the argument could presumably be extended to defend choice of levels; as Hauser remarks: "the way in which one chooses to model a given set of odds ratios is not arbitrary" (b, 454). But, as we have argued, prediction equations (or, if you will, inference rules) which must always yield identical decisions are not substantively different, and to represent such rules in ways which mask this identity is at best unhelpful. Notice that inferential equivalence separates this issue from the old one of selection amongst a



range of structural models, any one of which would of course be compatible with the data; for these models are distinguishable in that different models yield different sets of fitted values.

Finally, the  $C_{ij}$  formulation of equation (2) can be used helpfully to represent interaction effects in additive regression models involving categorical variables represented by sets of dummy variables. Typically users of regression aware of the indeterminacy of parameter estimation have restricted attention to increments in explained variance on inclusion of interaction effects. But for variables where some ordinal presentation is not absurd, the parameters of a  $C_{ij}$  presentation are invariant across arbitrary choice of estimation constraint; and the presentation helps us to see which "levels" contrasts are entailed by the fitted values and which are optional. The suggestion would be that we are less obliged to attend to the optional ones.

In conclusion, it may be worth again stressing that this is not a hostile critical note; there is clearly much merit in the Hauser approach. The note has merely expressed some caution and skepticism about some of the statements which have been associated with the model. As in other contexts (such as path analysis) it may be important to distinguish what we are required to say by the model (having imposed it) and what we may choose to say alongside the model. The critical reader will note that at various points the present discussion has come dangerously close to suggesting that the structural model attempts to reproduce the observed pattern of odds-ratios; it does not. But that attempt would sound substantively plausible; and perhaps we should discuss whether the quantities the classical loglinear model seeks to minimize are the quantities that substantively we might wish to minimize; for such questions are not determined, though they are influenced, by the statistical properties of estimators.

## Notes

1 Notice that this argument can be extended to defend the traditional mobility ratios which Hauser dismisses; if we see the overrepresentation in (1,1) as in part caused by underrepresentation in (1,2) and (1,3), then the pattern of highest affinity between origins 2 and 3 and destinations 2 and 3 is not unreasonable. Hauser's remark that "the mobility ratios distort the pattern of association which was evident from inspection" (a,925) of the table requires prior acceptance of the claim that the (1,1) cell is doing the work, and it is precisely that claim which is in dispute. Patterns of association are facts, not data.

2. Throughout, Hauser's computation strategy for estimating the parameters of his model is needlessly complex "I have represented cross-classifications as incomplete multiway arrays. I have used . . . ECTA to estimate frequencies by iterative rescaling . . . I have estimated the parameters by regressing logs of estimated frequencies on dummy-variable representations . . . I use an auxiliary program to renormalize the parameter estimates" (b, 417-8). The central algorithm implied is very simple, involving iterative fitting to row, column and level means, and can be written compactly (by way of illustration, a table-top 8-bit microcomputer running a Basic interpreter can estimate all the parameters of the model in Table 1 in less than 2.5 minutes elapsed time). At mainframe speeds this enables hill-climbing search strategies for "best-fitting" models. Hauser describes the model in Table 1 as "the outcome of a search procedure" (b, 420); J. M. Ridge and the present author, taking Hauser's (b, 430) suggestion of a

"quasi-perfect mobility model" as a starting "seed" and using a short-sighted hill-climbing algorithm have located the following alternative five-level model of transition from father's occupation to son's first occupation:

2	3	4	5	5
3	3	5	5	5
4	4	4	5	5
5	4	5	4	4
5	5	5	4	1

This yields a  $G^2$  of 18.5 (and there may well be tighter models) which contrasts favorably with the  $G^2$  of 66.5 obtained by Hauser (b, 423). We are of course aware of the familiar dangers in mindless data-fitting, but such exercises can have occasional suggestive value as one extreme strategy of model choice. Listings of an estimation program in Basic (for microcomputers) and an estimation and search program in Fortran-77 (currently running on a DEC-VAX/11 machine) are available on request from the author.

3. Notice that for any "levels" model which assigns a unique level to a particular cell (as Hauser, a, 933; or Goldthorpe, 100) only the "level" parameter for that cell is affected by changes in the observed value for that cell (and a model with a structural zero in that cell would give the same result); in particular both the row and column effects are unaffected by the number of respondents making that particular transition. This independence from these particular observed values fits ill with Hauser's suggested translation: "The row and column parameters represent conditions of supply and demand; they reflect demographic replacement processes and past and present technologies and economic conditions" (a, 930). Applying that translation to Table 1 would yield a supply and demand estimate for inflow into "upper nonmanual" first jobs which disattended completely to the sons of nonmanual workers (the numbers in cells 1,1 and 2,1).

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# Hope for the Mobility Ratio\*

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## ABSTRACT

*Macdonald's criticism of structural models of mobility is empty because it denies ideas or theories a place in model construction. Specifically, the equivalent models proposed by Macdonald do not meet the criterion of minimal asymmetry that was applied in the selection of models for British and American mobility tables. Under certain conditions, equivalent models meet the symmetry criterion, but these conditions do not apply to the examples discussed by Macdonald. Also, he exaggerates the usefulness of odds-ratios in formulating models for non-ordered data, and he proposes an empirical revision of the Featherman-Hauser model of mobility to first occupations that is less appealing than the original model. Hope's criticism of the new mobility ratio rests on an illogical comparison between the Featherman-Hauser model and various saturated models of the mobility table. Also, his commentary makes erroneous statements about the relationship between the Featherman-Hauser model and their criticisms of Blau and Duncan and about the relationship between that model and Goodman's quasi-independence models.*

Macdonald's commentary on structural models of the mobility table is both instructive and cautionary, but I disagree with much of it. I wrote some time ago that "Any number of models may imply the same set of odds-ratios, and in this sense they will be equivalent. One such model may be transformed into another by multiplicative rescaling" (b, 453). Macdonald cites this passage as the point of his note, but he questions whether "the force of the known has been attended to." I believe it has, but Macdonald ignores relevant features of my several published analyses of occupational mobility classifications. In this note I will show how Macdonald's criticism

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is irrelevant to my analyses of mobility classifications. I will discuss conditions under which a modified form of Macdonald's criticism might be made, and I will argue that the criticism is empty in both its original and modified forms. Macdonald also fits a revised model to the Featherman-Hauser (F-H) table of mobility of American men from father's occupation to son's first occupation, and I will show how that model is less appealing than the original F-H model.

### Macdonald's Critique

Macdonald is correct that the model of his Table 2A yields the same expected frequencies with the same degrees of freedom as the model of his Table 1A, taken from Featherman and Hauser (151, 155).<sup>1</sup> Moreover, there are non-trivial differences between the column and level parameters of the two models; that is, the differences are in no way due to a change in normalization of the parameters. Macdonald consistently maintains that a model consists of its expected frequencies—with allowance for the number of fitted parameters—and on this account, he argues, the equivalence between the models of Tables 1A and 2A renders suspect any interpretation of the former (or, I assume, of the latter).

If there were no other bases for model selection, I would agree with Macdonald, but I believe there are other reasons to prefer the F-H model of Table 1A to that of Table 2A. That is, Macdonald bases his refusal to choose between models on excessively limited criteria. His criteria are peculiarly narrow relative to those applied by most social scientists in modeling and interpreting data, and—more concretely—they ignore an explicit criterion for model selection used in Featherman and Hauser and Hauser (b). Throughout these analyses, I emphasized the importance of symmetry in the array of interaction parameters, pointing both to its substantive implications for the correlation between occupational supply and recruitment and to its heuristic value in the specification of parsimonious models (Featherman and Hauser, 184–7; Hauser, b, 437). Yet Macdonald never refers to this form of symmetry. If one applies this criterion to the models of Table 1A and Table 2A, the choice between the two becomes obvious: there is 1 asymmetry in the former case, but there are 7 in the latter.<sup>2</sup>

There is good reason to introduce the asymmetry between parameters of cells (2,1) and (1,2), but no others, in modeling the 1973 classification of mobility to first jobs. Under the model of quasi-symmetry in the full classification, the likelihood-ratio test statistic is 27.61 with 6 degrees of freedom.<sup>3</sup> If we permit asymmetry between the interactions in cells (2,1) and (1,2), but no other pairs of cells, the test statistic falls to the statistically insignificant value of 7.98 with 5 degrees of freedom. There are 10 pairs of cells in which one might introduce a single asymmetry between interac-

tions, but (2,1) and (1,2) are the only pair where the introduction of asymmetry leaves a nonsignificant residue. Thus, unlike the model of Table 2A, the F-H model of Table 1A is parsimonious in introducing the single asymmetry in interactions that is both necessary and sufficient to interpret the data.

In a similar vein, Macdonald argues that an illustrative 3 by 3 table, constructed with 500 observations in cell (1,1) and 100 observations in each of the other 8 cells, can equivalently be modeled so as to locate interaction effects in other cells than the (1,1) cell, where I located the interaction by inspection. Imagine that you are analyzing this hypothetical table 9 times, but at each turn a different one of the 9 cell entries is unknown to you. When the contents of cell (1,1) are unknown, and only then, will you conclude that there is no association in the remainder of the table. That is the sense in which it is obvious by inspection that the model of Macdonald's Table 5A is to be preferred to the model of his Table 5B. Note that the latter model is asymmetric and, further, that it requires us to locate the interactions in two cells, rather than in one cell of the classification.

#### A MODIFIED CRITIQUE

Under certain conditions there are equivalent models which satisfy my criterion of symmetry as well as the criteria used by Macdonald, but his note fails to specify those conditions. Further, I believe that those conditions do not hold in the case of my models of American or British mobility classifications. To illustrate this, consider the display in Table 1. Model A gives the locations of 7 interaction parameters in a hypothetical model. Note that the parameters are symmetric about the main diagonal, that parameters 1, 2, and 3 appear only in the intersection of rows 1, 2, and 3 with columns 1, 2, and 3, that parameters 4 and 5 appear only in the intersection of rows 4 and 5 with columns 4 and 5, and that parameters 6 and 7 appear only in the intersection of rows 4 and 5 with columns 1, 2, and 3 or in the intersection of columns 4 and 5 with rows 1, 2, and 3. Under these conditions I can write an equivalent model by multiplying every entry in rows 4 and 5 and in columns 4 and 5 by any non-zero scalar. Model B of Table 1 is such an equivalent model.<sup>4</sup> Moreover, Model B has the same number of parameters (7) as Model A, and those parameters are located symmetrically about the main diagonal of the table. By an appropriate choice of scalar multipliers, it is possible to reduce the number of parameters in the equivalent model. For example, in Model C I have set the parameter of cells (4,4), (4,5) and (5,4) equal to the parameter of cells (1,2), (2,2) and (2,1), thus reducing the number of parameters in the model from 7 to 6. Similarly, one could set the value of any one parameter in rows or columns 4 or 5 of Model A to be equal to the value of any other parameter that occurs outside of rows and columns 4 and 5. The resulting specifica-

Table 1. PARAMETERS OF SOME HYPOTHETICAL STRUCTURAL MODELS OF A SQUARE TABLE

Row	Column				
	1	2	3	4	5
Model A					
1	$\delta_1$	$\delta_2$	$\delta_3$	$\delta_6$	$\delta_7$
2	$\delta_2$	$\delta_2$	$\delta_3$	$\delta_6$	$\delta_6$
3	$\delta_3$	$\delta_3$	$\delta_3$	$\delta_6$	$\delta_6$
4	$\delta_6$	$\delta_6$	$\delta_6$	$\delta_5$	$\delta_5$
5	$\delta_7$	$\delta_6$	$\delta_6$	$\delta_5$	$\delta_4$
Model B					
1	$\delta_1$	$\delta_2$	$\delta_3$	$\lambda\delta_6$	$\lambda\delta_7$
2	$\delta_2$	$\delta_2$	$\delta_3$	$\lambda\delta_6$	$\lambda\delta_6$
3	$\delta_3$	$\delta_3$	$\delta_3$	$\lambda\delta_6$	$\lambda\delta_6$
4	$\lambda\delta_6$	$\lambda\delta_6$	$\lambda\delta_6$	$\lambda^2\delta_5$	$\lambda^2\delta_5$
5	$\lambda\delta_7$	$\lambda\delta_6$	$\lambda\delta_6$	$\lambda^2\delta_5$	$\lambda^2\delta_4$
Model C					
1	$\delta_1$	$\delta_2$	$\delta_3$	$\delta_6(\delta_2/\delta_5)^{1/2}$	$\delta_7(\delta_2/\delta_5)^{1/2}$
2	$\delta_2$	$\delta_2$	$\delta_3$	$\delta_6(\delta_2/\delta_5)^{1/2}$	$\delta_6(\delta_2/\delta_5)^{1/2}$
3	$\delta_3$	$\delta_3$	$\delta_3$	$\delta_6(\delta_2/\delta_5)^{1/2}$	$\delta_6(\delta_2/\delta_5)^{1/2}$
4	$\delta_6(\delta_2/\delta_5)^{1/2}$	$\delta_6(\delta_2/\delta_5)^{1/2}$	$\delta_6(\delta_2/\delta_5)^{1/2}$	$\delta_2$	$\delta_2$
5	$\delta_7(\delta_2/\delta_5)^{1/2}$	$\delta_6(\delta_2/\delta_5)^{1/2}$	$\delta_6(\delta_2/\delta_5)^{1/2}$	$\delta_2$	$\frac{\delta_4\delta_2}{\delta_5}$

tion would remain symmetric, and it would require one less parameter than Model A. Surely this has cautionary implications, but so far as I can tell they do not apply to my analyses of British or American mobility classifications. That is, I do not believe it is possible to generate equivalent reparameterizations of any of my models of British or of American mobility classifications (within the levels framework) without introducing more asymmetries or increasing the number of parameters or both.<sup>5</sup> The reason is that the parameters of my models are not separated between aggregates of rows and columns as are those of Model A in Table 1. For example, rows and columns 4 and 5 of Macdonald's Table 1A may be rescaled symmetrically to set parameter 5 equal to parameters 4, 3 or 2; to set parameter 4 equal to parameters 5, 3 or 2; or to set parameter 1 equal to parameters 2, 3,

4 or 5. All of these models will be equivalent, but each also adds one new parameter to the F-H model of Table 1A.

Let me summarize the discussion to this point. Macdonald is correct in noting the existence of equivalent models that use the same number of parameters as some of my models of mobility tables. At the same time, his proposed equivalent models are unappealing because they introduce needless asymmetries into the arrays of interaction parameters. Under certain conditions, one can find equivalent models that are both parsimonious and symmetric, but these conditions do not hold in my models of mobility classifications.

#### STRUCTURE AND MODELS

Up to this point, I have worried along with Macdonald about the possibility of specifying equivalent models, but the fact is that I do not share his concern about the matter. I do not believe that a model consists only of a set of expected values, but it also (and mainly) consists of the structure or story that we use to interpret and explain those expected values. It is all well and good when a model has no equivalents, that is, when it carries unique implications for population data, but that is rare indeed in the social sciences. Most of the time, a model is no more than a vehicle for rendering a complete and internally consistent interpretation of a body of data in light of the ideas we draw from observation, theory, convention, or whatever. Sometimes, the model will illuminate a facet of a phenomenon in a way that we had not anticipated. Sometimes, the fit or findings of one model will lead us to reject other models. In the usual course of social scientific modeling, we are rarely in a position to regard the fit of one model to the data as grounds for rejecting all other models, but this is the standard against which Macdonald seeks to measure structural models of mobility. We can (and do) learn a great deal from models that fall far short of this goal, and I see no need to propose a special standard for mobility models.

Let me illustrate. Fitting a mobility classification is like fitting a variance-covariance matrix; indeed, there is more than analogy here. When we fit a variance-covariance matrix—say, with a regression analysis, a path model, a factor model, or whatever—do we throw up our hands at the thought that the same population moments might be implied by other than our preferred model? After all, when we regress  $Z$  on  $X$  and  $Y$ , that implies the same set of moments as the regression of  $X$  on  $Y$  and  $Z$  or the regression of  $Y$  on  $X$  and  $Z$ . No, we usually have some reason to think that  $X$  and  $Y$  cause  $Z$  and not otherwise, or we want to predict  $Z$  and not  $X$  or  $Y$ . We set up the model the way we think things work, and we interpret the results in light of our presuppositions. We report the coefficients of the preferred model, not the moments estimated under it. We question the



model if it yields parameter estimates that strain belief. We impose overidentifying restrictions on the data, and violations of these may lead us to reject the model. For example, we may say that  $X$  affects  $Z$  through  $Y$  in a zero-order causal chain, and we cannot maintain that model in the face of a non-zero partial relationship between  $X$  and  $Z$ . Yet other models imply the same zero partial. Our test treats only a necessary condition, but not a sufficient condition of the model. The same holds when we model a mobility classification. When we specify a simple model of a complex table, the degrees of freedom for error reflect the overidentifying restrictions of the model. We know that we can reject the model if we do not fit those restrictions within the limits of sampling error, but we do not know that the model is true when the restrictions are met. At that point, we have to decide whether we have reason enough to believe what the model says about the expected counts.

The real question, then, about the structural models of British and American mobility tables, is whether they tell a story worth hearing. In light of the fact that I used exploratory methods to specify those models, are they so free of content that there is no reason to prefer any of them to any equally parsimonious alternative? I think not. While I explored the data to specify the models, my procedures were neither mechanical nor unmotivated.<sup>6</sup> As discussed in Hauser (b, 430–44), I showed how models of mobility from father's occupation to son's first occupation and from son's first occupation to son's current occupation could be developed from such simple ideas as the clustering of observations on or near the main diagonal, the randomness of destinations in long-distance mobility, and—as previously elaborated—the substantial equivalence of propensities toward movement in either direction between each pair of occupations. These are the ideas expressed by the models and elaborated through successive revisions of them. Of course, in modifying the models to fit the data, I ran the risk of overfitting. Ultimately, the only protection against that is cross-validation (see below), but I also used various smoothing techniques in an effort to reduce the risk of overfitting the data. Minor differences in specification, on the order of those between the F-H model and that of MacDonald's Note 2, are about as consequential for interpretation as those ordinarily entailed by the addition or deletion of one or another coefficient in a system of recursive structural equations. Regardless of the uniqueness of the specifications, I believe that the models are of interest to the degree that the reader wants to pursue the implications of the ideas they express, and no further. If the reader has other ideas, other models may be of greater interest.

## LEVELS AND ODDS

Macdonald argues that odds-ratios for adjacent rows and columns are more useful than parameters for individual cells because they are more compact and because they are unique (conditional upon an ordering of the categories). He uses such displays skillfully to elucidate the conditions under which equivalent models may or may not exist. I am reluctant to express my misgivings about the use of odds-ratios for adjacent rows and columns, for as Goodman (b) has shown, they are extremely useful when one posits or wishes to posit an ordering in one or both criteria of classification.<sup>7</sup> At the same time I believe that Macdonald overstates the usefulness of the array of  $C(i,j)$ —logs of odds-ratios—in the present context. He writes, "And whilst choice of a particular  $C(i,j)$  array involves an ordinality decision it is not one fraught with substantive import, for no claims about groupings of cells rest upon it, on changing it we are not led to unsay things previously said, and the alternatives are self-evidently equivalent." Macdonald appears to have missed one of the central features of my analysis, namely, that I was trying to avoid metric assumptions about occupations. In working with occupational data, I constantly choose between two forms of criticism. If I do not adopt a metric of occupational social standing, I am told that I have missed the central feature of the occupational structure, that it is a hierarchy.<sup>8</sup> If I do assume a metric, I am told that I have picked the wrong one. The two most common forms of the latter criticism are, first, that upper manual workers are higher in status than lower non-manual workers and, second, that farmers are higher in status than lower manual workers. In Table 2 I have permuted the rows and columns of the display in Macdonald's Table 1A to reflect these two inversions, and in Table 3 I have displayed the  $C(i,j)$  for adjacent rows and columns of the permutation, using the notation of Macdonald's Table 3A wherever the same terms appear. It now takes six distinct terms (including zero) to describe the interaction in the classification, and the display of the  $C(i,j)$  contains two asymmetries. I do not believe that this display is "self-evidently equivalent" to that of Macdonald's Table 3(a), but the display in Table 2 is self-evidently equivalent to that of Macdonald's Table 1(a). The moral is that it pays to be agnostic and flexible in deciding how to display the features of a model.

## SEARCH STRATEGIES

In his Note 2 Macdonald argues that my computational strategies were needlessly complex. He is right, and in the passage that he cites, I recommended other strategies (Hauser, b, 418). I can only wish that I had the computational facilities four and a half years ago—when I did the work that Macdonald criticizes—that either Macdonald or I have now. It is not

**Table 2.** A PERMUTATION OF ROWS AND COLUMNS IN THE MODEL OF MACDONALD'S TABLE 1(A)

Father's Occupation	Son's Occupation				
	(1)	(3)	(2)	(5)	(4)
1. Upper nonmanual	2	5	4	5	5
3. Upper manual	5	5	5	5	5
2. Lower nonmanual	3	5	4	5	5
5. Farm	5	5	5	1	4
4. Lower manual	5	5	5	4	4

**Table 3.** ODDS-RATIOS FOR ADJACENT ROWS AND COLUMNS IN THE MODEL OF TABLE 2

Upper row	Left-hand column			
	(1)	(3)	(2)	(5)
1.	a	-x	x	0
3.	b	x	-x	0
2.	-b	-x	c	-z
5	0	0	-z	z

news that one can improve the fit of the F-H model in Macdonald's Table 1A; just this possibility is discussed in the source (Hauser, b, 425-6). It is also not news that mechanical search procedures can be used to improve fit; I implemented such an option, but found it unsatisfactory in comparison with a study of residuals at each round of specification (Hauser, a, 949; b, 441-3). Macdonald notes "the familiar dangers in mindless data-fitting," and I want to illustrate these by a comparison between the model of his Note 2 and the F-H model of his Table 1A. First, as shown above, the asymmetry between interactions in cells (2,1) and (1,2) is extremely important. Macdonald's revision of the model specifies two other asymmetries (between cells (3,2) and (2,3) and between cells (4,2) and (2,4)), but the fit of his model is still marginally improved by the introduction of a parameter for cell (2,1). (The test statistic declines by 3.6 with 1 degree of freedom.) Second, the asymmetry between interactions in cells (2,1) and (1,2) appears also in the table of mobility to first jobs in 1962. That is, in both the 1962 and the 1973 data, that asymmetry is larger than any other single asymmetry, and the remaining departures from quasi-symmetry are non-significant. The 1962 data on mobility to first jobs were not used to develop the model of the 1973 data, but they were used to cross-validate that model with

some success (Hauser, b, 450–2; Featherman and Hauser, 205–6). Third, when Macdonald's revised model is fitted to the aggregate table of mobility from father's occupation to son's first occupation in the 1962 data, the fit is inferior to that obtained when the F-H model of Table 1A is fitted to the same data; the test statistics are 64.6 and 38.2, respectively, in comparison with a test statistic of 4,334 under simple independence. At the same time, the performance of the two models is very similar in a second cross-validation, carried out on a sample of 4,017 male Wisconsin high school graduates who were interviewed in 1975 (Sewell and Hauser).<sup>9</sup> The test statistics are 18.7 and 20.8, respectively, in comparison with a test statistic of 838.4 under simple independence; neither model can be rejected at the .05 level. In sum, I do not believe that the model of Macdonald's Note 2 constitutes any improvement over the F-H model of his Table 1A, but this is by no means to suggest that no improvement in the latter is possible.

### Hope's Critique

Hope's commentary on mobility ratios calls for a response because of its strong and categorical assertions, "that values of the ratio are highly dependent on the model fitted to account for interaction, and that criticisms of Blau and Duncan's conclusions about the mobility of American society, which derive from application of the new ratio, cannot be sustained." Those assertions are wrong.<sup>10</sup>

The first assertion—model dependence—is pointless as criticism because it is half right and half wrong. The right half expresses the main virtue of the mobility ratio; net of the error component, it is the interaction parameter of a multiplicative model (Hauser, b, 427).<sup>11</sup> The other half of Hope's first assertion is wrong because there is nothing arbitrary or indeterminate about the choice of the model that determines the ratios. That is, unless one denies the virtues of parsimony or of theory, the second half of Hope's first assertion is meaningless. It is absurd to claim that the mobility ratio is arbitrary because it differs from parameters of any number of saturated models. My models are not saturated models, and they are empirically distinguishable from saturated models by dint of fit and parsimony. As Macdonald writes, "the saturated model always fits, the Hauser model not always." Hope's comparison of mobility ratios to parameters of the saturated model is so poorly conceived that I am left to wonder whether his commentary was an ineffectual effort to anticipate Macdonald's argument, to which I have responded above.<sup>12</sup> It would be ironic indeed if Hope were trying to produce the sort of equivalent reparameterization discussed by Macdonald, for exactly the same equivalence holds between Hope's "diamond" model of the effects of mobility and status inconsis-

tency and the square additive model that Hope (a) was criticizing. When House took note of this, Hope (b) came out foursquare for theory.

Hope's second assertion is "that criticisms of Blau and Duncan's conclusions about the mobility of American society, which derive from application of the new ratio, cannot be sustained." This is twice wrong. First, it is wrong because Hope's first assertion is wrong. Second, it is wrong because those criticisms of Blau and Duncan can be sustained without application of the new mobility ratio. I think it is important to elaborate the latter point because of its substantive implications.

Hope refers in his commentary to two of Blau and Duncan's conclusions with which Featherman and Hauser disagreed. First, he writes, "it is held that the new ratio shows that American society is a good deal less mobile at the top and bottom than was suggested by Blau and Duncan on the basis of the old mobility ratio." Consider the 1973 OCG table of mobility from father's occupation to son's first occupation, analyzed by Hope and by Macdonald, as well as by Featherman and Hauser and Hauser (b). Under simple statistical independence, the (likelihood-ratio) chi-square statistic for this table is 6167.69 with 16 degrees of freedom; the immobility ratios in order, from cell (1,1) to cell (5,5), are 2.35, 1.56, 1.59, 1.39, and 3.46. Under the model of quasi-perfect mobility, the test statistic falls to 683.06 with 11 degrees of freedom. While quasi-perfect mobility does not fit the data, the fit is far better than in the case of simple independence. Thus, the diagonal parameters of this model—described by Hope as "exemplary"—provide a far better basis for interpretation of the data than do the immobility ratios under simple independence. The diagonal parameters under quasi-perfect mobility are 3.80, 1.35, 1.48, 1.63, and 18.13.<sup>13</sup> Note that these values display exactly the same property, relative to the diagonal ratios under simple independence, as do the parameters of the model preferred by Featherman and Hauser. That is, they show greater immobility in the top and bottom categories than do the old mobility ratios. Hauser (b, 430–5) displays other quasi-independence models that lead to the same result.

Hope also cites a passage in which Featherman and Hauser (178) criticize Blau and Duncan (58–9) for identifying asymmetric flows of men with intrinsic barriers to exchange between occupations. While Featherman and Hauser do use their structural models of mobility to elucidate this criticism (152, 169–73, 175), they also use the model of quasi-symmetry to the same end (184–7). The model of quasi-symmetry merely constrains the interactions in the mobility classification to be invariant with respect to transposition of the classification. It has no necessary connection with the structural models developed by Featherman and Hauser or with the new mobility ratio.

A few other points in Hope's commentary warrant brief mention.

His effort to draw distinctions between Goodman's (a) models of quasi-independence and my models of mobility tables is confused. Hauser (b, 430–5) elaborates the relationship between those models. Hope writes that one cannot assess fit in the F-H models as in the Goodman models, and this is incorrect. Hauser (c) discusses methods for assessing fit at each level of the model.<sup>14</sup> Hope's confusion rests in part on what he terms the "two-stage character of the quasi-independence model," which is modified in the F-H models. While this property is irrelevant to an evaluation of the F-H models, one can almost reproduce the F-H model of mobility to first jobs in 1973 using the "two-stage" notion. Fit a model of quasi-independence to the cells of the classification designated at level 5 in the F-H model of Table 1A, and ignore the remaining 8 cells (in the upper left and lower right corners of the table).<sup>15</sup> The only difference between this model and that of Featherman and Hauser is the equality constraint linking the parameters of cells (1,2), (2,2), (4,4), (4,5), and (5,4), each of which is designated to lie at level 4 of their model. The mobility ratios under this quasi-independence model are substantially equivalent (up to a normalizing scale factor) to the parameters of the F-H model.

I do agree with two things in Hope's commentary. First, Hope demonstrates nicely (in Tables 2, 3, and 4) that the parameters of the F-H model are only renormalized by shifts in the reference point of row, column, or interaction effects, whereas parameters of saturated models are drastically altered by such shifts (Table 5).<sup>16</sup> Readers of Featherman and Hauser will note their efforts to point out the invariance of the model with respect to such changes in normalization (156) and their use of two normalizations, one of which has a probability interpretation (153). Second, I agree fully with Hope that in the case of "any aspect of the model which is identified with a single degree of freedom . . . it makes sense to report the value of the parameter which accounts for that degree of freedom." This is exactly the case with each of the parameters of the F-H model.

## Notes

1. Macdonald creates the equivalent model (in his additive formulation) by adding the difference between interaction parameters 2 and 5 to column 3 and adding the difference between interaction parameters 2 and 4 to columns 2, 4, and 5.

2. In the F-H model of Table 1A the single asymmetry holds between cells (2,1) and (1,2). In the model of Table 2A there are asymmetries between cells (2,1) and (1,2), (3,1) and (1,3), (3,2) and (2,3), (4,1) and (1,4), (5,1) and (1,5), (4,3) and (3,4), and (5,3) and (3,5).

3. For further discussion of the model of quasi-symmetry, see Hauser (b, 437) or Bishop et al. (286–90).

4. Without loss of generality, I have suppressed the renormalization of parameters in Models B and C of Table 1.

5. I leave tests of this claim as an exercise for the reader. It is possible to relax the condition illustrated in Table 1 without increasing the number of parameters in the equivalent model, but even the relaxed conditions are not met by any of my models of British or of American mobility classifications. This claim does not apply to other classes of models; Seppo Pöntinen has suggested a parsimonious and equivalent crossings model of the American table that can be written with one asymmetry.

6. At the same time it is satisfying to see that Macdonald's completely mechanical fitting procedure leads from quasi-perfect mobility to a model that closely resembles the F-H model of mobility to first jobs in 1973. See Macdonald's Note 2 and further discussion of it below.
7. The reader should bear in mind that Macdonald uses  $F(i,j)$  to denote the natural logarithm of a frequency, while Goodman uses the same symbol for the frequency itself. Also, there is a minor inconsistency between the normalization of the  $C(i,j)$  in Macdonald's equation 3 and the presentation of them in his Table 3.
8. Note that roughly two-thirds of Featherman and Hauser uses a standard metric of occupational socioeconomic status.
9. The supplementary tables are available from the author on request.
10. I shall not discuss Hope's perfunctory effort to rehabilitate the old mobility ratio, nor do I think this is the appropriate occasion to discuss my reservations about the scheme for mobility analysis that Hope outlines in the latter portion of his commentary.
11. I think the discussion of mobility ratios in Hauser (b) and Hauser (c) is better than that in Hauser (a) or Featherman and Hauser. I erred in earlier work in emphasizing the use of the mobility ratio in interpretation as well as in model specification. I do not believe that anything in Hope's commentary or in my response to it would be altered if it were taken to refer to interaction parameters—as Macdonald's note does—rather than to mobility ratios.
12. Hope's commentary was accepted by *Social Forces* before Macdonald's was submitted. Note that Macdonald neatly undermines Hope's comparison of the F-H model with saturated models; see the discussion surrounding Macdonald's equation 4. Halliman Winsborough made a similar observation to Hope in a seminar at Madison.
13. These results are taken from Hauser (b, 429–35), but the diagonal parameters under quasi-perfect mobility may also be reproduced from Hope's Table 2.
14. Hauser (b) was published nearly a year before Hope's commentary was submitted to *Social Forces*, and a draft of Hauser (c) was given to him before his commentary was submitted to *Social Forces*.
15. Note this quasi-independence model differs in only the (3,3) cell from the specification of Model Q2 in Hauser (b, 431).
16. Featherman and Hauser (171–3) and Hauser (c) discuss other disadvantages of a conventional, row by column parameterization of the saturated model.

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# The Danger of Committing the Ecological Fallacy Persists: Comment on Gove and Hughes\*

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Gove and Hughes claim to have used methods that permit valid inferences concerning individual level causation from aggregate data without committing the ecological fallacy. Aggregate data consistently show correlations between the percentage of people in a community who live alone and the rates of suicide and mortality related to alcoholism in the community. Gove and Hughes manipulate these data in an attempt to eliminate all individual level causal possibilities except that increased incidence of suicide which occurs among those who live alone.<sup>1</sup> But as they acknowledge, their program of evaluating mathematical models and statistically controlling variables is inconclusive. The possibility remains that suicide is not caused by living alone, but rather by living in a social environment with others who live alone. These two possibilities are indistinguishable using aggregate data.

Gove and Hughes recognize that this possibility remains after their statistical manipulations, but reject it summarily.

The question then becomes, *after these controls how probable is it that as the proportion of persons who live alone increases, the suicide rate of those who live with others substantially increases?* There is no reason in the literature to expect such a relationship. We have not been able to think of one, nor has anyone who has read earlier versions of this paper (1166).

This summary rejection is unwarranted and the methodological and substantive conclusions based on it are invalid.

In fact, the literature does suggest the possibility of such a causal relationship. Blau gathered and analyzed data from a public assistance agency and reported that three variables—values, cohesiveness, and the nature of communication networks—exhibit arrangements that are homologous to the one dismissed by Gove and Hughes. Blau called these

\*Gove, Walter, and Michael Hughes. 1980. "Reexamining the Ecological Fallacy." *Social Forces* 58(June):1157-77. This commentary was submitted in July 1980.

arrangements structural effects. In general, structural effects exist when the frequency distribution of categories of a variable has effects on individual conduct that are independent of the category in which the individuals belong. For example, Blau found that the distribution of values in an agency affected conduct independently of individuals' own values.

Blau's study establishes that structural effects are not merely a mathematical possibility, but occur in nature. The possibility that they may occur in other situations cannot be ignored. But if the probability of their occurrence is suitably low, we might choose to reject the hypothesis of their presence in particular instances. The literature on suicide does not indicate the presence of structural effects. Does this imply that the probability of their occurrence is low? Unfortunately it does not. As Gove and Hughes point out, satisfactory individual level data on suicide are not available. The exhibition of structural effects requires both aggregate and individual data. Consequently, the literature could not indicate the presence of structural effects. We can draw no inferences about structural effects in the causation of suicide from their absence in the literature because their demonstration is precluded by the data employed.

An estimate of the probability of structural effects could also be based on research in other substantive areas. In the 20 years since Blau established the presence of structural effects, sociologists have not modified research design or theory to accommodate them. Again, the failure of the literature to indicate the presence of structural effects is an artifact of sociological research practices and does not imply anything about their occurrence. So long as sociologists neglect to look for phenomena that are already known to occur, the failure of the literature to mention them has no substantive implications.

Gove and Hughes clearly acknowledge the mathematical possibility of structural effects, although not by name. But they are apparently unaware of Blau's empirical observations. Because structural effects have been observed to occur but have not been systematically studied their absence in the literature must be considered artifactual. Two specific points must be stressed with respect to inferring that a phenomenon does not exist from one's inability to find it in the literature. First, in a field that is as highly specialized as sociology, we must be very wary of claims that the literature omits some topic. None of us knows the entire literature. This is primarily a challenge for editorial boards and referees who must provide expertise in areas other than authors'. Second, we must be certain that the omission of a topic in the literature is not artifactual. Two important types of artifact are relevant to the Gove and Hughes argument: the selection of research topics and the selection of types of data. Omissions that result from a failure to search must be interpreted differently from omissions that result from a search that fails.

Gove and Hughes report that neither they nor those colleagues who

read earlier drafts of their paper were able to think of a reason to expect a structural effect to occur. Soliciting hunches from a small sample of convenience of sociologists about matters not addressed in the literature is, in the absence of sound theory, a reasonable way to generate and select hypotheses for testing. But such hunches are not reasonable bases for empirical determinations—the use which Gove and Hughes suggest.

It is especially disturbing that this attempt to circumvent the limitations of aggregate data was made in a substantive area with policy implications. Insistence on establishing individual level causation, even when individual level data are not available, cannot help but obscure the ameliorative implications of the aggregate data. Gove and Hughes' review of the literature on suicide makes a convincing case that high rates of people living alone are associated with high rates of suicide. Reducing the proportion of people who live alone, then, ought to reduce the suicide rate. The proportion of people living alone is manipulable. Tax benefits can be provided to people who live with others. Benefits and eligibility requirements for welfare and social security can be altered to encourage people to live with others. Incentives can be provided to builders, developers, and landlords to build and maintain units that are best suited by size, design, and price for multiple occupancy. These manipulations do not require a new bureaucracy nor an army of therapists to serve the target population. The costs may be high, but they can be estimated in advance and calculated accurately. If they are not justified by results, the incentives can be removed without dismantling programs. Moreover, such a social experiment would provide a test of our analyses of aggregate data. If reducing the proportion of people who live alone does not reduce the suicide rate we will be sure that the aggregate correlations between the two are spurious.

Ultimately, sociological theory will have to incorporate individual level causation to some extent. In the absence of adequate data, though, it seems preferable to leave questions moot, build theory on sound, available findings, and stop presuming that social problems can only be ameliorated by locating likely individual victims and delivering them to clinicians for treatment. In many cases, the social facts may be directly manipulable without coercion if we can convince the government of our programs. The consistent failure of programs based on individual (and small group) treatment may help us in that task. The health or even survival of our discipline may well depend on our ability to make sound, distinctive policy recommendations that justify research expenditures. Insistence on stretching our data to suit our prejudices of what theory must finally be and traditional ameliorative approaches may be self-destructive as well as methodologically dubious.

**Note**

1. My argument also applies to mortality related to alcoholism. For ease of presentation, though, I will refer only to suicide.

**Reference**

Blau, Peter. 1960. "Structural Effects." *American Sociological Review* 25(April):178-93.

# Theory and Research in a Dangerous World: Reply to Handel\*

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Professor Handel makes essentially two points in his comment on our article (Gove and Hughes). First, he argues that although we do acknowledge the possibility of structural effects, we dismiss them as implausible for reasons which are not compelling. Second, he suggests that the search for individual causation encourages ineffectual policies for dealing with social problems. We will respond to these two points.

Handel's argument hinges on the possibility that aggregating people who live alone may create a structural effect. Given his framework this means that as more persons live alone, some process is put into motion which results in persons who do not live alone having a higher probability of committing suicide than previously. If this process could be conceptualized and indicators for the variables in the process measured, one could enter these variables in the regression equation and this would remove the effects of percent living alone on the suicide rate.<sup>1</sup> He argues therefore that we have a misspecified model. As we noted in our article, Hanushek et al. have shown that if one has a properly specified model then aggregated data may be used without fear that the ecological fallacy will be committed. (This, of course, assumes that there are no unaccounted for structural effects.)

We dismissed the structural effects to which Handel refers because there was no theoretical reason to anticipate such effects, and such effects seemed implausible to us and our colleagues who read the paper. In short, there is nothing in the literature or in common folk knowledge which suggests that as the proportion of persons living alone increases, other people start killing themselves.<sup>2</sup> Handel argues that our dismissal of this possibility is unjustified because the absence of such possibilities from the literature is due to the fact that sociologists in general have ignored structural effects (even though he acknowledges that we did consider them).

\*We would like to thank Glenn Firebaugh for his comments on an earlier draft.

It is a serious distortion of the facts to state that sociologists have ignored structural effects. There is a vast literature on this topic and it is rapidly increasing. It has come a long way since that article of Blau's which Handel cites. In fact, it is so large that citing a list of the important works in this area might, itself, exceed our space limitations. Handel and other interested readers should look at Boyd and Iverson, Hüttner, Valkonen, and Firebaugh. It is very difficult in the face of such a large body of work to support the argument that the literature is empty and the results of our literature search are not valid. It may be that there are many sociologists who prefer to study individuals and think about what causes individual behavior of various kinds (for an interesting discussion of the lack of merit in such enterprises, see Mayhew, a, b). It is clearly not the case, however, that structural and other contextual effects have been ignored. In fact, in our paper we specifically consider the contextual effect of level of urbanization as measured by city size and persons per acre.

Handel's advice appears to be that if there is a mathematical possibility that a relationship is spurious, then we should automatically reject the findings—even if there is nothing in the literature to suggest a spurious relationship—even if no one (including, prominently, in this case, the critic) can think of a plausible reason that the relationship is spurious. If most sociologists who engage in social research took this advice then virtually nothing would get done. Any relationship between any two variables may be spurious due to missing variables in the model. Sociologists live in a dangerous world. The only way we have of being able to come to tentative conclusions in science is to let ourselves be guided by theoretical considerations. There are infinite possibilities for spurious relationships and we therefore cannot rely entirely on our data. In this case, there was nothing empirical or theoretical in the literature suggesting that structural effects were operating, and failing that, our own theorizing and that of our colleagues (and Professor Handel's as well) resulted in nothing. We felt comfortable in dismissing the possibility. It is something that sociologists and other scientists do every day.

Handel's second point may be summarized in two quotes from his comment: ". . . a search for individual causation obscures the ameliorative implications of the aggregate data"; and later he implies that we are among those who presume "that social problems can only be ameliorated by locating likely individual victims and delivering them to clinicians for treatment." Aside from wondering how data can be ameliorating (perhaps he meant policy), we have no idea how our article stimulated such a criticism. It does, however, merit some reply.

Handel suggests that since there is much in the literature to suggest that persons who live alone are more likely to kill themselves than persons who live with others, in the absence of any individual studies which determine causation it would be convenient to simply reduce the proportion of

persons who live alone, through social policy, and that this would perhaps reduce the suicide rate. We find this rather frightening—frightening particularly because Handel suggests that such policy implementations are especially appropriate when we are able to manipulate aggregate variables while not knowing *all* of the effects of such manipulations at the individual level. This is apparently part of what he means by aggregate data being ameliorative. When we do not know what is happening at the individual level, but are able to manipulate some aggregate level variable which is related to individual behavior in an attempt to control that behavior, we have absolutely no idea what other factors are being affected. For example, in a forthcoming article (Hughes and Gove) we show that persons who live alone are in no worse, and on many indicators, better mental health than persons in the same marital status who live with others. We suggest that living alone is causally related to the suicide rate because of social control (i.e., a person present to intervene), not increased meaning of life produced by communal or familistic living arrangements. It is thus plausible that reducing the proportion of persons living alone could have the effect of creating a net rise in the amount of misery in the world because social policy makers chose to ignore the individual effects of living alone.

We are perhaps lucky as citizens that policymakers ignore most of what sociologists say; we may be quite unlucky as sociologists (Gibbs) for our own research funds may dry up if we are not more careful and effective in our policy recommendations.

## Notes

1. Often when structural or contextual effects are examined, this is not done. Rather, individual level data are used and individual level properties are aggregated for certain categories of individuals (e.g., by school or neighborhood, etc.), and the mean values of categories are used as indicators of the structural or contextual variables for individuals. The presence of structural or contextual effects in such cases is indicated by both empirical findings and theoretical arguments which detail the processes—processes which are not intuitively obvious by simply looking at the regression coefficients. It should be noted that there is a lively debate over whether such apparent effects are actually structural effects or if they simply mean that the individual level model is not properly specified (see Hauser).

2. Actually, status integration theory (Gibbs and Martin) does provide a rationale for structural effects of this kind; however, at the relatively low percentages of the population who live alone in cities in the United States (far below a majority of the population), as the proportion of persons who live alone increases, we would not expect a rise in the suicide rate among those who live with others as a result of a decline in status integration.

Handel's paper serves to reinforce our contention that contextual effects are implausible in this case. As we noted in our article we could think of no theoretical grounds for expecting contextual effects in our study. Neither could our colleagues. Significantly, neither did Handel. Indeed, Handel's only evidence—the Blau study—is no evidence at all. In point of fact, Blau's study is not of suicide; it is a study of the orientation of public assistance agencies to their clients. Aside from the fact that Blau's study is methodologically flawed (see Tannenbaum and Bachman) and thus his finding concerning contextual effects in public assistance agencies is itself in doubt, the finding of a contextual effect in public assistance agencies hardly speaks to the question of a contextual effect on suicide rates for cities.

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# Airplane Accidents, Murder, and the Mass Media: Comment on Phillips\*

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Professor David Phillips' recent analysis of the effect of mass media reports about cases of murder-suicides on subsequent fatal aircraft accidents is replete with methodological and conceptual problems. According to Phillips, both commercial and noncommercial accidents show a statistically significant increase within a span of several days (3 days with noncommercial aircraft fatalities and 7 days for commercial aircraft fatalities) following a widely publicized murder-suicide. Drawing on his prior studies of the linkage between suicides by "personalities," mass media accounts, and an increase in single vehicle automobile accidents within three days of the publicized suicide, Phillips (b,c,d) asserts that the statistically significant increase in aircraft fatalities can be explained due to suicide, as well as a "consciously or unconsciously" induced motive on the part of the pilot to also murder some person or persons. What Phillips does, in effect, is impute suicidal motives to some deceased persons on the basis of the statistically significant increases in accidents. Such a jump is conceptually unwise because it is based on a tautology: the statistical increase is the basis for defining some cases as suicide, but these cases are also used to explain the increase. Moreover, unless there is clear evidence that the cases under investigation are suicides, independently of their inclusion within a general class, then other explanations of the accident increases must be ruled out before positing suicide as a viable explanation. That Phillips has not done this will be illustrated with six cases he uses as data in analyzing U. S. Air Carrier crashes occurring in the Control Period—7 days prior to a publicized murder-suicide, and those occurring in the Experimental Period—7 days following the publicized murder-suicide.

There are two crucial methodological problems apparent in the Phillips' (a) data. One involves the definition of the original murder-suicide. In Note 3, Phillips acknowledges that four cases were defined as murder-suicide even though the individuals were shot by the police. This is justi-

\*David P. Phillips. 1980 "Airplane Accidents, Murder, and the Mass Media: Towards a Theory of Imitation and Suggestion." *Social Forces* 58(June).1001-24.

fied, he explains, since there is some literature which suggests that some killings are victim-precipitated and therefore qualify as suicides. However, the literature cited is based on some detailed case studies, but similar background information is not contained in the four instances cited by Phillips. Thus, treating these cases *prima facie* as murder-suicide is not justified. Another definitional problem involves the category, "U. S. Air Carrier Crashes." While most of the cases included by Phillips are "scheduled domestic passenger service," he also includes at least one fatal accident involving flight training, and two accidents involving Cessnas—noncommercial aircraft—which collided with commercial air carriers. Even though these data are grouped in air carrier accident reports, there are clearly some important differences.

The second methodological problem is far more important for the thrust of Phillips' analysis and subsequent theoretical interpretation. Important substantive information about the cause and related factors involved in the accidents cited by Phillips is not presented, even when this information is available and fundamentally challenges the interpretation that an aircraft accident was a murder-suicide. Federal authorities do detailed investigations of fatal aircraft incidents,<sup>8</sup> and on the basis of these data, present individual case reports which fix the probable cause (e.g., pilot, personnel, weather, etc.) and/or one or more factors which may have contributed to the accident. These and other data are summarized in a 66 × 60 matrix in each annual report of the U. S. National Transportation Safety Board.

The federal reports are extremely important for Phillips' (a) findings and subsequent explanation of some aircraft crashes as murder-suicides, since the investigators do attempt to ascertain the extent of pilot involvement and responsibility for the accident. If these investigations reveal that a pilot was not the probable cause or one of the major factors, then any researcher would seemingly be hard-pressed to insist that a particular crash was nevertheless a murder-suicide simply because it occurred within a given time period, along with other accidents.

Based on the records currently available to me, I have examined six of the sixteen aircraft accidents cited by Phillips (a). Examining the briefs of these accidents raises serious questions about the explanation of some of these crashes as murder-suicide. In what follows, pertinent information for the four crashes in the Experimental Period will be given, followed by information from the two accidents listed in the Control Period for which I have information.

### **Experimental Period (Defined as "Murder-Suicide")**

1. August 10, 1968: A Fairchild FH-227 crashed during the final approach to landing. Thirty-five people, including three crew members, were killed. Probable cause: "Pilot in Command—misjudged distance and altitude." Factor: "Weather—fog . . . visibility at accident site: 1/4 mile or less." Remarks: "Unrecognized loss of altitude orientation during final portion of approach into shallow dense fog."
2. August 14, 1968: A Sikorsky S-61L crashed during normal cruising. Eighteen people, including three crew members, were killed. Probable cause: "Rotor assemblies . . . fatigue fracture"; Personnel: "maintenance, servicing, inspection: inadequate maintenance and inspection." Remarks: "Fatigue origin in area of substandard hardness and inadequate shot pending on m/r blade spindle."
3. December 4, 1971: A Cessna 206 (noncommercial) collided with a Douglas DC-9 (commercial) during the final approach to landing. Both occupants of the Cessna were killed. Probable cause: "traffic control personnel." Remarks: "Relative aircraft flight paths and configurations physically limited each flight crew's ability to see, avoid other aircraft . . . DC-9 descended onto Cessna."
4. May 30, 1972: A Douglas DC-9, with crew of four on a training flight, crashed during the final approach to landing. All four crew members were killed. Probable cause: "miscellaneous—vortex turbulence." Remarks: "Vortex turbulence from preceding DC-10. Tower gave warning. Crew unable to evaluate hazard due to insufficient information."

### **Control Period (Not Defined as "Murder-Suicide")**

5. August 4, 1968: A Cessna 150 (noncommercial) was descended upon by a Convair 580 (commercial). The three occupants of the Cessna were killed; one of twelve persons aboard the Convair—a crew member—was seriously injured. Probable Cause: "Personnel-pilot of other aircraft." Remarks: "(Convair) struck (Cessna) in flight."
6. March 3, 1972: A Fairchild FH2278 crashed during the leveling off/touch-down phase of the final approach to landing. Seventeen people, including two crew members, were killed; thirty-seven persons were injured. Probable cause: "Powerplant; propeller and accessories;" Factor: Pilot in command—"inadequate supervision of flight—diverted attention from operation of aircraft." Miscellaneous acts, conditions: "instruments—misread or failed to read; crew coordination, poor."

Several points should be noted about these cases. First, pilot related negligence was found by federal investigators to be a significant factor in two accidents, #1 and #5, although pilot considerations were also noted in #6. Thus, pilot negligence was involved in cases located in Phillips' Experimental as well as Control Periods. Second, federal investigators concluded that four crashes could be accounted for, at least in part, by mechanical failures (#6), weather hazards (#1), including "vortex turbulence" caused by a preceding aircraft (#4), or by errors or oversights of other personnel, e.g., maintenance (#2), traffic control (#3). Further, two of the crashes (#3 and #5) where fatalities occurred, were due to collisions when other aircraft descended upon noncommercial Cessnas during landing. Yet, Phillips includes #3 as an example of "murder-suicide"!

It is apparent that time order of occurrence and statistical peaks are inadequate modes of explanation without more detailed examination on a case-by-case basis. This additional information not only sheds more conceptual light on the crashes included in Phillips' (a) analysis, but also suggests that when prior investigation by authorities has ruled out pilot related negligence as a major cause or even a factor, then murder and suicide as possible motives in these crashes must also be ruled out, unless the adequacy of these federal reports is challenged and undermined, perhaps in the manner illustrated by Altheide and Johnson. Moreover, it should be clear that when further case information discredits the logic of defining one crash as a murder-suicide, then it would be unwise to not attempt to validate the remaining cases. The present analysis raises serious questions about the empirical and conceptual distinctiveness of six cases.

Finally, there is the problem with the theoretical interpretation of statistical regularities. As we seem destined to continually relearn, correlation is not causation, and binomial tests do not demonstrate substantive relationships; other factors must be considered. And insofar as Phillips' (a) imitation and suggestion hypothesis is not supported by the available information, it remains to be demonstrated.

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# The Complementary Virtues of Qualitative and Quantitative Research: Reply to Altheide

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Before I can comment on Professor Altheide's remarks, some background information will be needed. In a series of papers in the *American Sociological Review*, the *American Journal of Sociology*, *Social Forces*, and in *Science* I presented evidence suggesting that publicized suicide stories trigger imitative suicides, some of which are misclassified as accidents (Phillips, a, b, c, d, e; Bollen and Phillips). These papers showed that: (1) U.S. suicides rise significantly just after publicized suicide stories (e.g., that of Marilyn Monroe). (2) Fatal automobile accidents also increase just after publicized suicide stories. (3) Single-car accidents increase more than other types of accidents. (4) The drivers in these single-car accidents are unusually similar to the person described in the suicide story, but passengers in these accidents are not similar to the person described in the suicide story. (5) The more publicity given to the suicide story, the greater the increase in suicides and the greater the increase in motor vehicle accidents. (6) The increase in suicides and motor vehicle accidents occurs primarily in the geographic areas where the suicide story is publicized. These findings are based on a systematic survey of more than 80,000 suicide and accident statistics.

In follow-up papers in *Science* and in *Social Forces* I showed that these findings for suicides and motor vehicle accidents also hold for airplane accidents. I examined 172 airplane accidents (not 16 as Dr. Altheide claims) and showed that: (7) U.S. private plane crashes increase significantly just after publicized murder-suicide stories. (8) The greater the publicity devoted to the murder-suicide story, the greater the increase in private plane crashes. (9) The increase in plane crashes occurs primarily in the geographic areas where the murder-suicide story is publicized. These findings hold not only for non-commercial but also for commercial plane crashes. Specifically, (10) U.S. commercial plane crashes increase significantly just after publicized murder-suicide stories. (11) The greater the publicity devoted to the murder-suicide story, the greater the increase in commercial plane crash fatalities just afterwards.

All of these eleven findings are statistically significant and persist after one corrects for seasonal fluctuations in the data and for trends over time. Alternative explanations for the findings were assessed. At present,

the best available explanation is that suicide and murder-suicide stories trigger imitative suicides and murder-suicides; some of these are disguised as accidents.

In sum, I have presented a series of eleven mutually consistent findings, which are based on a systematic investigation of more than 80,000 cases. In contrast, Dr. Altheide investigates six cases, which were haphazardly selected. He apparently believes that this sample, small and haphazard though it may be, constitutes sufficient evidence with which to assess my findings.

Dr. Altheide's labor-saving approach to sample selection, although attractive, is nonetheless unsound. This point can be illustrated with the following example. Suppose we wish to assess the statistical evidence linking cigarette smoking and lung cancer. Following Dr. Altheide, we first select a haphazard sample of six persons, three of whom have lung cancer, and three of whom do not. We then analyze case histories of these persons very carefully. After analyzing these six persons, can we then provide a definitive assessment of the statistical evidence linking smoking and lung cancer? Surely not.

Dr. Altheide's approach exhibits the strengths and the weaknesses of much qualitative research. On the one hand, there is a praiseworthy insistence on collecting as much information as possible per case. On the other hand, there is a regrettable willingness to be satisfied with a small, haphazard sample of cases.

In contrast, much quantitative research (including mine) exhibits a different set of strengths and weaknesses. On the one hand, there is a praiseworthy insistence on the systematic collection of large numbers of cases. On the other hand, there is a regrettable willingness to be satisfied with a small amount of information per case.

Evidently, qualitative and quantitative approaches are complementary, not competitive. Powerful research would combine both approaches. First, one could use large-scale quantitative analysis to demonstrate the existence of a previously unsuspected phenomenon. Then, one could use small-scale qualitative analysis to investigate the detailed processes producing the phenomenon.

In view of these considerations, I am delighted that Dr. Altheide has begun a qualitative reanalysis of my data. To be most convincing, such a reanalysis should examine, not six cases, but all 172 airplane crashes studied. An individual case history of each of these crashes is available from the National Transportation Safety Board, and can also be found in the Documents Department of many libraries.

The research approach sketched by Altheide should also be used to analyze case histories of suicide and motor vehicle fatalities. Using these case histories, the qualitative investigator should seek to determine whether

deaths occurring just after a suicide story are generally different from deaths occurring just before.

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# Book Reviews

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## **Community Power Succession: Atlanta's Policy-Makers Revisited.**

By Floyd Hunter. Chapel Hill: University of North Carolina Press, 1980. 198 pages. Cloth, \$15.00; paper, \$9.00.

Reviewer: FREDERICK W. FREY, *University of Pennsylvania*

In the mid-50s, two provocative works on power in American society splashed before us. Both were written by sociologists: Floyd Hunter's *Community Power Structure* (1953) and C. Wright Mills' *The Power Elite* (1956). Both asserted primarily business-based elite domination of the political community analyzed—Atlanta, Georgia, by Hunter and the national United States by Mills. Both aroused vehement disagreement and defense, precipitating what was later christened "the elitist-pluralist controversy." Mills' work, of course, also became one of the required texts of the New Left movement and Hunter's prompted numerous community power researches.

In tone and approach, however, the two works displayed conspicuous differences. Mills' was a classic polemic. Its actors were tendentiously labeled ("Warlords," "The Political Directorate," etc.), its evidence was slim and secondary, and its interpretations were often strained. Nonetheless, it was an important work, not in theory, findings, or methods, but because it focused public and scholarly attention on the problematic nature and the vital importance of our understanding of the distribution of power in American society. Though Mills cannot be said to have made his case, we are in debt to him for the attempt—as courageous as it may have been outrageous.

Hunter's *Community Power Structure* was much less dramatic, more scholarly, empirical, and methodological, as was his national study, *Top Leadership U.S.A.*, which followed in 1959. The great virtue of his work was that it treated the discovery of patterns of power in a major political community as an explicit, objectively researchable problem and attempted to develop a technique for that research. Hunter's claim, in his new book, that he "opened up a new era, the empirical study of social power" is largely justified, at least for non-anthropological community power analyses.

What followed the publication of *Community Power Structure* was progressively productive, perplexing, and disappointing. Not surprisingly, there were serious problems with the panel variant of the "reputational" or attributional method of locating and measuring power that Hunter developed. Pioneering research rarely escapes such flaws. The early criticisms of Hunter's work flagged these difficulties and markedly improved our grasp of the concept of power, the measurement of power, the notion of an elite, and so on. Unfortunately, rather than being presented in the spirit of joint scholarly pursuit of common concerns, many



of the criticisms of Hunter's work (and its defenses, by others) quickly became acrimonious. This may have been due to the presumed association of Hunter with Mills, whose hyperbolic, accusatory style offended many. Whatever the reason, there was a failure to credit Hunter adequately for his remarkably influential, albeit flawed, achievement, together with a more general failure to appreciate the significance of the community power research effort, ineffective though it may have been. If we cannot fathom the patterns of power in relatively accessible American cities and towns, what confidence can plausibly be placed in our claims for political analyses of alien national systems? The community power research effort, at the very least, exhibits more clearly than other areas of political inquiry most of the fundamental problems that beset the analysis of power—problems that tend to be even more recalcitrant and much less well perceived elsewhere.

*Community Power Succession* reports Hunter's return to Atlanta after more than two decades in order to investigate possible changes in its power configurations and the reasons for them. Unhappily—and I should like to say otherwise—it constitutes a sad and retrograde denouement to the entire episode. Part diary, part polemic, part ostensibly scholarly analysis, it abandons the exploratory, innovative, strongly empirical approach and the reasonably straightforward prose of his earlier work for rigidity, pontification, and Millsian rhetoric. In the Introduction, Hunter deplores too much time wasted on unconvincing methodological discussion and suggests that his own preferred strategy for responding to criticism lay "in correcting in the succeeding works . . . any mistakes I recognized." This work reveals throughout that he has recognized few or none. Thus, his ultimate "reply to his critics" is simply repetition and more insistent restatement. It is difficult to ascertain whether power patterns in Atlanta have changed as little as Hunter suggests; but Hunter himself as a power analyst has hardly changed at all.

The bases for this negative evaluation of *Community Power Succession* can best be discussed under several rubrics: methods, structure, succession, hypotheses, and presentational style.

*Methods:* Methods are especially significant for Hunter, despite his aversion to methodological discussions, since he repeatedly stresses the "empirical" thrust of his work. Its main claim to attention has been the panel variant of the attribution method that he first systematically applied to community power research and the strongly data-oriented approach he adopted, carefully distinguishing himself from others such as Mills in this respect.

One of the first problems that a careful reader of *Community Power Succession* faces, however, is Hunter's elusiveness in providing precise explanations of his methods. He states that he "used a routine reputational assessment of power structure," asking "major groups" in the city for nominations of "top-rated power wielders" and interviewing "the principal policy-makers to determine their roles." What "major groups?" Can we assume the same four as in 1950: the Community Council, Chamber of Commerce, League of Women Voters, and newspaper editors and other civic leaders? In 1950, these groups suggested "lists" which were then referred to a panel of fourteen judges who chose the "top leaders" on each list. Was this done in 1970? There is no mention of any such panel. What specific questions were put to these "major groups?" One needs to know to check possibly hypothesis-begging formulations (the assumption of a "top twenty," "Mr. Big," etc.) and to investigate masking difficulties. How much consensus existed con-

cerning the nominees? From the data provided in this volume one cannot answer any of these essential factual questions regarding Hunter's methods. Furthermore, there are many examples of a form of spurious empiricism, where a plethora of certain kinds of data is provided (names of persons and organizations, weighting factors, numbers of instances, etc.), but without the key information to permit informed perception of precisely what was done or the specific evidential basis for a conclusion. The result is similar to certain governmental publications that provide percentages to the hundredth place, but with no absolute numerical bases.

To be sure, Hunter could be expected to encounter a serious methodological problem that often plagues re-studies. If the follow-up research design is altered to correct flaws in the original research, one confounds the comparison over time. Hunter escapes this difficulty by insisting on the complete validity of the 1950 study, regardless of its well-known problems.

One course open to him was to repeat, as faithfully as possible, the original procedures and then to add more refined techniques deliberately designed to examine the validity of the original methods and to improve them. Jennings, in his follow-up to Hunter's study of Atlanta, *Community Influentials* (1964), made a conscious effort in this direction. Hunter lists this work in his spotty bibliography but, oddly, fails to consider it in his text. Jennings interprets his research to provide "conclusive evidence that members of the attributed elite do not come close to exclusive domination of the pivotal roles," actually a rather inconclusive finding for comparison with Hunter, since the latter may claim not to argue "exclusive domination." In fact, one can plausibly interpret Jennings' data as supporting Hunter's general position, as Rossi did in his review of Jennings' study. The essential point, however, is that Hunter's methods and findings are notably and justifiably controversial; hence, they clearly require special attention, but Hunter strangely avoids and ignores this.

The one mensurative addition to Study II is what Hunter calls a "power scale." He cautions that it is in a "developmental stage," but "nevertheless it points to the fact that power may be rated and measured more precisely than seemed possible heretofore" and it may "make possible economies in time and expense in finding and polling top leadership in communities and in the nation." Unless one is willing, as Hunter seems to be, to make automatic inferences from very general resources, status, and formal authority to actual power, regardless of issue, decisional phase, or activity, this claim seems not only to be doubtful but to betray an inadequate understanding of the relational and situational complexities of power phenomena. The "power scale" consists of 39 items, many with responses weighted according to inadequately specified criteria. Many items are cryptic, e.g., "Independent judgment as member, leading corporation," "Personal Qualities: Interaction with others," or "Other items, by positive or negative weight points." More significantly, there seems to be a general bias in the "power scale" toward business and formal politics: by my calculation, more than a third of the items have unique or special reference to business positions and activities. If so, the danger is that a "power scale," supposedly unbiased but actually oriented toward the resources and positions of actors in business and formal politics, would be used to generate a set of "top leaders" whose requisite characteristics would then be discovered to be business position, wealth, office, etc.

Another grave difficulty with Hunter's methods is that, like Mills, he *assumes*

that those who have highest formal authority in organizations have individual, largely uninfluenced control over those organizations. The internal power structures of the organizations are presumed to be completely hierarchical, despite frequent evidence to the contrary. For example, when Hunter gets into the details of skyscraper construction in Atlanta, a complex lattice of influential interactions among numerous actors emerges, including, in apparently important roles, many of the professionals, planners, and second level officials whose significance he belittles. Viewing the U.S. Presidency in similar fashion, Hunter would see it as omnipotent rather than as the complex mixture of power and constraint that Carter aptly described in his farewell address. Power is most often a matter of degrees and contingencies, but Hunter's orientation and methods allow little examination of either.

*Structure:* Hunter repeatedly uses structural metaphors: "the power structure," "anchor points of power," "the inner core," etc. Once again, however, this seems as misleading as the "empiricist" label. First, the panel-attributional method produces only a *listing*, whose "rankings" may as well reflect mere panel disagreement as relative power positions. It does not provide a true structural picture of *relationships* among actors. Once the list is obtained, one can, of course, obtain fuller structural information. This Hunter does for "some fifty-six persons," though only 53 are included in his list of "power names." The structural relationships are vaguely described as "primary relationships"—i.e., "business, professional, and political relationships." Among these, "four major sociometric clusters emerged, each with a 'star-isolate' to whom all of each cluster were related." All clusters were, in turn, connected by "primary mutual relationships," and these "tied all fifty-six together in a structure." The "star-isolates" were the major financial, industrial, or business leaders of the community, according to Hunter. Such is the essential description of the sole effort at more rigorous structural analysis. It precludes evaluation, other than rejection on grounds of vagueness. The specific types of relationships involved (power, acquaintance, communication), the kind of clustering technique employed, and even the names of the actors in various structural positions and groups (so lavishly furnished at other times) are not adequately indicated. The result is the assertion of structural analysis without confirmatory evidence.

Another unfortunate feature of Hunter's treatment of structure is his vocabulary. He defines a power structure as a "coordinated system, public and private, formal and informal, of learned and repeated power roles and relationships, the function of which is the maintenance of any prescribed differentiated social order." The last part of this definition is a significant deviation from the more common, more neutral conception of a social structure as a set of social relationships mapped onto a set of actors. Hunter's conception leads him to speak of "*the power structure*" to mean what most would call a power elite. Thus, he is wont to say that "the power structure has kept a strong hand on" something or other, or that such-and-such is a "gathering place for the power structure," and to refer to "lists of power structures." This deviant usage has led some of his critics to deny the utility of the notion of a power structure. In general, the structural content of this volume is primitive and impressionistic to an extreme.

*Succession:* Study II finds power in Atlanta in 1970 in the hands of a business-based elite, as it was in 1950. Five of the top ten in 1970 "deemed by their

peers to be most powerful had succeeded a member of their own family in acquiring their present position." Of the next 26 power leaders, seven "held power as an 'inheritance'" [?], and so the analysis goes. Of the total of 56 top leaders in 1970, six were also in the power elite of 1950. On the critical continuing issue of urban annexation that surfaced in both studies, roughly half the "power-generating organizations of 1952 were active . . . in 1972." Hunter interprets such data as indicating a high degree of elite succession, just as he interprets corresponding "overlap" data across issues and sectors as indicating cohesive elite control. One could, however, clearly use the same data to make a completely contrary argument. Everything depends on one's expectations, and norms are conspicuously lacking in this area.

An intriguing feature of Atlanta's power structures, even as described by Hunter, is the apparent growth in the power of a number of black leaders. Some blacks now frequent the elite Commerce Club; they have won the mayor's office more than once, and have moved into political offices at all levels. Predictably, Hunter is quite equivocal concerning such changes. He tends to argue that these are "token" advances, a minimal price paid by the white financial community, etc. This is in keeping with his general inclination to dismiss the failures of his putative power elite, the victories of non-elite elements, and all other counter-instances as anomalous or trivial. Yet, at other points the text states that "a goodly portion of 'political' power has been *wrested*" (my emphasis) from the power elite by the civil rights movement—essentially blacks and white liberals. Hunter concedes that blacks constitute a "powerful voting force" and that black office holders won their positions despite the fact that "the white power structure never wavered in its behind-the-scenes opposition to their obtaining office." Similar contradictions are evident in his treatment of the power successes of liberals, professionals, and other groups.

*Hypotheses:* Hunter states that "a set of social and political hypotheses constitutes the essential base of reasoning behind the political discussion running through the whole work." There are no special contributions in methods or methodological discussion, in the systematic analysis of succession and other power phenomena, or in practical suggestions for reform (Hunter concludes with a superficial although refreshingly well-labeled "assertion" that a mature cooperative movement could provide a beginning toward political improvement). Thus, unless one is so particularly involved with Atlanta that factual minutiae have intrinsic interest, the final area for a potential contribution would appear to be these hypotheses. Alas, this prospect for social scientific significance also peters out.

The first problem is definitional. Many of Hunter's most basic conceptual tools, bearing familiar labels such as "power," "power structure," etc., are defined so as to carry very dubious interpretive baggage. For example, he contends that "power can be precisely defined only as a *social function*, that of *establishing and maintaining given institutional and social modes of order*" (my emphasis). Through these definitions and hypotheses his "essential base of reasoning" is revealed to include such *assumptions* as that of a bifurcated power structure with a "ruling circle" and an "understructure." Power is *assumed* to be essentially the vehicle by which the ruling circle maintains its positions and values. Hunter *assumes* that "the economic institution [is] the ascendant one in world technological society." He may be right or wrong in these perceptions, but a social scientist cannot appropriately decide

these matters by assumption. Although at times he labels some of the assumptions "hypotheses," they are never truly treated as such.

The second problem is that Hunter's "hypotheses" are usually untestable because many of his key concepts are not operationalized and probably cannot be. He certainly makes no conscientious effort to confirm his hypotheses, although he sporadically points to events or phenomena that he believes illustrate them. In other instances, the hypotheses far outpace his data, as when he proceeds from Atlanta, to cities, to nations, to the international order or the world. Even granting him the picture of national power sketched in *Top Leadership U.S.A.*, which many would be loath to do, his assertions far exceed his evidence.

*Presentational style:* In a footnote, Hunter relates that Mills once said to him that "he was never empirical if he could help it," to which Hunter replied that "he was never theoretical and polemical if he could help it." Judging from the present volume, the reply was a half-truth. The book is certainly not theoretical in any scientific sense, but it is strongly polemical. The one change discernible from Study I is a marked increase in Millsian rhetoric. There is the same exaggeration, the same condescension, the same biased comparison and, above all, the same polemical labeling and name calling. One encounters the "corporate/governmental juggernaut," "white overlords," "big brother government," "big daddy corporations," "the ownership establishment," "America's ensconced corporate money aristocracy," "the patrons," "cogs of the system," "the Uriah Heep postures" of the professional understructure, and so on. Economists are castigated as "the modern day counterparts of the augurers of ancient Rome, those who read the future by stirring the entrails of slain animals." The middle class lives in "debt peonage or, at worst, debt slavery." The purpose of such emotional labeling is to win by stereotyping what cannot readily be gained by reason and evidence. It does not belong in a self-styled work of social science.

All in all, then, this is an unfortunate book, offering little to the serious student of social power. Many of us share Hunter's obvious concern for the poor and the powerless. Indeed, it seems, to many of us, as to him, that there is a pronounced oligarchic pattern to most large-scale social systems, although differences in degree are important. Nevertheless, as social scientists, if we indulge ourselves in the kind of work that Hunter here presents, we commit both a professional and a civic disservice.

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### **Classes, Strata and Power.**

*By Włodzimierz Wesolowski. London: Routledge & Kegan Paul, 1979. 159 pp. \$6.95.*

*Reviewer: GERHARD LENSKI, University of North Carolina at Chapel Hill*

For more than a quarter of a century, Poles have been struggling in the shadow of the Soviet Union to create a more democratic form of socialism. Polish sociologists have made important contributions to this struggle, not least of which has been the challenge they have raised to Marxist-Leninist orthodoxy and dogma. Beginning with the pioneering work of Stanislaw Ossowski (*Class Structure in the Social Consciousness*, 1956), a succession of scholars, both Marxists and non-Marxists, have brought the insights of modern sociology to bear on the study of their own society

and, in the process, have laid the foundations for a new and exciting sociology of socialist societies.

Włodzimierz Wesolowski has been one of the most important contributors to this process, and the present volume, now translated into English for the first time (it was originally published in Polish in 1966), is one of his most important contributions. This collection of theoretical essays established the basic agenda for fifteen years' research on social inequality and stratification in socialist Poland that has done more to advance our understanding both of stratification and of socialism than any other program of research or scholarly activity anywhere. One need not agree with all of Wesolowski's ideas to appreciate the magnitude of his contribution. As George Kolankiewicz indicates in his introduction to this English-language edition, Wesolowski anticipated by some years a number of the important contributions of western European Marxists, such as Althusser.

This volume consists of three theoretical essays. The first examines critically the Marxian concept of class domination, which Wesolowski disaggregates into three distinct components: economic domination, political domination, and ideological domination. He argues that it is a serious mistake to treat the last two as mere appendages of the first, as so many have done. In capitalist societies, political elites are not merely a segment of the economically dominant bourgeoisie, nor are they merely their lackeys. "By comparison with the feudal system, capitalism is characterized by the separation of two functions which had hitherto appeared jointly: the ownership of the means of production and the representation of state power." Because of this, the politics of capitalist societies are far more complex than much Marxist writing would lead one to suspect and this synthetic-evolutionary model, which weds Marxist and elitist theories, lays a foundation for a far more sophisticated mode of analysis than either can provide alone. The value of this model becomes most clearly evident in the final section of the book, where Wesolowski turns his attention to socialist societies.

The second, and least innovative, of the essays is a critique of the Davis-Moore theory of stratification. Those who have followed the controversy generated by this theory will find few surprises, but it is a sound critique and worth the reading.

The most interesting and valuable of the essays is the last. Here Wesolowski applies his theoretical model to the analysis of contemporary socialist society. Marx's own writings, of course, are remarkably thin in their discussions of post-revolutionary societies. For understandable reasons, most twentieth-century Marxists have found it convenient to limit their use of the master's tools to analyses of capitalist societies, emphasizing the great gap between capitalist reality and socialist ideals. Wesolowski, in contrast, insists on the necessity of applying these tools to the analysis of socialist reality. Drawing on the distinctions between economic, political, and ideological domination established earlier, Wesolowski argues that the abolition of the private ownership of the means of production in no way guarantees the elimination of political and ideological domination or of social and political inequality. Classes may vanish, but strata remain, as does a politically dominant elite. Thus, special interests persist and social conflict is unavoidable. In short, the gulf between contemporary capitalist societies and contemporary socialist societies is not nearly as great as Marxist analyses of capitalist societies would lead one to expect. This is not to say that the two types of societies are the

same, but Wesolowski does appear to say that the same basic conceptual tools can be applied in the analysis of both. Finally, as an evolutionary Marxist, Wesolowski insists on the dynamic character of contemporary socialist societies and argues that through various kinds of policies they are moving slowly in the direction of the ideals they espouse. For example, he sees a process of decomposition of class attributes underway in contemporary Poland (i.e., a process loosening the linkages between education, income, occupational prestige, etc.).

This brief summary cannot do justice to the sophisticated, critical, and creative theoretical analysis developed in *Classes, Strata and Power* and I urge anyone interested in political sociology, stratification, or macrosystem change to read this volume. Some of Wesolowski's statements have proven to be remarkably prescient. For example, he wrote at one point that in socialist societies "Groups at different income levels tend to express less dissatisfaction *vis-à-vis* each other than they do towards the state as the regulator of incomes. . . . Paradoxically, the socialist state—no longer the guardian of privilege and exploitation, striving as it does to satisfy the needs of all its citizens—may appear to people as more often 'the antagonist' than the state which is less concerned for their interests."

While one may question Wesolowski's position on a number of specific points, the basic ideas are sound and lay the foundation for important advances in both theory and research in the years ahead. In fact, as noted earlier, Wesolowski's ideas have already been put to good use by numerous Polish sociologists, especially in studies of stratification. With this new translation, his ideas can now be picked up and used by English-speaking sociologists. If that happens, it will mean a welcome revival of interest in some neglected topics of research (e.g., the study of power elites) and a substantial expansion of others (e.g., comparisons of socialist and capitalist systems of stratification).

**Main Currents of Marxism. Volume 1: The Founders; Volume 2: The Golden Age; Volume 3: The Breakdown.**

By Leszek Kolakowski. New York: Oxford University Press, 1978. 434 pp.; 541 pp.; 541 pp. \$22.95 per volume.

Reviewer: CRAIG CALHOUN, University of North Carolina at Chapel Hill

As a dissident political commentator and philosophical critic, Leszek Kolakowski attracted attention and praise from many members of the "New Left" during the late 1950s and early 1960s. Writing from within Poland, he managed to combine historical erudition, wit, and humanism in veiled yet biting critiques of the orthodox Communist regime. Kolakowski was primarily an expert on early modern philosophy, yet even his essays on Spinoza seemed pregnant with modern implications. Eventually, however, he moved beyond revisionism; more or less simultaneously he moved beyond the limits of Polish tolerance. After being dismissed from his professorship at Warsaw and expelled from Poland in 1968, Kolakowski taught at Montreal, Yale, and Berkeley, settling finally in Oxford as a Fellow of All Souls College. This establishment itinerary (and his rejection of Marxism for being necessarily connected to the repressive regimes he had experienced) turned Kolakowski's former admirers on the Left into critics. In a hundred-



page "Open Letter to Leszek Kolakowski" (*Socialist Register*, 1973), the great British Marxist historian E. P. Thompson expressed a very personal "sense of injury and betrayal," laying out his own biography and moral standards to invite reconciliation. Kolakowski replied courteously but unbendingly in "My Correct Views on Everything" (*Socialist Register*, 1974), and his ties to at least that segment of the "Old New Left" were severed. He now describes the New Left as a bunch of adolescents.

Not surprisingly, when Kolakowski came to publish a three-volume "handbook" on Marxism, he met a good deal of comradely criticism. He was held to be too much a humanist, too little a Marxist, unfair to the Third World, and most of all, somehow disloyal to Marxism. Nonetheless, his book remains the only resource of its kind, so it exerts an influence, even on the Left. But readers should beware on at least two counts: first, this is a handbook only in the vaguest of senses; second, Kolakowski has no sympathy whatever for his subject matter.

This is very much a philosopher's book, about a philosopher's Marxism. There are political comments, of course, and a discussion of Marxism as the ideology of the Soviet state, but Kolakowski's treatment focuses much more on the Marx who followed Hegel than the one indebted to Smith and Ricardo, or on the radical politics of early nineteenth-century workers themselves. Similarly, Kolakowski is interested in later Marxists primarily to the extent that they can be considered as philosophers; references to Marxist economists, historians, social scientists, and the like are casual at best, unless they can be seen as arguing philosophical questions. This of course distorts Kolakowski's representation of Marxism as a field of discourse.

It should also be noted that this self-styled handbook is not an introduction. It is written at a fairly advanced level, in a clear, but far from simplified, prose. More importantly, Kolakowski assumes throughout a considerable familiarity with the historical context of the works he discusses, and with intellectual history and philosophy in general. The book is not likely to be effective as a text, yet it is not really encyclopedic or accessible enough to be an adequate reference work. Since few scholars can approach Kolakowski's level of erudition, this book will contain a good deal of new information for most readers, but I suspect it will be most useful to those who have already mastered a good deal of the original literature to which it refers, but who would like to see the various parts laid out in order and related to each other and to the whole.

Some independent knowledge of the thinkers with whom Kolakowski deals is particularly important because he has the habit of making straw men of them. This becomes more pronounced as one draws nearer to the present day. Kolakowski allows that Marxism had some creative potential in Marx's day, but believes it was lost by the twentieth century. Since he sets up Lukács, Adorno, Bloch, and others only to knock them down, it is hard to grasp why anyone ever took them seriously in the first place, or why Kolakowski himself offers discriminations among their relative qualities.

All this said, what are the "main currents of Marxism" with which the book deals? Kolakowski begins with Plotinus and neo-Platonism, and moves quickly through various contributors to the tradition of dialectical reasoning, pausing at length only with Hegel. Marxism is held to begin with the breakup of the Hegelian system among Hegel's followers. These are briefly discussed, which is good, since they (and especially their involvement with the critique of religion) are poorly



understood even by commentators on the Marxian texts. Marx's own major works are discussed chronologically; although the weight of attention is very much on the earlier works, the discussion of *Capital* and the other later works is clear and generally sound, although brief and focused on the non-economic issues. Marx's socialism is compared with that of several other early nineteenth-century writers and activists, and a discussion of Engels (almost exclusively on the scientific treatment of the dialectic of nature) forms the primary bridge to the second volume. Engels appears as a minor and unhelpful supporting actor in this version of the Marxist drama. As a discussion of Marx's work, Volume One is good enough, but not at all outstanding. It offers too few new insights not to have been written in a more readable fashion, at a more elementary level.

Volume Two treats of Marxism during the Second International—its golden age, according to Kolakowski. Chapters summarize the work of Kautsky, Luxemburg, Bernstein, Jaurés, Lafargue, Sorel, Labriola, Krzywicki, Kelles-Krauz, Brzozowski, and the Austro-Marxists. One might quibble, but all the major figures are there, at least from the Continent. William Morris may have deserved a chapter, or at least discussion in a chapter on Britain, but there is some justice to Kolakowski's argument that British socialism of the period was less influenced by Marx than that of the rest of Europe. The tone of the volume to this point is one of straightforward reportage, though rather categorical (usually negative) judgments do abound. Sociologists should find the discussions of Sorel and the Austro-Marxists interesting, since these major figures tend to be left out of the conventional disciplinary genealogies.

Kolakowski's handbook becomes more polemical in the last chapters of Volume Two, which recount the rise of Russian Marxism. The problem is not just that Kolakowski is unfair (although he often is), but that while the best polemics are brief, this one extends for several hundred pages. The argument is basically that Lenin was not untrue to Marxism; that he perhaps missed some of the democratic spirit "hidden" in Marx, but that he was theoretically and politically at one with what the Soviet state became; therefore, that Stalinism was not an aberration, but a fulfillment of Lenin's work. That, unfortunately, so fails to confront the complexity of Marxism in the Soviet Union that Kolakowski is quite precluded from offering a real analysis. At best, he shows that Stalinism was not completely incompatible with Leninism. To show that it was a necessary outcome would require empirical study, not just theoretical derivation; the necessity, if such it was, was practical, not intellectual.

It is surprising that Kolakowski does not give more attention to the impact of the Soviet Union on Marxism elsewhere. That impact was, first, stultifying, as dependence on the USSR or deference to its revolutionary success silenced theoretical minds throughout much of Western Communism. Kolakowski at least notices this impoverishment, attributing it primarily to censorship and fear. He does not consider empirical studies of the role of the Soviet Union in international communism, such as Claudin's *The Communist Movement*. The second impact of the Soviet Union was more indirect and more creative. Western Marxists realized that they had to come to grips with the nature of the Soviet state apparatus, and to develop Marxism outside of its immediate control. This Kolakowski hardly notices. Had he considered empirical studies (I suspect the most important appeared only after the initial drafting of his account) he would perhaps have been forced to

consider that Lenin's work has been used in the analysis of the Soviet Union itself (for example by Bettelheim, in *Class Struggles in the USSR*). The analytic impact of Lenin's work—whether one agrees with it—demonstrates its difference from Stalin's.

For Kolakowski, modern Marxism is either seriously impoverished by accommodations to Leninism or else a part of the breakdown of Marxism as a coherent body of thought. With little sympathy he attacks the former branch, including Trotsky whom he sees as no different from Stalin. Korsch, Goldmann, and Bloch fall into the second category, but attract little praise. The Frankfurt school is criticized on the serious grounds that negative dialectics are self-destructive, and on such spurious grounds as the difficulty of Adorno's prose. Marcuse is dismissed as the leader of the "adolescent" New Left. Lukács gets a little credit for his earlier work on Marx's thought, but is condemned as an ideologue whose works on literature were little more than erudite expressions of dogma. A few compliments are applied sparingly to Gramsci, who appears as at least a positive symbol of alternatives in communism, despite the fact that his thought lacks order and consistency, and to the post-Stalin revisionists, on condition that their revisionism takes them, like Kolakowski, beyond Marxism altogether.

In short, in the absence of an alternative (awaiting perhaps that being edited by Hobsbawm), Kolakowski's trilogy has a place in a scholarly library. The work offers many facts and many good arguments, but because so many sections read like negative book reviews, readers will have less sense of the intellectual enterprises on which the great Marxists embarked than they should after 1500 pages. The problem is not that Kolakowski is an outsider; indeed, one wishes that a sympathetic yet critical outsider like Lichtheim had written a work of this scale. Unlike a Marxist writing from within the tradition, Kolakowski has no interest in construction from historical sources to shape future work. Nor does he attempt an interpretation of Marxism by which to bridge the gap between those inside and out. Instead, Kolakowski writes the history of Marxism to announce, and perhaps to hasten, its demise. He is less priest, jester, heretic, or historian than Marxism's would-be exorcist.

### **Multi-Dimensional Sociology.**

*By Adam Podgórecki and Maria Lós. London: Routledge & Kegan Paul, 1979. 344 pp. \$25.00.*

*Reviewer: GEORGE RITZER, University of Maryland*

On the basis of initial impressions, I should have liked this book. It deals with subjects of great interest to me—sociological theory and metatheory. It uses an approach very similar to my own—the multidimensionality of sociology and the possibility of reconciling at least some of the differences within the field. The senior author even drafted his portion of the book in the same place I am writing this review—The Netherlands Institute for Advanced Study. However, here is proof once again that one cannot judge a book by its cover, subject matter, orientation, or even geographical provenance.

This is a very ambitious book; in fact, one of its great faults is that it is too

ambitious. In Part One the authors deal with most of the major sociological schools, or theories. Topically the coverage is fairly complete, although one might quibble over such things as the inclusion of hermeneutic sociology and the exclusion of exchange theory. The problems lie in the substantive coverage of each theory. Little more than ten pages is allotted to analysis *and* critique of each theory. The result is highly selective and superficial coverage. Making matters worse is the fact that the authors all too often rely on secondary sources rather than original material. For example, they use Denzin's very weak and controversial summary of ethnomethodology and a Polish language summary by Wesolowski of Davis and Moore's theory of stratification (both authors are apparently Polish by birth and training). It may be, at least in part, the reliance on secondary sources that accounts for the extraordinarily high number of egregious errors, of which the following is just a sampling:

- We are told that Durkheim wrote *The Rules of Sociological Method* in 1919 (it was 1895).
- One of the most famous statements in modern sociology, Merton's functional paradigm, is supposed to be "relatively little known."
- Sociologists are purported to have neglected Talcott Parsons' application of functional analysis to the study of general and concrete social structures.
- The authors argue that there are "no obvious features" that distinguish the Frankfurt school and that it has already lost its intellectual momentum.
- The authors are apparently unaware that Alfred Schutz has been dead for more than twenty years. They say, "since 1943 he [Schutz] has been lecturing."

This list could be lengthened considerably.

The highlight of Part One, and perhaps of the book, is the junior author's summary statement on multidimensional sociology. (Each chapter is credited to one or the other of the two authors. My general impression is that the essays by Lós are the better ones, although certainly not without faults. This pattern of alternating authors makes the book more inconsistent than it would have been had the authors collaborated on all the material.) The point of this chapter is that pluralism in sociology offers many advantages and the objective is for the field to take fuller advantage of its multidimensionality. This point is also made clearly by both authors in their brief introduction to the book. They also recognize the possibility of developing a more integrated paradigm, but deem that a far less realistic objective than more creative use of pluralism in sociology. While this is the case, I do not believe, nor do the authors, that this means one should give up efforts at greater integration.

Part Two, entitled "Social Structure and Consciousness," is supposed to build on the multidimensional theme, especially the relationship between the objective and subjective dimensions of sociological analysis. Although this theme recurs throughout, this part of the book is devoted to a rather loosely connected set of concerns including small groups, reference groups, ethos, social control, and deviance. While the reader can find a useful idea and even an insight here and there, on the whole this part of the book is badly written (as is the rest of the book for that matter), confused, and pretentious. I wonder whether we really need such fancy new concepts as "meta-attitudes," "tertiary social control," and best (or

worst) of all "incipient relative naturalism," the definition of which reflects the "lucidity" of much of the book:

the apprehension of a given stage of development of some process or of its place in the total structure of a given system which, when separated from its genesis inclines to regard that stage or place as an independent, idiopathic element, as a principal matrix of comparison unconnected with systems of reference which determine its other, previous stages of development or other possible aspects of its place in the totality of the given structure.

Not content with having dealt with all sociological theories, the relationship among them, and a number of subareas within sociology, the authors turn in the last part of the book to applied sociology, "sociology and practice." This is another loosely connected set of essays, ranging from sociotechnics to the intelligentsia to global ethics. There is not much that is very practical here, but we are treated to the authors' dreams of a new world in which the intelligentsia will save us from social nihilism by helping to create a new system of global ethics, to embrace not just all human beings, but all living creatures.

Evident through the entire book is the authors' background and experience in Polish society. Most of the illustrative material is drawn from that world. In the end, the authors admit to "have been struggling . . . to combine their unique East European experience with the academically elaborated Western rhetoric." While such a struggle is both useful and laudable, the authors seem to have struggled largely in vain.

**Power and Powerlessness: Quiescence and Rebellion in an Appalachian Valley.**  
By John Gaventa. Urbana: University of Illinois Press, 1980. 267 pp. \$16.50.

*Reviewer:* JOHN B. STEPHENSON, *University of Kentucky*

John Gaventa accepted an enormous challenge in undertaking this book, but he was uniquely qualified to do it, and the result is an excellent and invigorating achievement. The question to which this case study is addressed is why democracy does not work as certain theories say it should, especially in settings characterized by tremendous inequalities such as Central Appalachia. Having read the theories about open systems which allow the dispossessed to take action freely in their own interests and to compete in a political "fair fight" in working toward compromises among equals, Gaventa contrasts this model with what he finds in the mountain valley which is the subject of his book: inequality, quiescence, powerlessness. What requires explanation is not so much the occasional acts of rebellion, protest, and conflict, but the consistent pattern of quietness in the face of prodigious injustices.

For this theoretical framework, Gaventa emphasizes the concept of the third dimension of power, an idea taken from Stephen Lukes' work. The first dimension of power is the "pluralist" idea that political systems are open marketplaces where political interests compete more or less fairly. The second dimension, developed in part by Bachrach and Baratz, deals with ways in which political participation is structured out (the "mobilization of bias") so that political interests do not share equal access to decision-making and policy arenas. The third dimension of power calls attention to processes which prevent the powerless from formulating understandings of their own interests: if political interests are not identified in the first

place (because, let us say, the people do not possess the language and concepts for analysis), then structural access cannot be mobilized, and pluralist-type competition is precluded. Gaventa maintains that these three faces of power are cumulative and complementary in their effects.

The theory is amply illustrated in this sparkling historical and contemporary (up to about 1975) study of a coal mining area in the Tennessee-Kentucky Cumberland plateau area near Middlesboro, Kentucky. The history itself is fascinating enough without the political analysis. Middlesborough, the Magic City, begun in the late nineteenth century with English capital by a visionary Scottish developer named Alexander Arthur, gives the lie to the myth of isolated Appalachia. From the beginning, it was a coal colony owned by and operated for the benefit of its London owners, assembled under the corporate name of the American Association, Ltd. The Association's tight control of the area was reinforced by an ideology of progress and its attendant benefits. Mountaineers were transformed into miners, and the dominating ideas of the day did not lend themselves to an understanding of their new powerlessness.

The analysis proceeds more or less chronologically through chapters recounting the quelling of the rise of unionism, the autocratizing of the UMWA, and, more recently, the foiling of varied attempts at grassroots community development, in some of which Gaventa was himself centrally involved.

The book is methodologically provocative as well, suggesting that there are solutions to the thorny problem of imputing motives, interests, and values to politically silent populations. If quiescence is a product of the exercise of power, for example, then the silence should lessen when power weakens. Or, for another example, the responses of the powerful to challenges should indicate whether the more normal state of quiet is partly a product of power.

Gaventa is comprehensive in applying this three-dimensional framework as he illuminates everything from the Battle of Evarts to the Yablonski murders and the construction of a Holiday Inn at Cumberland Gap. As an explanation, it may border on the monofactorial, but Gaventa's interpretation helps the reader comprehend and reinterpret incredibly diverse qualities and events in Appalachian history and contemporary life. Its application, of course, extends quite naturally to other places and times: northern and southern mill towns, plantation life, and company towns generally, for example.

The major contribution of the book is that it redefines the research question about "cultures of silence" so that their origins are not sought in false consensus but in power relationships. It is, as Gaventa notes, a case study; though inclusive, it is not exhaustive, and much work remains to be done in showing the important roles of such institutions as family and religion.

Gaventa's work shows how necessary it is to understand the historical antecedents of contemporary powerlessness, and it demonstrates the importance of viewing the nature of power "from the bottom up." This book represents an important breakthrough in studies of powerlessness in Appalachia and elsewhere.

**Property, Paternalism and Power: Class and Control in Rural England.**

By Howard Newby, Colin Bell, David Rose, and Peter Saunders. Madison: University of Wisconsin Press, 1978. 432 pp. \$27.50.

**Social Change in Rural England.**

By Howard Newby. Madison: University of Wisconsin Press, 1980. 301 pp. \$21.50.

Reviewer: LAWRENCE BUSCH, University of Kentucky

Over the past several years, rural sociology has undergone a major transformation. Rightly known for a tendency toward "dust-bowl" empiricism in the past, much recent work has been better grounded theoretically, more likely to employ a wide variety of theoretical and methodological approaches, and more willing to probe hitherto neglected topics. These two books (along with a third volume by Newby, *The Deferential Worker*, a study of farmworkers) represent major contributions to the "new" rural sociology.

In *Property, Paternalism and Power*, the authors state that "it is a premise of this study that property is at least as important a feature of the stratification system of modern Britain as the distribution of income and authority." Since, as in the United States, property has been largely unstudied, the authors set out to rectify the problem by (1) examining the ways in which property ownership confers authority on the ruling class in rural England, and (2) illustrating "how various justificatory theories concerned with the legitimacy and domain of the distribution of property, once they have entered our stock of knowledge and vocabulary of understanding, retain their essence and are transmitted down the generations and across classes, to reappear as unthinking 'statements of fact.'" In short, the authors wish to examine how class relations are organized as well as how they are legitimated and reproduced.

Despite the aristocratic, landowning tradition of rural England, the picture the authors paint resembles rural America. Farm size is increasing, with a small number of large farms accounting for most production. Land prices are inflated, in part due to speculation. Taxation encourages investment in land. State intervention in the agricultural economy is commonplace. Farmer organizations see the Ministry of Agriculture as "their" ministry. Agricultural labor has been "decasualized": i.e., it is now largely a year-round labor force, though isolation makes labor organization difficult and keeps wages low. Large landowners tend to dominate local politics. Urbanites are moving to picturesque country villages, forcing the local population out and creating serious housing shortages. And the environmental movement has pitted urbanites against farmers, who spray crops and rip up hedgerows. Only the paternalism and links between farmers and the national upper class distinguish this description from similar pictures of American agriculture.

The authors focus on farming in East Anglia, drawing three separate samples: (1) a random sample from 44 parishes, (2) a sample of farmers who employ farmworkers (used in Newby's previous study, *The Deferential Worker*), and (3) a sample of farmers with holdings of 1,000 or more acres. In each case, personal interviews were conducted and many questions were left open-ended, insuring a wealth of qualitative data. The latter are liberally sprinkled throughout the volume

in the form of short quotations to illustrate particular points, a particularly effective device, although purists will be upset by the authors' penchant for mixing statistical, dramaturgical, and critical methods. Those not so concerned will be delighted by the three-dimensional character of the study.

A treatment of the relations between ideology and class structure goes a long way to explain the "conservatism" of the countryside. The authors note that, despite a fair dose of paternalism and the restricted mobility afforded by life in a small village, farmworkers tend not to believe in the ideology of the ruling class (i.e., of farmers). However, they point out that ideological manipulation is generally sufficient to neutralize the effects that deprivation has on most rural workers. So well organized is the ruling-class ideology—for example, local politics are seen as nonpartisan, and large farmers often present themselves as solely husbanding the land for the public good—that others are unable to organize an effective opposing worldview. Farmworkers tend to identify more with farmers than they do with workers in industry. And they blame their low wages, despite the very comfortable lifestyles of their employers, on a national cheap-food policy.

There are a few limitations to this volume. First, it is confined solely to arable (the English term for crop) farming. While no detailed information is presented, the authors lead the reader to suspect that class relations are quite different in the more pastoral parts of the country. A second limitation is an unfortunate tendency to take scientific and technical change as a given rather than examining how it fits into the class structure and ideology the authors describe. Finally, despite the concerns of the volume, there is only one reference to Habermas, and no references to Gramsci.

*Social Change in Rural England* is in many respects a summary of the two previous volumes. Ostensibly written for a broad, nonacademic audience, it surveys the whole of rural England, including a useful historical review of changes over the last hundred years. This volume would be particularly useful for an upper-level undergraduate or a graduate course in social change or comparative sociology. However, despite a lack of footnotes and some amusing political cartoons, it remains too analytic for the general reader. It successfully integrates the literature on farming with questions of community structure, return migration, and the rural environment. It also shows how planning has created a new group of problems as it has resolved others. The effect of newcomers on the established patterns of rural life is particularly well treated. As Newby says early on, "rural life has become the Good Life with a vengeance." Newcomers compete all too successfully for services, look outside the village for social life, and insist that the village remain "picturesque, ancient, and unchanging." Ironically, this has the effect of destroying the very qualities that made village life desirable in the first place. The farmworkers identify more and more with farmers and, as a result of the short supply of housing, move to "tied" housing on the farm. And the ancient village becomes little more than a high-class bedroom community.

In conclusion, both volumes offer important contributions to our understanding of class structure, ideology, and social change. Moreover, the authors successfully avoid comparing rural and urban society; instead they show how rural England has become dependent on urban *institutions*, in a reversal of the pattern that held for centuries before.

**The Sociology and Politics of Development: A Theoretical Study.**

By Baidya Nath Varma. London: Routledge & Kegan Paul, 1980. 219 pp. \$25.00.

Reviewer: ROBERT E. WOOD, *University of Massachusetts at Boston*

A promising feature of this book is its multidisciplinary approach. Varma, a sociologist, ambitiously sets out to present "a comprehensive model of modernization" which can be "applied to all countries of the world." This is no mean task, and as Varma notes: "The field of modernization studies is growing enormously and at such a fast pace that it is difficult to keep track of the latest developments."

Alas, this seems only too true with regard to this study. What Varma actually provides is a tour of 1950 and early 1960 thinking about what was then optimistically called "modernization." All the names of the figures who staked out what came to be called modernization theory are there: Pye, Lerner, de Sola Pool, Eisenstadt, Smelser, Shils, Kuznets, Singer, Rostow, Hirschman, Hoselitz, Almond, Verba, Deutsch, Redfield, Lewis, and others. The heart of the book is four chapters surveying sociological, economic and political, anthropological, and "activistic" theories of modernization. What is strange about this survey is that the intellectual history of modernization theory seems to end by the late 1960s—as do the dates of the vast majority of Varma's footnotes. Absolutely no hint is ever given of the fact that this brand of modernization theory has been under sustained attack for over a decade, and that it has been deserted by almost all younger scholars in the field, to say nothing of activists in the Third World.

This lack of even passing mention applies to the critiques of modernization theory both from the right and from the left. Samuel Huntington's assertion that not political modernization, but political decay, characterizes much of the Third World; Clifford Geertz's work on agricultural involution; Reinhard Bendix's careful critique of the assumptions of modernization theory—all are as totally ignored as the works of Andre Gunder Frank and the dependency theorists, Immanuel Wallerstein and world systems analysis, James Petras, Arghiri Emmanuel, Samir Amin, and many others. The reader gets virtually no flavor of the contemporary debates concerning development, because Varma seems oblivious to the paradigm shift which took place in the 1960s and 1970s.

Because Varma claims to provide the reader with a comprehensive review of theories of modernization, I think it is legitimate to criticize him for what he has left out. Unfortunately, even within its own terms of reference, the book has serious shortcomings. Despite Varma's attempt to provide a "model for the general paradigm of modernization," the book lacks theoretical focus and coherence. Even when potentially interesting issues are raised (such as planning), the discussion is so general that little of use can be gleaned from it. Although Varma happily does not share the reflexive anti-Communism of many of the modernization theorists he relies on, his bland statements that "Modernization is a continuing process which never stops for any country," that "both systems of modernization (capitalist and socialist) qualify as democracy," and that "For the first time in history, the modernizing countries have a choice between these two viable models or any variant thereof," are so indeterminate as to be essentially meaningless.

Immanuel Wallerstein has observed: "We do not live in a modernizing world but in a capitalist world." The possibility of capitalist underdevelopment as a



continuing process is not part of Varma's universe, nor are the severe constraints imposed on those Third World countries which opt to pursue socialist strategies. In only one paragraph in his whole book does Varma even hint at the need for an analysis of the impact of the international order on the course of development in Third World countries. Absolutely no mention is made of the increasingly urgent calls from Third World nations for a New International Economic Order.

As a work in the history of ideas, *The Sociology and Politics of Development* provides a reasonably comprehensive survey of the basic ideas of the social scientists of the 1950s and early 1960s who gradually evolved the corpus of work which came to be known, especially to its critics, as modernization theory. But for the rest of the story—what happened when a new generation of scholars and activists confronted this theory and elaborated alternatives—the reader will have to look elsewhere.

### **African Businessmen and Development in Zambia.**

By Andrew A. Beveridge and Anthony R. Oberschall. Princeton: Princeton University Press, 1979. 382 pp. \$22.50.

Reviewer: CALVIN A. WOODWARD, *University of New Brunswick*

The study of middle-range and lower levels of economic enterprise and development in post-colonialist societies has been somewhat overlooked in the rush to comprehend the macro-economic aspects of nation-building. Books like the one under review, which help to redress this imbalance, are therefore welcome and important even when their empirical scope is narrow. Beveridge and Oberschall address themselves primarily to three large and connected topics about which empirical data are scarce but theoretical supposition extensive. One has to do with the sociology, drive, and success of emergent business classes in Third World nations. Another focuses on the relation between private and public enterprise as these are determined by development strategies. In this context what Beveridge and Oberschall want, on the one hand, is to clarify the implications of the growth of an indigenous business class for a developing economy; on the other, they seek to contribute to knowledge regarding the impact of government policy on business growth, social structure, and political systems. A related objective is to see how the emergence of a native business elite can help a new nation move away from dependence on foreign skills and capital without sacrificing economic growth.

In looking at the development and achievements of African entrepreneurs in Zambia from this broad theoretical vantage, the authors come up with some useful findings of a general nature. It is important, for example, that in Zambia, ethnic, religious, and socioeconomic considerations have played far less of a role in regard to entrepreneurial success than have innovative enterprise, sound business management, and similar professional and cross-cultural dynamics. But the greater contribution of this book lies in its descriptive analysis of the Zambian case itself. It shows that in the years since independence the near-monopoly of small business enjoyed by the Asian population in Zambia has been cut into, to the extent that now 75 percent of retail output and 10 percent of the economy are in "private African hands." While this shift in the ownership of businesses is significant, the

growth of African entrepreneurs has had little effect on the overall control of Zambia's economic order by foreign and domestic oligarchies. Copper is undisputed king in Zambia and this industry has remained almost fully under European control and management. Moreover, a large number of secondary and tertiary industries in Zambia are linked to copper. Consequently, this economic giant influences the sociological infrastructure of Zambia to such a degree that emergent networks provided by the growth of African entrepreneurs have changed the general picture only slightly, if at all. Nor has that development altered the level of inequality in Zambia. Beveridge and Oberschall do not blame African businessmen for Zambia's turning away from the egalitarian dreams that inspired its nationalism. That divorce, as they contend, has been a historical fact even in socialist countries. They conclude simply that the "limited replacement of privileged Europeans and Asians by privileged Africans in business as well as in other spheres of society does not constitute fundamental change."

The book is empirically sound and the story is interestingly told. It is based on intensive fieldwork and relies heavily on interviews and questionnaires. But the book is replete with accounts of personal experiences and vignettes, thus sparing readers the dull recitation of data which so often characterizes the reporting of this kind of research.

Theoretically, however, the study is not strong. For one thing, the general utility of its findings is compromised by the authors' failure to develop clearly their theoretical focus. They introduce the work by listing "some questions" of general concern and by reviewing a *potpourri* of theoretical interests and priorities which have been involved in the study of economic development in new states for more than a generation. But the analysis is not coherent and it does not involve the formulation of rigorously worked-out theoretical questions. As a result, the relation between data and theory is not consistently clear, and one cannot be sure of the more general meaning of critical aspects of the Zambian experience. The work had the potential of taking us further than it does.

### **Conflict and Conflict Management**

*By Joseph S. Himes. Athens: University of Georgia Press, 1980. 333 pp. Cloth, \$23.00; paper, \$9.00.*

*Reviewer: EDWARD J. WALSH, Pennsylvania State University*

This book, a thoughtful functional analysis of social conflict from what has been called a "breakdown"—as contrasted with a "solidarity"—perspective, summarizes and attempts to synthesize much of the accumulated knowledge in "both the science of conflict and the technology of conflict management." The first part of the book attempts to answer the question of what sociologists have learned about social conflict. The second part examines various modes and techniques of conflict management.

Chapter 1 presents the author's typology of social conflicts, distinguishes "legitimate" from "nonlegitimate" conflict, and presents a brief historical summary of sociology's ambivalence toward social conflict. Chapter 2 summarizes various conflict theories and sketches an "explanatory model." Chapters 3 and 4 discuss

social conflict from structural and power perspectives, respectively, and Chapter 5 focuses on violence as the leading form of "nonlegitimate" conflict. Chapter 6 presents a succinct summary of the functions of conflict, and Chapter 7 examines the possibility and role of prediction in the study of social conflict.

The second part of the book begins with a chapter on "instrumental conflict," a term the author uses to refer to "the deliberate, purposeful, tool-like character of realistic or substantive conflict." The final four chapters examine strategies groups use to manage such conflict. Chapters 9 and 10 deal with the prevention strategies of elimination of causes and institutionalizing the struggle. Chapter 11 focuses on conflict resolution, and Chapter 12 on the suppression of social conflict.

While acknowledging that social science predictions of conflict may be misused by authorities to repress legitimate dissent, Himes uses his model from Chapter 2 to derive a "strategy for conflict prediction" in Chapter 7. Despite the failure of social scientists to anticipate the turmoil of the 1960s, especially in the United States, he argues that "the social sciences have a long and impressive (though not perfect) record in social prediction, including the prediction of the onset and termination of nonlegitimate conflict." His own predictive model derives from the idea that social conflict appears as a byproduct of processes of breakdown in a society. The model is necessarily crude, but the author offers an interesting *post hoc* application to the case of Soweto.

In another section of the book, Himes presents an analysis of the black power mobilization processes which emphasizes the grievances and resources within the black collectivity itself. His discussion does not include any reference to the "resource mobilization" perspective in the social movement literature, which is really quite relevant in this connection. Whereas resource mobilization theorists would stress the critical importance of mainstream white support in explaining the insurgency of poor blacks during the 1960s, the author prefers an explanation based the mobilization of resources within the black community itself. This difference of emphasis is not due, I would suggest, to the fact that Himes happens to be black while the resource mobilization theorists he cites happen to be white. Rather, it derives from the complexity of any widespread mobilization process which necessarily involves both macro- and micro-processes. Himes emphasizes the micro-processes in his discussion, but tends to ignore societal factors such as the 1954 Supreme Court school desegregation decision or the increase in white middle-class affluence. Resource mobilization theorists, on the other hand, tend sometimes to ignore micromobilization processes in their emphasis on societal factors facilitating and impeding protest organization.

This book brings together a wide variety of research traditions in the effort to present a summary of current social-science thinking about conflict. The discussion is necessarily somewhat rambling precisely because of the inclusion of so much material. Numerous typologies are introduced, only to remain unused. The resource mobilization perspective which, to me at least, seems relevant to this discussion is virtually ignored. An Introduction and Conclusion, elaborating some of the ideas now included on pages 2 and 166, as well as other general themes, would have been useful for most readers, and it seems to me that as much as a third of the material could have been omitted without affecting the considerable value of this book. A single paragraph summary at the end of each chapter and a more thorough index would also have been helpful.

Despite these flaws, however, Himes has performed a real service for social scientists by collecting so much material (his bibliography is 45 pages). The book, or sections from it, could be used in senior and graduate courses in political sociology, social movements, collective behavior, and related topics.

**Mutual Accommodation: Ethnic Conflict and Cooperation.**

By Robin Williams. Minneapolis: University of Minnesota Press, 1977. 413 pp. \$15.00.

Reviewer: CHARLES U. SMITH, Florida A&M University

Robin Williams has long been recognized as one of the most astute and thorough scholars in the field of intergroup relations. His earlier work, *The Reduction of Intergroup Tensions*, clearly demonstrated his ability to analyze enormous quantities of research findings, and to summarize them succinctly into operable strategies and guidelines for public policy.

Similarly, in *Mutual Accommodation*, Williams tackles the monumental task of surveying and assessing the vast and diverse research literature and other writings dealing with interracial and interethnic relations in his search for consistencies, uniformities, and patterns from which sound generalizations can be derived. He attempts to appraise both social harmony and social conflict as significant realities in historical and contemporary intergroup relations, in an effort to identify and extract valid principles that impact on a "hopeful" prognosis for the United States as it pursues its ideal of democracy as a maturing society.

*Mutual Accommodation* is clearly optimistic in its outlook for interracial and interethnic relations in the United States. While recognizing the need not to ignore negative findings, Williams candidly observes that, in the face of much pessimism, his concern with "positive" aspects of intergroup relations might be regarded as heretical, but might also be refreshing to many who have not encountered some of the data he presents.

The book's tone is established in the very first chapter, titled "It Can Be Done." Using empirical data from a variety of sources, the author shows how positive changes have taken place in such areas of intergroup relations as school desegregation, residential access, political participation, economic and educational attainment, and even self-esteem and minority group morale. In subsequent chapters dealing with the range of issues and problems in intergroup relations, including strategies and processes for addressing conflicts, tactics for group action, and the use of constraint, inducement, and persuasion, Williams distills the existing knowledge and wisdom into "propositions," sometimes followed by "recommendations" and conclusions, supported by outlines or specifications of procedural steps or conditions necessary for effective action.

As a reviewer, I found *Mutual Accommodation* more difficult to read than most books I have encountered. This was true partly because of its length, which was probably unavoidable, due to the great bulk of materials included or analyzed. The greater difficulty, however, was due to the unusually small type size and the poor contrast between the print and the pages. It may be that printing costs and the necessity to economize are the culprits in this situation. If so, it is truly unfortunate that it had to happen to a book of this caliber.

Despite this reservation, Robin Williams' work is outstanding. Some might argue that the book is unduly optimistic, but a careful reading reveals that the author consistently identifies pernicious, if not insolvable problems, with appropriate caveats to balance his positive projections. I concur completely with Williams' conclusion that "The fact that some basic problems are not solvable, in our time and place, does not imply that we should give up in the face of problems that may be open to at least partial solution. . . ." *Mutual Accommodation* will not be the easiest book on intergroup relations in the United States to read, but for those interested in effectively pursuing the ideal of American Democracy, it should not be overlooked.

**Racial Prejudice, The Individual and Society.**

By Christopher Bagley and Gajendra K. Verma. Westmead, England: Saxon House, 1979 (Distributed in U.S. by Lexington Books). 235 pp. \$19.00.

Reviewer: ALVIN W. ROSE, University of Miami

In a skillful, thoughtful synthesizing of the findings of some 505 studies (all but about eight published since 1950) these two British scholars, while offering almost no new theory, have nonetheless given us both the finest summary interpretation so far of this third quarter-century's work in the social psychology of racial prejudice and a fairly good view of contemporary British racial and ethnic relations.

This is the second of two volumes in which the authors seek to interpret racial prejudice in terms of the tri-level relations of personality, culture, and social system. The first volume was an essay on the role of culture in the etiology and dynamics of prejudice: suggesting, for example, that among British and Dutch samples, with comparable demographic and psychological characteristics, the British have significantly higher levels of prejudice, because of the deep-rooted cultural traditions of racism in Britain. This second volume is a consideration of social-system variables (class, status, mass media, etc.) that mediate between attitudes and behavior, between prejudice and discrimination.

A good definition of prejudice is given in the discussion of "multiple approaches to the study of prejudice" which introduces the book's six chapters:

Prejudice is a negative attitude towards ethnic or minority groups held (in the absence of, or in the face of, evidence to the contrary) with various degrees of salience and cognitive elaboration, and with various cognitive, affective and behavioral readiness aspects implicit in the attitude. These attitudes can be expressed implicitly, at the institutional level, or explicitly in negative or hostile behavior towards the ethnic groups concerned.

The first chapter presents a three-fold summary of research findings on racial attitudes. In the first place, expressed attitudes *are* behavior, sometimes of considerable consequence, as, for example, in referenda, polling, or in public speeches that may legitimize, even trigger, hostile acts. Second, the many problems in measuring attitudes flow from such factors as (1) unreliable instruments (as in dissimilar results from Guttman and Likert scales); (2) time lag between eliciting attitudes and the behavior; (3) unidimensional measurement of attitudes which almost always are multidimensional; (4) failure to view attitudes phenomenologically; (5) public vs. private selves (Turner's self-image/self-conception); (6) toler-

ance of one's own hypocrisy (saying one thing while believing another); (7) conflicting values sincerely internalized; and (8) the role of social situations. In the third place, almost all the studies in the 1950s, 60s, and early 70s of attitudes toward West Indian and Asian residents in England show about 20 percent of the British to be extremely prejudiced; 20 percent accepting, tolerant, or pluralistic; and 60 percent assimilationist, accepting immigrants only if they "abandon their traditional way of life and settle down." While the higher salience of such concerns as the "Irish problem" have caused these percentages to remain fairly stable, more recent studies suggest a possible negative drift in British racial attitudes. Marsh found 25 percent of the British extremely prejudiced, only 13 percent unprejudiced.

Chapter 2 looks at the relations of age, sex, education, and social class to racial prejudice. Prejudice increases with age (unencumbered with other status factors)—though the reasons for prejudice may differ in the several age phases (competition in the middle-aged struggle for goal-fulfillment, alienation among the senior citizens).

Based on inferences from the Wilson-Patterson conservatism scale, young women are less prejudiced than men, but as age increases, females, particularly married females (probably because of increased anxiety), become much more prejudiced.

While the relation of education to prejudice is complicated, and educational quality and content are more important than quantity, the raw correlations do show education and prejudice to be inversely related. The extreme tentativeness of this conclusion is stressed by these authors, just as it has been in the past. As usual, we have studies with nonconfirming if not precisely contrary findings.

The relation between socioeconomic status (primarily occupational status) and prejudice tends toward curvilinearity. Thirty-four percent of Class A (the professionals) were prejudiced, 35 percent of Class B (intermediate professionals), 44 percent of Class C-1 (clerical), 50 percent of Class C-2 (skilled manual), 50 percent of Class D (semi-skilled), and 48 percent of Class E (unskilled). However, the middle class (the institutional gate-keepers who control the points of entry for minorities), while probably not as prejudiced as the lower classes, actually practice discrimination more than either the lowers (among whom there is little to lose) or uppers (for whom minorities pose little if any competition).

This conclusion about prejudice and social status may represent some modification of earlier interpretations. An early summary was that "the most common finding is that individuals of low economic status are most likely to have unfavorable attitudes toward Negroes."

What is thought to be little if any racial conflict in Brazil is partially explained in terms of Lenski's rank equilibrium theory (i.e., high status crystallization in education, income, and race). Racial conflict in the United States, on the other hand, is unsurprising because blacks are said to be making some progress in education, and even in income, while their relative ethnic status remains low. The elaborations of rank equilibrium theory by Homans, Geschwender, and Runciman, along with its usefulness and limitations in studies of prejudice, are faithfully summarized.

Similarly, studies in relative deprivation theory (Merton, Pettigrew, Vanneman, Runciman, Gurr, Bagley, et al.) using data from South Africa, the United

States, England and Rhodesia, and employing the subcategories of fraternal and egoistic deprivation are imaginatively criticized.

In at least two major regards, conclusions to be drawn from the social-mobility prejudice hypotheses initiated in mid-century by Bettelheim and Janowitz and by the Adorno studies are unclear. While the downwardly mobile seem to be more prejudiced than the upwardly mobile, there is considerable question as to (1) the adequacy of the additive model; and (2) the significance that culturally cushioned contexts (as in Sweden) may have for determining the relationship between mobility and prejudice.

The writers are impressed with the possibilities of Gibbs and Martin's status integration theory for the study of prejudice, particularly the concepts of achieved versus ascribed atypical status integration. But they may be betrayed by an innocence of recent and rapid changes in American attitudes and values. An example of achieved atypical status integration in America, they suggest:

would be a young, high status female engineer who was divorced and had no religious affiliation. Such a combination of statuses could only occur as a result of individual mobility in a relatively open society, and certainly would be unlikely to occur in the states of Virginia, Kentucky, Georgia, Tennessee, North Carolina, Oklahoma, Arkansas, South Carolina, Alabama, Louisiana and Mississippi, which have a high prevalence of authoritarianism. Individuals would be more likely to move into atypical combinations of status in these states—California, Washington, Indiana, Kansas, Connecticut, Ohio, and New Jersey—which have a low prevalence of authoritarianism.

The literature from South Africa (John Ray, John Myrhart, Patrick Heaven) which questions the presumed relationship of F-scale responses to prejudice has appeared since this publication. Nevertheless, the authors report that the British data did show a high correlation between status integration (married, skilled-manual, basic education, male), authoritarianism, and prejudice.

The analysis of the relation between mass media and prejudice concludes that the media serve to support racial opinions already existing in the larger population.

It might have been preferable if the authors had resisted the tradition of concluding books on prejudice with a chapter on ameliorative strategies. We are told that people should become educated.

More regrettable is the failure to consider the relation of power to prejudice. The military, legal, and political usages of power are the big story in this third quarter-century of racial and ethnic relations, and this is an area that social psychologists have not ignored. Military expeditions, police actions, guerilla warfare, and terrorist activities have been important aspects of racial and ethnic relations in recent years and are now widespread, worldwide. Similarly, Higginbotham's marvelous summary of legal power and prejudice is hardly more than a look at the top-level, more official dimensions, below which are the riots, protests, demonstrations, boycotts, "sit-ins," and committee meetings which comprise many of the day-to-day concerns in the field of racial and ethnic tension. And the politics of prejudice, whether in a national or local election in the United States, England, South Africa, or Israel, are also important. Bagley and Verma consider none of these military, legal, and political parameters of power. But these must somehow be

**Violent Criminal Acts and Actors: A Symbolic Interactionist Study.**

By Lonnie H. Athens. London: Routledge & Kegan Paul, 1980. 104 pp. \$11.50.

Reviewer: JUNE KRESS, *Institute for the Study of Labor and Economic Crisis, San Francisco*

In an era when fear of rape is on the minds of a significant proportion of the female population, when an average of nearly two citizens are killed by the police each day, and when the senseless killing of black children has lately thrown an entire city into desperation, it is incredible that a book which purports to be a "radically new view of violent criminality" barely mentions the cancer of crime spreading throughout urban America—hitting hardest at minorities and the working class in general. Lonnie Athens' new book may examine the phenomenon of violent crime, but it does so in a manner so detached from reality as to hinder any understanding of what millions of Americans face every day.

The book consists mostly of interviews with offenders convicted of violent crimes. Athens' sample of 47 men and 11 women were asked to describe their violent behavior for the purpose of interpreting such situations. The result is a complex classification system modeled on the work of George Herbert Mead and Herbert Blumer (who has written the Foreword to the book).

In Chapter 1 Athens criticizes the dominant approaches to the study of violent criminality, especially positivism, which he argues does not take into account man's role as *actor* in the commission of violent crime. This is followed by a brief explanation of the theoretical basis of the study, symbolic interactionism, an approach which rests on the premise that human action always takes place in a *situation* and that actors act on the basis of defining the situations that confront them. Athens then constructs numerous typologies ranging from one dealing with interpretations of violent criminal acts ("physically defensive, frustrative, malefic and frustrative-malefic") and "near violent" acts (affected by "fixed line of indication, restraining judgment and overriding judgment"), to those of self-images held by violent criminal actors ("violent, incipient violent and non-violent") and of their career patterns ("substantial violent, unsubstantial violent and negligible violent" leading to careers referred to as "stable, escalating and de-escalating").

Athens' summary and conclusions, all of two and a half pages, attempt to make some sense out of the endless categorizing. He concludes two things: that people who commit substantial violent acts have "different generalized others" (they take on the role of others with whom they interact) that may or may not provide them with moral support for acting violently, and that these "generalized others" may change over time. Therefore, Athens asks, "(1) to what extent . . . can the moral support for acting violently toward other persons . . . be codified into a specific set of norms or shared rules prescribing violent conduct? [he does not say why we should undertake this]; (2) regardless of the extent to which [this] can actually be codified . . . what is the nature of the social process which promotes their development among certain members of society? [he does not specify who]; and (3) once developed, how can violent generalized others be transformed into non-violent ones? [he does not even attempt to answer this question]."

Keeping straight all of the typologies is a job in itself, but there are far more serious limitations to this book. In his Foreword, Blumer points out that Athens' findings "suggest ways of exercising effective control of violent behavior." But in



fact, Athens offers no strategies, no tactical insights, no goals, no ideas about how working class people can deal with the problem of crime. He advocates neither hard-line conservative penal practices nor the more liberal band-aid reform approach to slowing the spread of violence.

A second weakness has to do with Athens' lack of scholarship. His critique of the literature does not even mention the body of knowledge grounded in Marxist methodology and the pioneering work that has been done on violent criminality from a materialist perspective, such as Paul Takagi's studies of police killings (1974, 1977), Dorie Klein's treatise on battered women (1979), or the recently reprinted study from 1965 which shows that intra-class crime sharply decreased in the South as a result of civil-rights organizing (1980). (All of these studies are reported in *Crime and Social Justice*.) One must doubt that such omissions are due to ignorance, since Athens was a graduate student at the Berkeley School of Criminology, a leading center of Marxist criminology before its forced closing in 1976; it seems rather a conscious unwillingness even to consider the scientific merits of Marxism in examining crime.

The most serious shortcoming of the book is the way the subjects are completely disassociated from the political economy that shapes their lives. There is no explanation of their class backgrounds or economic position; we get little sense of who these people are, how they have been dehumanized under capitalism, and why they, in turn, dehumanize those around them. Crime, to Athens, is a behavioral problem, and thus he uses abstract psychological terms to explain what is in fact a very concrete, albeit complex, problem.

While it is true that Athens cannot be placed among the rising neo-realist wing of criminology, his ideas are as conservative (and just as cynical) as those of Norval Morris or Ernest van den Haag. Athens in no way challenges the conventional criminological approach; he personifies it. And in a period when the crisis of Western capitalism has reached such proportions that the crime problem will only get worse, it is a pity that the realities of crime against the working class are not adequately addressed in this book. Rather than asking how we can codify shared rules (whatever these may be), we should be providing analytical materials to help set in motion an organized response on the part of the working class to austerity capitalism and to one of its most serious by-products, violent criminality.

**The Analysis of Educational Productivity, Volume I: Issues in Microanalysis.**

*Edited by Robert Dreeban and J. Alan Thomas. Cambridge: Ballinger, 1980. 273 pp. \$25.00.*

**The Analysis of Educational Productivity, Volume II: Issues in Macroanalysis.**

*Edited by Charles E. Bidwell and Douglas M. Windham. Cambridge: Ballinger, 1980. 265 pp. \$22.50.*

*Reviewer: MAUREEN HALLINAN, University of Wisconsin-Madison*

The eleven papers in these two volumes were presented originally at a conference sponsored by the Educational Finance and Productivity Center at the University of Chicago in June 1978. The five papers in Volume I present microanalyses of

educational productivity; they focus, for the most part, on the student within the classroom and discuss issues related to assessing schooling effects on learning. The six papers in Volume II are concerned primarily with how organizations and societal institutions affect the process of learning within schools. None of the papers is empirical; rather they present conceptual and methodological models that aim to go beyond current research on educational productivity.

In their introduction to Volume I, Dreeban and Thomas claim that the collection of papers represents "the emerging second generation of investigations in educational effects." First-generation investigations presumably include production function studies, such as the Coleman Report, that attempted to relate educational resources to academic achievement or educational attainment; status attainment studies, in the tradition of the Wisconsin model, that examine how schooling produces change in academic achievement or social mobility; and studies that relate variation in adult knowledge, attitudes, and behaviors to different amounts or kinds of schooling. The second generation, according to Dreeban and Thomas, moves beyond these three approaches to examine more closely the processes that relate school resources, student characteristics, and length of schooling, as well as the hierarchy of organizations in which the classroom is embedded, to student achievement and other outcomes. This review will respond to that claim by evaluating the volumes' potential for advancing the study of school effects.

While the study of educational productivity over the past two decades has been slow and discouraging, it has nevertheless generated more interest and debate, more systematic research, and more conceptual and methodological advances than perhaps any other area in sociology of education. At the same time, for the past few years, school effects research has stagnated, yielding few creative insights or models to improve research in the field. To what extent do these volumes serve to rekindle interest in school effects and to motivate a research effort comparable to earlier efforts?

The papers abound in criticisms of past work and cautions for the future. In Volume I we are alerted to the dangers of using standardized achievement data to measure learning, of employing economic models depicting a single output produced by many inputs (e.g., the Coleman Report), of not correctly taking into account the several levels of analysis that affect student outcomes, of not informing our models by classroom observations, and of proposing models hopelessly narrow in scope. Similarly, in Volume II we are told that we have not conceptualized educational productivity correctly, that we have failed to take into account the effects of societal change on education, that much past work lacks theoretical grounding, that our existing data sets cannot answer the questions we pose, and that while cross-sectional data are inappropriate to depict school effects, longitudinal data are difficult to analyze. These criticisms and cautions may paralyze some readers: one could conclude that it takes a foolhardy researcher to work in an area so fraught with dangers.

Recommendations for future research also abound. These suggestions come from different perspectives. The consensus of the authors in Volume I appears to be that what happens to students in the classroom is of paramount importance and that higher organizational levels such as the school, school district, or community

may have little effect on learning. The papers in Volume II assume that the social structures in which a school exists affect the amount of learning that occurs.

Three chapters deal with theory. Harnischfeger and Wiley focus on one link in their previously formulated model of school learning, namely, the relation between school or classroom resources and pupil opportunity to learn. They attempt to demonstrate that a teacher's differential allocation of resources to students is a result of teaching strategy rather than curriculum differences. Levin proposes a similar theoretical model, also based on Carroll's learning theory. He identifies teacher capacity, effort, and time as the determinants of learning and argues that a model of educational productivity must take these teacher variables into account rather than simply including teacher background characteristics. Meyer distinguishes between the school as an institution and as an organization. He focuses on the student role and formulates a set of hypotheses about student participation in the institution.

These chapters broaden our perspective on schooling by suggesting that we take societal factors into account and that we examine the processes that relate traditional input variables to student outputs. These arguments, as Bidwell and Windham point out, lead to comparative research in order to examine the interplay of societal, institutional, and organizational effects on educational productivity. On the other hand, the chapters fail to provide a new theoretical model to guide future research efforts. This is unfortunate, since the absence of a well-conceptualized model is, without a doubt, the major reason for the slow progress in explaining school effects. Furthermore, to understand educational productivity fully, greater attention must be paid to non-cognitive outcomes. It is curious that while several authors stress the importance of examining student outputs other than achievement, no such model is presented.

The remaining chapters, with one exception, deal with methodology. The exception is the last chapter in which Coleman discusses parent-child rights and lays out a plan for a voucher system. Aggregation issues are treated in two chapters. Burstein discusses the multi-level properties of educational data and outlines the major issues associated with specifying levels of analysis in a model of educational productivity. Rogosa presents some approaches to the analysis of longitudinal panel data, including hierarchical data. While useful, these two chapters would have benefited from greater integration. Brown and Saks review types of production functions and recommend a multiple-outputs joint production function model for studying school effects. Their paper is a reminder of the strong influence economists have had on the conceptualization of educational productivity. Felsom and Land propose a global societal indicators model that takes into account the effects of rapidly changing social situations on schooling. Heyns provides reasons for skepticism in the interpretation of research based on standardized test scores. The problems of scaling, distribution of test scores, and correlated errors that she identifies will more likely be solved by psychometricians than by school effects researchers. Eckland proposes a multi-purpose national data archive, based on a series of longitudinal studies beginning in first grade. Despite the attractiveness of this plan, it may not be feasible from a management point of view, not to mention the effects of decreased federal spending for educational research. Berliner avoids the methodological problems associated with survey research by

suggesting small scale observation studies of a clinical nature. Taken as a whole, these seven papers bring together current methodological insights into the design and analysis of school data and provide guidelines for more rigorous and sophisticated approaches to the collection and analysis of data in the future.

In short, these two volumes represent the "emerging second generation of investigations in educational effects" only if stress is placed on the term emerging. In the absence of further theoretical development, these papers can hardly be viewed as a major new effort in the study of educational productivity. Similarly, in the absence of empirical studies that rely on the analytic techniques discussed, it is hard to predict whether any more precise documentation of school effects is imminent. Nevertheless, the volumes may herald a new era. They represent a bridge between past research and what may emerge in the future. They point to what we know and what we still need to know to understand and explain the process of schooling.

**The Legacy of Aging: Inheritance and Disinheritance in Social Perspective.**

By Jeffrey P. Rosenfeld. Norwood, N.J.: Ablex, 1979. 158 pp. \$14.50.

Reviewer: FRED C. PAMPEL, *University of Iowa*

Inheritance laws in the United States allow relatively great freedom to bequeath estates outside one's family. Tocqueville predicted that this freedom would level class differences by redistributing the income of wealthy families. In practice, however, normative devotion to the family has maintained kinship as the primary basis of inheritance. Yet the potential for disruptive effects of nonfamilial bequests remains large. This monograph compares the social contexts and characteristics of those who leave their estates to their families and those who do not.

Rosenfeld identifies four patterns of inheritance, based on the presence or absence of obligations to kin (reciprocity ties) and to outsiders (complimentary ties). When obligations to kin are present and strong ties to outsiders are not, the result is familistic inheritance. When both exist, articulated inheritance—bequests both to family and nonfamily—occurs. When obligations to kin are not present, but ties to outsiders are, family members may be disinherited. And if ties neither to kin nor to outsiders are present, property goes to the state, a process defined as escheat. This typology of testamentary behavior provides the dependent variables in the study.

Variations in the dependent variables are studied within three groups: (1) residents of a geriatric nursing center; (2) residents of a retirement village; and (3) aged residents of an oceanside resort town on Long Island. The three settings are assumed to be characterized by three different geriatric life-styles: the nursing center by disengagement, the retirement village by proliferation of social ties, and the town by continued participation in families. Although the actual activities and social ties of decedents were not studied, Rosenfeld argues that the three contexts are sufficiently different to allow variations in testamentary behavior to emerge.

Rosenfeld examined death certificates, obituaries, autopsy cards, and morgue reports to identify those who died between 1967 and 1971. These names could be matched against court records, and characteristics of the wills and testaments could be coded. In addition to measures of the social relation between decedent and

beneficiaries, total assets bequeathed, and demographic characteristics were obtained from either the will or the death certificate.

Familistic inheritance was most common in the oceanfront community, where family contacts across generations were strongest. This indicates "that members of the family may be less apt to become beneficiaries when elderly benefactors die after being involved with cohesive peer groups, as in Golden Village [the retirement community], or after being isolated from the outside world, as in nursing homes or geriatric wards. . . ." Most of those who specified articulated inheritances came from the retirement village. While this suggests that the new social ties developed in the village led to bequests to persons outside the family, it could also be likely that those in the retirement village simply had greater wealth, which allowed them both to take care of their families and to make bequests to outsiders. Disinheritance and escheat were most common among residents of the nursing home, who may have resented being forced to live in a nursing home, may have lost touch with family members altogether, or may have entered the nursing home with less income and fewer family resources.

In summary, this is a good first effort to study a neglected topic. Yet given the difficulty in studying such behavior, many of the issues Rosenfeld raises require a better research design. Trends cannot be examined with a cross-sectional design, and the three sampling sites may not actually provide the assumed variation in the amount of family and social activity. In any case, more studies like this one, which go beyond description and illustration to explanation and analysis, are clearly desirable.

### **Children's Rights and the Wheel of Life.**

By Elise Boulding. New Brunswick, N.J.: Transaction Books, 1979. 179 pp. \$10.95.

Reviewer: FLORENCE KARLSTROM, Northern Arizona University

Here is a fresh and wide-ranging perspective on children's rights and on human rights in general. As in her earlier works, Boulding's focus is worldwide. Most of the 26 tables present data from United Nations or related sources. The project itself was stimulated by the International Year of the Child.

Boulding finds the rights of children neglected everywhere. In non-industrial societies, children participate more nearly as adults, but often suffer deprivation and hardship. In industrial societies, age segregation is pronounced, to the disadvantage of the overburdened middle group as well as of the young and old. A "doctrine of protection" has grown up around minors and the elderly, commonly well-intended, but unfortunately isolating and depersonalizing them. It is not only in the United States that schools are more oriented to control than to teaching, or that juvenile offenders risk harsh treatment offered in the name of special protection.

Boulding's advocacy extends beyond protection to nurturance, and to the right to participate in the total fabric of community life, including work and social and political decision-making.

Part One marshals data for an assessment of the capabilities of the young—their participation in the labor force (substantial); in voluntary associations (con-

vincing evidence of competence); in the military ("minors have always been put into armies"); as parents (many can cope, but teenage unwed mothers are numerous and seriously overburdened in most countries).

*Part Two examines abuse of children, victimization by families and schools by poverty and lack of health care. The 1970s brought deterioration in life conditions in many parts of the world. Inflexible definitions of the "minor" status hardly distinguish between infant and near-adult, so that reasonable freedoms and potential contributions to society are arbitrarily denied. More child autonomy is not anti-family; on the contrary, healthier families and societies could emerge. Sexual and other physical abuse is widespread, ancient, difficult to remedy. Lacking the right of movement, many children are trapped and endangered.*

A short Part Three elaborates the author's concept of "the wheel of life." The need is for wholeness, throughout the life span—of persons and of societies. But the aged, like the young, lack meaningful roles, and are subject to countless inappropriate exercises of power. Sorting by age intersects with sortings by sex, race, ethnicity, and class, producing complex inequities, debasing the personhood of children and the elderly, and (in industrial societies) leaving adults between 25 and 55 to carry an extraordinarily heavy load.

Extension of a Declaration of Human Rights to those under 21 is proposed. It would promise freedom and autonomy as warranted by a child's maturity, and enunciate rights to food and shelter, to education, work, and equal treatment under the law. Half the world's population is under 25. What a radical reshaping of society is here suggested!

Other recent books on children's rights and on abuse and neglect offer greater detail; none provides so macrosocial a view, nor tries to balance rights, responsibilities and the need to participate in society's mainstream. Boulding's work meshes with others which see alienation and rebellion among youth not as signs of overindulgence, but as reaction to exclusion and to trivialization of their lives. Several themes are shared with *Rethinking Childhood*, edited by Arlene Skolnick (1976): society neglects mismatches between age grading and psychological growth (Keniston); before the mid-eighteenth century, most American children, like their counterparts elsewhere then and now, worked after age seven (Krett); industrial societies "invented" adolescence, then institutionalized it through compulsory education, child labor laws, and juvenile courts (Bakan, Skolnick).

Boulding's style is direct and lively. The "wheel of life" is a provocative concept. The phenomenon of "age-ism" is highlighted in a useful way. The proposal to extend a Declaration of Rights to the young is a sensitizing device, remote though its attainment may be. Articulating a "right to participate" is significant. In a sense, the author is advocating a once-and-for-all, general liberation movement.

The book will be valuable in courses on life cycle and age stratification. (Students should examine the balance between necessary, genuine protection, and needless repression.) This work can provide fresh material, arouse discussion, and relieve cultural myopia in a variety of courses, and will also appeal to a general readership.

**Amateurs: On the Margin Between Work and Leisure.**

By Robert A. Stebbins. Beverly Hills: Sage, 1979. 280 pp. Cloth, \$18.95; paper, \$8.95.

Reviewer: LOUIS KUTCHER, *University of South Florida*

The need to develop a precise definition of "amateur" emerged, for Stebbins, from an earlier study of amateur classical musicians. The first chapter in this work is an expanded version of that attempt, a definition that would, for the sociologist, usefully separate the dedicated amateur in sport, art, science, etc., from both the professional and the nonserious "playful dabbler." That chapter and the final one on "Marginality and Amateurism" represent the sociological meat of the work. The remaining nine chapters are narrative discussions of Stebbins' observations and interviews in three areas, theater, archeology, and baseball, using his conceptual framework.

Stebbins suggests that "modern amateurism" is a parallel development, along with professionalism, in those areas where what was considered play becomes work for some. With the development of professionalism in a given activity, new standards of excellence emerge and confront all participants. The nonprofessional appears mediocre by comparison:

He is thus faced with a critical choice in his career as a participant: restrict identification with the activity to a degree sufficient to remain largely unaffected by such invidious contrasts or identify with it to a degree sufficient to spark an attempt to meet those standards.

The first produces the "dabbler," or part-time, nonserious player. He has neither dedication nor obligation, his participation is play and, "apart from his ordinary, real life."

The second produces participation that moves away from play toward seriousness, dedication, and obligation; ultimately to the "status of modern amateur for some and professional for others." The distinction between these two is not, Stebbins demonstrates, simply skill level and income source. He distinguishes them first as part of a functionally interdependent system of "Professional-Amateur-Public" and then on the basis of an attitudinal set, separating the amateur from professional and each from their publics.

Drawing from previous literature, he provides an acceptable definition of professional too lengthy to elaborate in a short review. Publics are "groups of people with a common interest, which are served by professionals or amateurs or both and which make active demands on them." Publics are functionally related to the other two elements in the system, providing financial support, critical feedback, role support, occasional participation and, as a target audience, they are taken into account by both the professional and amateur in their productions.

The amateurs are functionally related since they often serve the same publics as the professional and are oriented by the same standards set by the professional. Professionals often educate amateurs, who become a unique part of the professionals' public. Because of broader knowledge, the amateur can serve to restrain the professional from superficiality and demand the best for both amateur and public. The amateur ranks provide a locale for participation for the preprofessional and the postprofessional.

In addition to the amateur's position in an interdependent social system he is

characterized by five basic attitudes; confidence, perseverance, continuance commitment, preparedness, and self-conception. Stebbins further describes the "modern amateur" as occupying a unique marginal position in the world of leisure. His preoccupation is misunderstood by those who are exclusively engaged in nonserious "popular leisure," regarding it as work, not leisure. On the other hand, he is excluded from the ranks of professional, whose standards he espouses. While the amateur is a "pure-blooded user of discretionary time," his participation is neither work nor play.

Stebbins' analysis of the three areas mentioned is based on extensive systematic field observation and a total of 83 unstructured interviews. While some may be critical of an "impressionistic" data base and while his ideal-types may suffer in real world application, he provides in-depth insight into a neglected area of leisure life-styles. His liberal use of quotes from field notes and informants adds to a very readable book.

Some self-styled "serious sociologists" may receive this work with a loud "ho-hum" and it is unlikely that the vernacular, quasi-legal use of the term "amateur" by such groups as the NCAA will be affected. However, for those of us who are concerned with the increasing importance of leisure in a technocratic society, Stebbins has made a considerable contribution.

**"Moonies" in America: Cult, Church, and Crusade.**

By David G. Bromley and Anson D. Shupe, Jr. Beverly Hills: Sage Library of Social Research, 1979. 269 pp. Cloth, \$18.00; paper, \$8.95.

**Science, Sin, and Scholarship: The Politics of Reverend Moon and the Unification Church.**

Edited by Irving Louis Horowitz. Cambridge: MIT Press, 1978. 290 pp. \$6.95.

Reviewer: WILLIAM SIMS BAINBRIDGE, *University of Washington*

These two books report the American phase of a fascinating religious movement, the Korea-born Unification Church, familiarly known as the "Moonies." *"Moonies" in America* is a book of real value to the student of contemporary religious movements, prevented from being a great book by a confusion of purposes. Neither fully successful as an empirical report or as a theoretical analysis, it nonetheless contains much useful information and many stimulating ideas. *Science, Sin, and Scholarship* is a journalistic exploitation book of no redeeming sociological value.

The problem with *"Moonies" in America* is that it suffers from compulsive over-analysis. Data are buried in a mountain of interpretations, and often it is difficult to know which statements are plausible conclusions and which are simple statements of fact. The data shine most clearly in sections about the public actions of the Unification Church and the controversies which surrounded them. Bromley and Shupe rely heavily on reports by journalists, at one point tabulating 150 articles from the *New York Times*. The extent of their own primary research is difficult to judge from the evidence in the book, although it seems they conducted interviews



and participated in a few semi-public activities of the church. They also drew on limited ethnographic studies carried out by students.

Unfortunately, Bromley and Shupe found little published information on the origin and development of the Unification Church in Korea. Before we can understand the American phase, we need to know more about the ministry of the founder, Rev. Sun Myung Moon, as he struggled to create a church first under Japanese occupation, then in Communist North Korea, then in South Korea. I am surprised Bromley and Shupe were not able to get original Korean-language sources to strengthen the early chapters of their study. Certainly it is not difficult to find a Korean graduate student in the United States, or a scholarly correspondent in Korea, who could have been persuaded to discover and translate such material.

One positive quality of *"Moonies" in America* is its success in identifying empirical topics worthy of future study. Bromley and Shupe themselves have elsewhere considered more deeply the public reaction to the movement and the controversies which it naturally generated. *"Moonies" in America* also touches on the issue of schism. The authors quote extensively from a recent master's thesis by David Taylor based on ethnographic research in the Oakland, California, branch of the movement, a group which may be in the process of splitting from the main organization. I hope Taylor or someone else continues research on this local church, because it would be extremely valuable to have careful, empirical studies of processes of schism which either lead to the birth of new religious organizations or which are successfully contained within the original organization.

Although Bromley and Shupe are generally scrupulous in giving full credit to their sources, they (like many others writing on the Unification Church) fail completely to note that the original study of the church in America was carried out not only by John Lofland, but also in significant measure by Rodney Stark who was co-author of the first and by far the most important of Lofland's publications on the subject (*American Sociological Review*, 1965).

*"Moonies" in America* might be seen not so much as an ethnography or history of the Unification Church, but as a theoretical work which merely uses the movement as an anchor for its speculations. As theory, the book employs the currently fashionable "resource mobilization" perspective. For Bromley and Shupe, this perspective is an agenda or an emphasis guiding them in choosing which facts and which ideas to consider most closely, rather than a rigorous deductive theory or collection of formal explanatory models. As a corrective to earlier "collective behavior" perspectives, it rightly stresses the conscious strategies and the material or social resources used by the movement in pursuing its aims. The book overflows with italicized and numbered propositions of general applicability to social movements of many kinds. While the empirical evidence seldom proves that these resource-mobilization ideas in fact apply to the Unification Church, the reader will find them provocative and stimulating. Many are stated with sufficient precision that they might be tested more rigorously on data about other social movements. Indeed, students should be alert to the possibility that Bromley and Shupe are in the wholesale "thesis business," offering at low cost highly valuable topics for dissertation research.

In contrast, the collection of journalistic and opinionated essays edited by Horowitz will not profit the reader. Perhaps some people want a couple of Rev. Moon's

speeches, a few vague defenses of the movement by placid and distant commentators, and several vitriolic denunciations by Horowitz and other enemies of the church. But social scientists cannot be included among the audience for *Science, Sin, and Scholarship*. Only the second of the three nouns in the title correctly describes the book. I am sure the Unification Church can provide volumes of church doctrine for those who wish to study it, and serious scholars would have to do their own surveys of the popular literature on the movement anyway, and would find Horowitz an unreliable guide. The only work of social science included in the collection is an interpretive essay by Thomas Robbins, Dick Anthony, Madeline Doucas, and Thomas Curtis, reprinted from *Sociological Analysis*. A single back issue of that fine journal would be worth far more to the student or social scientist than this embarrassing book.

**Yearning for Yesterday: A Sociology of Nostalgia.**

By Fred Davis. New York: Free Press, 1979. 146 pp. \$10.00.

Reviewer: WILLIAM S. FOX, Skidmore College

A systematic sociology of nostalgia of the sort that Fred Davis attempts in *Yearning for Yesterday* is certainly a worthwhile and long overdue intellectual project. Unfortunately, though, Davis' work contributes rather little to our understanding of nostalgia and offers but few insights that are likely to inspire further investigators.

The disappointment of this short book stems in part from the intellectual strategy that Davis adopts. Davis deliberately chooses to avoid examination of specific nostalgias such as Beatlemania or the Fifties revival with the dubious rationale that such analyses encourage the unsociological assumption that nostalgias stem from qualities inherent in their objects and that sociological explanations of particular nostalgias are virtually impossible. This questionable strategy of avoiding detailed case analyses results in a work that so tends to the abstract and general that it often seems more an intellectual exercise than an attempt to understand an empirical phenomenon of some importance.

Davis does begin most appropriately with a brief etymological analysis of the term "nostalgia." It is a high point of the book. Davis traces "nostalgia" from its early medical reference to the disease of homesickness that plagued Swiss mercenaries, to its current meaning, suggested by Davis, of positive feelings toward a personally experienced past. Davis' emphasis on the personal nature of nostalgic experiences unfortunately and unnecessarily limits his analysis since much of what we reasonably call "nostalgia" seems to involve reconstructions of the pasts of others. Consider, for example, the Fifties craze among students born in the 60s.

Following an exploration of nostalgia's relationship with the past, Davis suggests three "ascending orders" of nostalgia: simple nostalgia, or unexamined experiences of the phenomenon; reflexive nostalgia, or critical consideration of the historical accuracy of nostalgic feelings; and interpreted nostalgia, or critical treatment of the nostalgic experience itself. Little theoretical rigor or empirical evidence informs this typology and Davis himself makes little real use of it in his subsequent analyses.

Although hardly novel, Davis' account of the relationship between nostalgia

and personal identity is perhaps the most sensible chapter of the book. Davis argues that nostalgia results in part from the problematics of identity construction and maintenance and thus serves to foster continuity of personal identity. However plausible this account and despite reference to such social theorists as Goffman and Strauss, this social-psychological treatment of nostalgia also lacks theoretical rigor and empirical support. Davis does make good use of his explanation of the nostalgia-identity relationship to analyze the interconnections between nostalgia and the life cycle. He observes, for example, that the association of nostalgic experiences with adolescence and old age may reflect the fact that these are the two life cycle stages with particularly acute identity problems.

Moving beyond such social-psychological concerns, Davis examines the dialectical relationship between nostalgia and art. Suggesting that art both shapes and draws from nostalgia, Davis quite properly argues that nostalgia is a distinctive form of consciousness. His reliance on Schutz's notion of multiple realities and his incorporation of specific examples gives this discussion a conceptual framework and empirical grounding all too lacking elsewhere in the book.

Davis approaches the larger social consequences of nostalgia by treating nostalgia as a form of collective search for identity. His analysis is generally well-served by an intensive examination of the nostalgia craze of the last decade—a craze that Davis believes was rooted in the social disruptions and dislocations of the 60s and 70s. Davis remarks in this connection on the conservative tendencies of nostalgia as well as on its generally radical role in defining and shaping generations.

The book concludes with an analysis of "contemporary nostalgia," with special attention to the connections of nostalgia to economic institutions and the mass media. Whatever historical uniqueness characterizes our times may well be reflected in the "nostalgia industries" that we create and that help create us. Davis suggests potentially useful distinctions between public and private nostalgia, and considers some interesting "fanciful possibilities" (e.g., the deliberate planning of future nostalgic revivals in the marketing of new products). A more extensive analysis of the relationship of nostalgia to the many other contemporary manifestations of the reconstructed past is clearly called for. One wonders, for example, where nostalgia fits in among the TV pseudo-documentaries, fabricated folklore, and other individual and collective recreations of a past that never was.

Although a rigorous sociology of nostalgia remains to be written, *Yearning for Yesterday* serves as a reminder that we make selective use of the past as we confront the social and psychological realities of the present. Therein lies its principal value. Some readers may also appreciate Davis' discursive writing style, reminiscent of nineteenth-century essayists.

**The Enduring Asylum: Cycles of Institutional Reform at Worcester State Hospital.** Edited by Joseph P. Morrissey, Howard H. Goldman, and Lorraine V. Kleman. New York: Grune & Stratton, 1981. 356 pp. \$31.50.

Reviewer: ROBERT J. GREGORY, Marshall University

Community mental health centers expanded rapidly in the activist 1960s, then a

strong movement promoting deinstitutionalization took over in the 1970s. The pendulum is beginning to swing back now, a move perhaps correlated with economic troubles. State mental hospitals are in no way the vanguard of a new direction, but Morrissey, Goldman, and Kleman do regard them as an essential component of a comprehensive human service system.

*The Enduring Asylum* traces the history of the Worcester State Hospital in Massachusetts from its origin to the present, and includes some predictions about the future. The changing social milieu, internal and external politics, and the many forms of patient care are discussed intelligently and with feeling. The story deals in turn with social institutions, organizational dynamics, and social policy.

The first part of the book offers a historical overview. The idealism of the early, broad-based movement to help those with mental problems was followed by a decline into narrow, professional management of an externally controlled organization. This history mirrors the impact of industrialization and an increasingly class-structured society. The somewhat sad, but occasionally improving, state mental hospital as social institution is well-described in Chapters 2 and 3. The story is completed by David Myerson's insightful examination and analysis of his final term as medical superintendent. This period, 1969-77, is especially interesting, for it saw the transition from a contained, closed system represented by one hospital to a spreading network of community-based programs. The network appears to be both competitive and cooperative, as people and programs seek to provide improved services to all in need.

The middle section of the volume includes six chapters which elaborate on the opening and blossoming of the hospital-community system. The boundaries were crossed programmatically in the courts, the schools, an adolescent treatment center, and in general medicine, as well as through policy, staffing, financial arrangements, and similar ways. The new networks helped to bring about significant alterations in the character of the hospital, its internal and external dynamics, and the political and social policies necessary to organize the services.

In their concluding chapters, the authors argue that even after these changes, the state mental hospital has a role to play. They claim it persists because nothing else has been able to provide the last resort services needed. Mental health centers and the conglomeration of social, health, and welfare agencies available in most communities have been simply unable to offer the resources needed for the particular population served by the hospital. The multi-tiered private-public system has aggravated the problem, for more resources are allocated to those who can afford private health care. In the public sector, the state mental hospital serves as the agency for those "left over." The chapters are insightful, and eminently fair. The authors do not appear to have been severe critics of the system, but they have examined their own weaknesses as well as those problems inherent in the mental hospital.

The authors have produced a well-documented, readable, and meaningful account. They do not discuss the iatrogenic nature of such hospitals, nor do they deal with anti-psychiatric and anti-medical models, but they do illustrate why moves to develop community mental health centers and to deinstitutionalize have not forced state hospitals to close. Indeed, the new philosophy of government and the economy may further strengthen the state mental hospital.

**Catharsis in Healing, Ritual, and Drama.**

By T. J. Scheff. Berkeley: University of California Press, 1980. 246 pp. Cloth, \$12.95; paper, \$3.95.

Reviewer: GRAEME NEWMAN, State University of New York at Albany

The attractive cover of this book suggests a fascinating content. It reproduces a bizarre print by one of the Bruegels, replete with a head on a dinner plate, knives piercing hands and tongues, half-animal half-human bodies twisted into impossible postures, all of these bundled together in chaos, but contained under the calm gaze of a man of the church, a Moses-like figure with a staff. Perhaps the author chose this print to reflect the pain that he thinks underlies catharsis. But he does not comment on it at all, in fact gives only passing attention to the role of religion in catharsis and healing, except to agree with some commentators that ritual today is somehow empty. There is nothing of the mystery or emotional intensity of Bruegel's print to be found in the book, so the book is a monumental failure by the author's own standards: it is "overdistanced."

Distancing refers to the "extent to which the individual is an observer of his or her emotions: optimal distancing is when one can achieve double vision: being simultaneously a participant and an observer of one's emotions." But Scheff achieves a different double vision in this book: the split between the kinds of knowledge that emerge from a "soft" or "clinical" setting—impressions, intuitions, feelings; and the kinds of knowledge that issue from science—objective knowledge, hard, cold, facts. Scheff tries valiantly, but ends up very much the social scientist wedded to laboratory experiments and "reviews of the literature." It is in the latter role that he is strongest, reviewing past laboratory studies on catharsis, and reinterpreting them. But on the other side, there is not much there except a lot of talk about crying, and some superficial treatment of catharsis in drama. In trying to apply his notion of optimal distancing he has failed on both sides, but especially the "soft" side.

This need not have been the case, and does not mean, I think, that the two kinds of knowledge are irredeemably split. It means, rather, that Scheff has either not read or not understood the enormous psychoanalytical literature that relates to catharsis and to "distancing" which Scheff did not of course discover. (Reich, for one, talked of "character armour," which was pretty much the same thing, although Reich was clear that he wanted no distancing at all.) What are defense mechanisms for if not to distance one from emotions? At times, Scheff comes close to penetrating these problems, especially when he talks about repression. But he does not deal with this process except in a very general way. What is it that makes repression so necessary a part of everyone's psychological make-up? Scheff is content to say that "emotions are repressed when they seem so overwhelming as to be unbearable." But theorists such as Norman Brown and Ernest Becker have not been content with this simple observation (as against explanation). They wanted to know *precisely* what conjured up these unbearable emotions so that they had to be repressed. Becker saw it as a denial of death, closely related to the need for ritual, religion, the inability to live life in its full truth, the loss of faith. This overwhelming, terrible, *cultural* problem is what palls us, and no amount of catharsis is going to solve it. In fact, one might say that the sublimatory processes that transform

these fears into life forms are to a very large extent performing a cathartic function. Again, the author does not mention sublimation at all. Presumably, he takes a dim view of it, since sublimation is an indirect or converted "release of tension" (by one definition) and usually involves a lot of talking, or at least intellectual work. The fact that a number of psychoanalytically oriented theorists have argued that our civilization is based on this process surely requires Scheff to address this question, especially if he wishes to reinstate catharsis theory as a "legitimate" theory.

It might be said in his defense that he is concerned with catharsis in treatment, not as a cultural phenomenon, and he does raise an interesting point in dealing with the paradox of repression. "When the individual's attention is exactly divided between past distress and present safety (i.e., an optimum distance from the repressed emotion), repression is lifted, and catharsis can occur." But one wonders whether this balance can often be found, certainly on a lasting basis. Nor is it clear whether this balance would be satisfied by "crying behavior" and other kinds of tension release, or by the more intellectual or reflective "release," which Scheff claims to favor less, yet which seems to me much more consistent with the "optimal distancing" notion. The difficulty is that he has not specified exactly how much crying or intellectual effort is needed for an optimal distance. His bias towards the "intellectual" is illustrated by the very slight attention he gives to faith healing, a phenomenon that is an important part of many fundamentalist (and even less extreme) religions. As a treatment technique, Scheff has pushed forward the modern secular view of therapy, ignoring its deep roots in the past. The horrible distortions of the body in Bruegel's print conveyed the sixteenth-century view of mental illness: the problem was in the body, and had to be torn out of it. More importantly, it was to be torn out, *not repressed*, because it was the devil inside the body, so one could not leave it in: it had to be exorcized. We can see the same religious model with Scheff. The demon "emotion" must not be dammed up. Crying, screaming (but not too much of it) must be used to get rid of it.

The controlled release can occur through drama. Here again Scheff does not attend to the important issues because, I suspect, he is too eager to find support "in the literature" for his own theory of catharsis. He fails to attend to the questions of whether, and how much, catharsis is different in a small well-defined treatment situation, compared to the catharsis of drama in mass media and mass spectator sports. In this regard, there is a further glaring omission: he gives not the slightest note to the mass nature of both catharsis and ritual. Does Scheff not know that Freud had much to say about these questions in his *Group Psychology and Analysis of the Ego*, to say nothing of Reich's work on mass psychology? Did he not observe behavior at protest rallies in the 60s—were these not massive, ritualistic outpourings of emotion? And, similarly, the Iranian protests of the 70s? And the massive crowds that responded to Pope John Paul's visits and to the return of the American hostages from Iran?

It is going to take much more than a simple idea of "optimal distancing" to solve the paradox of repression. Indeed, it is virtually part of the definition of the word "paradox" that it cannot be resolved by moderation, and it is moderation that Scheff, in the long run, advocates. Individuals and whole cultures continue to try to come to grips with their past. There can be no doubt that the arts, drama, even science, are ways that civilizations try to solve this puzzle. I seriously doubt whether crying and laughing more is going to help very much. In the long run, in

fact, it is usually violence that temporarily resolves the paradox, and through which the classic tragedies of drama emerge. This has been well illustrated by Girard, in *Violence and the Sacred*, and the point was driven home recently in *Apocalypse Now*: "You have a right to kill me, but not to judge me," said Marlon Brando.

### **The Existential Sociology of Jean-Paul Sartre.**

By Gila Hayim. Amherst: University of Massachusetts Press, 1980. 148 pp. \$13.50.

Reviewer: MARK J. LA FOUNTAIN, West Georgia College.

Sophisticated works on the sociological relevance of Jean-Paul Sartre are just beginning to appear. Now joining Craib's *Existentialism and Sociology* (1976) is Gila Hayim's important little book. It offers a wealth of insights for critical and orthodox sociology, social psychology, and existential sociology.

Hayim's concern is with the intelligibility of human praxis and the recovery of human freedom. Effectively illuminating Sartre's dialectical humanism and regressive-progressive method, she renders his philosophico-social project concerning the human situation intelligible for sociologists. This is accomplished by demonstrating a continuity between *Being and Nothingness* and the *Critique of Dialectical Reason*.

From the first of these works, Hayim competently extracts Sartre's intricate notions of the "en-soi" and "pour-soi," consciousness, nothingness, freedom, anguish, bad faith, possibility, and transcendence. She also traces the circular path of being-in-the-world as an individual and being-for-others. To make these ideas accessible, she weaves the thought of Durkheim, Weber, and Mead in and out of that of Sartre, as well as that of Hegel, making him, too, available for sociological contemplation. This first section on ontology pointing toward sociality concludes with Sartre's Nietzschean notion of the spirit of seriousness, which is carefully related to the modalities of positivism and techno-bureaucratic society.

From an analytic summary of Sartre's phenomenology of social being and its nexus with the spirit and ethics of seriousness, Hayim presents the case for a Sartrean-inspired critical consciousness bound to moral choice and responsibility. This part of the book, underpinned by Sartre's existential psychology, is particularly strong. The result is a profound enrichment of the social action and interactionist perspectives by an existential theory of action and choice, and an implicit call for a critical-existential sociology.

A theory of action and ethics, however, is not complete without being embedded in group life and in the totalizing processes of history as taken up in individual praxis. To comprehend the fusion of ontology and sociality, Hayim here shifts attention to the *Critique*, and introduces another round of important Sartrean notions, e.g., worked matter, necessity, inertia, scarcity, totalization, and the practico-inert field. She presents a synthesis of Marxism, sociology, and existentialism, indicating how existential sociology might rethink structural ideas so as to maintain the sovereignty of individual freedom and praxis. She also provides an existentialist account of group formation and institutionalization, building on Sartre's analysis of such types as serial groups, groups-in-fusion, pledge groups, and organized groups, such as bureaucracy and the State. Throughout, the

troublesome problems of maintaining freedom from inertia, assuring the intelligibility of individual praxis within the totalizing praxis of the group, authority, terror and violence, and moral decadence are highlighted. The transition from groups based on interior, humane grounds to those grounded in exterior, material elements is a crucial issue for sociology. Like Sartre, Hayim indicates reasons for pessimism, but she also points out Sartre's optimism, an aspect of his thought sorely misunderstood.

The book is well-written and consistent with its objectives. Its chapters are concise and well-organized, the expression is clear, and examples are instructive (although sparse given the analytic tone of the work). Though competent, chapters 4 through 7 are a torturous and exhausting trek through the *Critique*. Hayim extracts a variety of sociologically relevant insights and presents them coherently, but with a tiring style. The reward for those who make the journey, though, is the triumphant—or painful, as the case may be—awareness of whether one has created freedom, morality, and transcendence in one's own life space.

Conspicuously absent is any effort whatsoever to critique Sartre's ideas from either a sociological or an existentialist (after Merleau-Ponty) point of view. Also absent is any reference to Sartre as an artist. Not once is a novel or play mentioned, and in very few places does one glimpse the pulsing, breathing life Sartre so poignantly displayed. This, after all, is the very feature of existence a critical, Sartrean sociology would want to recover and preserve. Nevertheless, these criticisms do not detract from the quality of Hayim's work. This is a very good book.

### **Sociologists in Search of Their Intellectual Domain.**

By Gunnar Boalt and Ulla Bergryd. *Atlantic Highlands, N.J.: Humanities Press, 1979.* 162 pp. \$33.75.

Reviewer: ROBERT MC AULAY, *Vassar College*

In an essay toward the end of this volume, Robert Erikson and Gunnar Boalt muse on the less-than-hoped-for response to one of their previous (theoretical) publications: "We saw in our mind's eye a procession of alert young researchers quickly following up our idea and turning it into an empty shell of self-evidence, like Pooh, when the five hundred and eighty-seventh Heffalump was licking its jaws and saying to itself, 'Very good honey this. I don't know when I've tasted better.' But the book was published anyway. (And no Heffalump turned up, sad to say.)"

Given the nature of this new collection of papers, Boalt and his co-authors may, this time, find themselves waiting indefinitely for an appreciative reviewer Heffalump to come along. At the very least, they will not find their optimal Heffalump here.

To begin with, this book might be better titled "*Swedish Sociologists in Search of Their Intellectual Domain.*" Most of the empirical essays which make up this collection are, in fact, centered on value clusters and associated social arrangements which have characterized recent Swedish sociology up to about 1975. Of more than passing interest for these authors is the challenge to the positivist paradigm dominant in Sweden, presented by radical students in the late 1960s and early 70s. Not surprisingly, the various studies reported here suggest an association between



political radicalism, anti-positivism, lack of research funding, and a theoretical or teaching orientation. These findings are used by Boalt and his colleagues to support Joseph Ben-David's observation that groups of scientists may differently perceive paradigm breakdown or the presence of paradigm "crisis."

The conceptual tool which organizes many of these studies is Boalt's own and favorite "summation" or "compensation" theory, which generates variations on the idea that "research reports tend to attain about the same sum of research values . . . and if you give up research values in one area, you have to take them from another." Boalt admits that this is rather basic, but maintains nonetheless that "it is very important to be able to predict these clusters of research values associated with one another." These studies indicate not only a separation of teaching and research interests, but also that research-oriented sociologists tend to delay marriage and to be less family-oriented than non-researchers or less successful researchers, who apparently "compensate" by stressing familial values. In addition, several interesting papers bear on the declining status of university professors, both in Sweden and cross-nationally. But, by and large, the research here produces rather limited empirical generalizations which at times, frankly, border on the commonplace.

The redeeming feature of this volume, in my view, rests on the authors' continual effort to assess reflexively their own social-scientific project, and on Boalt's apparent ability to recruit world-class Heffalumps like Joseph Ben-David and the late Alvin Gouldner to reflect explicitly on the significance of his work. Discussion of Gouldner's Introduction to Boalt's *Sociology of Research* is perhaps the high point of an otherwise largely empirically focused volume. Gouldner's comments, reproduced at length, attempt to pinpoint some of the assumptions underpinning Boalt's sociology and the formalistic "high science" model which he embraces. Ben-David's Foreword to the present volume extends this line of analysis by attempting to explain "why the Swedish tradition has been so uniformly empiristic and formalistic." In his view, that formalism rests in part on the effort by Swedish university departments to preserve their own strict criteria of degree-granting and promotion—a condition which Ben-David argues has "made Scandinavian sociology internationally recognized and respectable, but at the cost of cutting it off from the most important sources of theoretical innovation."

What both Ben-David and Gouldner have done, however, is to sidestep an evaluation of the substance of Boalt's work. In other words, using Boalt as a documentary example of Swedish formalism—i.e., treating his work as a topic rather than as a resource—avoids explicit consideration of whether that work can help us in our own endeavors. Despite my appreciation for Boalt's reflexive sensibilities, he and his colleagues are, I feel, imprisoned within an approach and set of concerns which inhibits their possible reach, so the significance of these studies seems limited and their effect less than inspiring. No hesitancy on my part, no fear of parochialism in rejecting Boalt's Swedish-focused sociology or of discounting work that some sociologists of world stature seem to find interesting, can overcome the fact that that is my conclusion.

**Research in Service to Society: The First Fifty Years of the Institute for Research in Social Science at the University of North Carolina.**

By Guy Benton Johnson and Guion Griffis Johnson. Chapel Hill: University of North Carolina Press, 1980. 442 pp. \$20.00.

Reviewer: RICHARD ROBBINS, *University of Massachusetts at Boston*

"On June 30, 1924, the Institute for Research in Social Science was organized at the University of North Carolina. It was the first institute of its kind in the nation, and it was destined for a distinguished career. It stands today as an enduring monument to its founder Howard W. Odum[,] his commitment to 'cooperative research in the social sciences' and his firm belief in the role of 'research in service to society.'" Thus begins the detailed record of an Institute well known to three generations of sociologists and other social scientists who have drawn on its productive work in regional studies, race relations, southern socioeconomic problems, public policy, political behavior, and southern folk culture. The authors, husband and wife, were present at the founding as young research assistants (another first of its kind). Over the years they made signal contributions to the work of the Institute, he as a sociologist specializing in race relations and what was then called Negro folk culture, she as a journalist and social historian with a particular interest in the history of the Sea Islands. They were thus in the best inside position to write the definitive account of the impact of the Institute over the past half century.

And so they have. As an insiders' house history *Research in Service to Society* works exceedingly well, the great clusters of research studies, facts, and figures enlivened by illuminating stories about Odum and the many social scientists who were associated with him or were his students. The book divides into procedural and substantive halves. In the first part the Johnsons describe Odum's struggle to establish the Institute, the years of growth, the changes in administrative structure, the changing funding base with philanthropic support gradually giving way to Federal-state and then largely state support. Then the Johnsons summarize in lucid fashion the impressive flow of research projects; there are chapters on folk, race, and culture studies, regionalism, economic issues ("The South at the Bottom of the Ladder"), social policy, behavioral science research, urban and health questions.

In a brief concluding overview, the Johnsons note that although the Institute is now solidly institutionalized, the battles with reactionary textile interests, racial bigots, and fundamentalist religious groups long gone, the Institute continues to be "a dynamic and vital force in the intellectual life of the region and the nation." Incidentally, in connection with these battles, it is never too late to pay tribute to Odum, to Frank Graham, Arthur Raper, Guy Johnson, Rupert Vance, and other social scientists who, in defense of the integrity of research and of humane values, did not hesitate to defy the norms of racial segregation by bringing black scholars to lecture at Chapel Hill, and did not permit themselves to be deterred from analysis of (and indignation about) such matters as low wage rates, lynching, and the new peonage of the sharecrop system in the South. To be sure, compromises had to be made, and Odum had to waste valuable research time placating hostile groups certain that sociology was socialism and that a Harry Elmer Barnes review in *Social Forces* was a deadly assault in behalf of godlessness. Yet in 1941 where but in Chapel

Hill would a southern university press publish a trenchant study of persistent southern feudalism by two scholars, one white, one black: Raper's and Ira De A. Reid's *Sharecroppers All*?

As a house history, then, *Research in Service to Society* will prove valuable and informative not only to social scientists who have been associated with the Institute, but to those with an interest in the sociology of social science, the way in which lines of research reflect the character of the social order at different times. However, for what one might call the sociological outsiders, for those less concerned with the internal structure of the Institute and the myriad of studies, more concerned with the interplay between an important social science center and a changing—and non-changing—South, there is bound to be some disappointment. The concluding review, where such a historical evaluation might have been made, is but eight pages long and largely a summary at that. I suppose that the absence of a tough-minded evaluation of the Institute's role in southern social structure derives from a natural reticence to judge persons and policies on the part of authors who have themselves been an integral part of the Institute's life.

Understandable. Nonetheless, the failure to weigh and to evaluate the Institute in a larger setting leaves many questions unanswered, questions which would be of great interest to a wider audience of social scientists. For example, if there is a hero in this book, it is Odum, founder of the Institute, of *Social Forces*, of a prestigious sociology department, and the force behind the burgeoning interest in southern regionalism in the 30s and 40s. Why, then, has his approach to regionalism—correlation of socioeconomic indicators in terms of North-South differentials—as well as his general sociological thought in all those many, many volumes turned out to be so thin and dated?

Or, consider race. The Johnsons worked in the midst of a tightly segregated racial system which had to be mirrored in a dual system of higher education and even a dual effort in social science research. (The only major rival for research honors on the black side was Charles S. Johnson's Social Science Department at Fisk University.) Odum and the others knew the system was unjust, and in personal terms they resisted it courageously. But how did racial segregation directly affect the agenda of the Institute? What could, and could not, be done? How was Odum able to develop working associations of black and white leaders and scholars? How was support from southern white liberals achieved? The Institute's diplomatic strategy, opening the way to so many important studies on so sensitive an issue, would have made a story as interesting as the analyses we are now reading of the shaping of Myrdal's *American Dilemma*.

Finally, to choose one other illustration, how would the Johnsons assess the transition in the Institute's orientation from the social problems approach of its first quarter century to its behavioral science ethos in the 50s? Was the turn to "theoretical models" a response to the call for greater scientific precision in research, or did the Institute, like so many centers in the nation, simply reflect the less adventurous, more apolitical climate of the Eisenhower era? Whatever the explanation, it is plain that, once past the Gordon Blackwell period of emphasis on highly rigorous and abstract model-building, the Institute did not regain its commanding position in social science research. Perhaps that was inevitable, the founders giving way to the specialists, but on this interesting question the Johnsons are content largely to summarize the studies.

Let the shepherds return to their sheep, the French say, reminding us to concentrate on the book that is and not speculations about the book that could be or should be. In these terms, as a history of the rise and continuing activity of a social science institute, *Research in Service to Society* takes its place with distinction on the long shelf of volumes from Chapel Hill that enrich our understanding of the South.

# Take Note

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**Arno, Robert F. (ed.).** *PHILANTHROPY AND CULTURAL IMPERIALISM: THE FOUNDATIONS AT HOME AND ABROAD.* Boston: Hall, 1980. 473 pp. \$24.95. Cultural imperialism is the organizing theme for this collection of papers on the origins, workings, and consequences of modern general-purpose foundations. Original research examines the activities of foundations (especially Carnegie, Rockefeller, and Ford) in the production of culture and the formation of public policy, especially in the fields of education and social science research.

**Aronoff, Myron (ed.).** *IDEOLOGY AND INTEREST: THE DIALECTICS OF POLITICS. POLITICAL ANTHROPOLOGY YEARBOOK 1.* New Brunswick: Transaction Books, 1980. 217 pp. \$29.95. Seven essays looking at the relationship between culture and politics in a variety of settings, ranging from rural Bangladesh to revivalist movements in the Caribbean. A welcome addition to the literature on political anthropology which should stimulate sociologists to participate in this increasingly multidisciplinary field.

**Bankowski, Zenon, and Geoff Mungham (eds.).** *ESSAYS IN LAW AND SOCIETY.* London: Routledge & Kegan Paul, 1980. 207 pp. \$18.00. Eleven essays with no particular theme in common, ranging over such issues as the legal profession, plea bargaining, juvenile courts, political trials, police, punishment and social organization, battered women, gambling, and industrial relations law. A sampling of current work on the sociology of law in Britain.

**Berremen, Gerald D.** *CASTE AND OTHER INEQUITIES: ESSAYS ON INEQUALITY.* Meerut, India: Folklore Institute, 1979. 335 pp. \$11.50. The way in which people think about rank and caste is the central theme of these essays by a distinguished anthropologist. Most focus on India, though several are developed through comparison with one or more other societies.

**Bickman, Leonard (ed.).** *APPLIED SOCIAL PSYCHOLOGY ANNUAL, VOLUME 1.* Beverly Hills: Sage, 1980. 308 pp. Cloth, \$20.00; paper, \$9.95. Sponsored by the Society for the Psychological Study of Social Issues, this annual presents a perspective that has been virtually lost to social psychology for over three decades. It focuses on the natural setting and shows how social psychology can deal with real-world problems. The contributors were guided by the principle that research must accomplish some social good, and its corollary, that only scientific research provides reliable guidelines for effective social action.

**Black, Donald (ed.).** *THE MANNERS AND CUSTOMS OF THE POLICE.* New York: Academic Press, 1981. 274 pp. \$9.50. Articles covering a decade of work. While some have appeared elsewhere, others present findings from current research. Topics include the mobilization of law, production of crime rates, social organization of arrest,

dispute settlement, and self-help in modern society. A mix of state-of-the-art research on police and acknowledged classics.

**Burstyn, Joan N.** VICTORIAN EDUCATION AND THE IDEAL OF WOMANHOOD. *London: Croom Helm, 1980. 185 pp. \$24.00.* Examines the Victorian ideal of woman as wife and mother and the challenges to it, as women began to pursue higher education. The arguments of those who opposed women's efforts to obtain higher education in the late nineteenth century are reviewed, and the author concludes that higher education became more accepted, not because opponents were convinced that their arguments were wrong, but because many families found the ideal unattainable and their daughters wanted work commensurate with their family's status.

**Burtchaell, James Tundstead (ed.).** ABORTION PARLEY. *Kansas City: Andrews & McMeel, 1980. 352 pp. \$20.00.* Twelve essays prepared for the National Conference on Abortion, convened in October 1979 at Notre Dame, represent a wide spectrum of knowledge and moral persuasion concerning abortion and its ramifications. Implications of abortion for the individual and family are treated, as well as broader concerns touching on political and social issues, ethical values, and human rights.

**Carson, Clayborne.** IN STRUGGLE: SNCC AND THE BLACK AWAKENING OF THE 1960s. *Cambridge: Harvard University Press, 1981. 359 pp. \$22.00.* This detailed and well-documented descriptive history of SNCC retraces the well-known history of the Southern Voter Registration campaigns and sheds new light on the causes of SNCC radicalization and decline. After the Civil Rights victories of 1964 and 1965, SNCC developed only tenuous links to urban ghetto blacks. The black power rhetoric of some of its leaders, amplified and distorted by the news media, increased even as its organizational and grassroots strength was declining, creating a misleading image of its threat to the social order. The result was internal factionalism, withdrawal of liberal financial support, police harassment and prosecution, and eventual death.

**Cohen, Stanley.** FOLK DEVILS AND MORAL PANICS: THE CREATION OF THE MODS AND ROCKERS. *New York: St. Martin's Press, 1980. 235 pp. \$22.50.* This new edition of a now classic study originally published in Britain in 1972 includes a 30-page evaluation of the wider implications of the author's argument. On the basis of a case study of two forms of teenage collective behavior in the 1960s, the author draws some general conclusions about societal reaction and the production of deviance.

**Donzelot, Jacques.** THE POLICING OF FAMILIES. *New York: Pantheon, 1979. 242 pp. \$4.95.* A student of Foucault analyzes the forces which have served to control the family during the last two centuries. Focusing on France, he shows in detail how such institutions as the church and the school functioned to shape and preserve family structures, as did political and cultural constraints. A model for emulation by sociologists of the family in the U.S.

**Fisher, Glen.** AMERICAN COMMUNICATION IN A GLOBAL SOCIETY. *Norwood, N.J.: Ablex, 1979. 165 pp. \$17.50.* Combines empirical and anecdotal evidence from pilot studies and the author's experiences to analyze the growing impact of Western culture on the developmental opportunities of Third World countries. Problems created by the one-way flow of information, the dissemination of popular culture,

and Western domination of the knowledge industry are analyzed. Fisher's work contributes to the rapidly expanding interdisciplinary study of the political economy of information, and will be of interest to sociologists as well as journalists and political scientists.

**Fletcher, Ronald.** *SOCIOLOGY: ITS NATURE, SCOPE AND ELEMENTS, VOLUME I.* New York: Scribners, 1981. 255 pp. \$20.00. As an account of what sociology is all about, this British secondary school text rivals most American introductory texts and surpasses many. It is a measure of its seriousness that it could be read with profit by professors, and a measure of our problems that most American college freshmen could not read it at all. It opens with a manifesto of sorts, which brooks no fashionable equivocation about the discipline, examines the major classical theories clearly and concisely, and then turns to "sociology now." Obviously, to cover all this, the book must use words rather than the pictures so beloved by American publishers (and students?). Volume I treats theory; Volume II will take up the study of social systems.

**Foster, Lawrence.** *RELIGION AND SEXUALITY: THREE AMERICAN COMMUNAL EXPERIMENTS OF THE NINETEENTH CENTURY.* New York: Oxford University Press, 1981. 363 pp. \$19.95. An innovative, comparative study of Shaker, Oneida, and Mormon communal experiments traces their origins to what the author terms the antebellum crisis in marriage and family life. Instead of emphasizing the ultimate failure of these communities, he analyzes the factors that let them maintain a successful alternative system for more than a quarter of a century.

**Gillespie, David F., and Dennis S. Mileti.** *TECHNOSTRUCTURES AND INTERORGANIZATIONAL RELATIONS.* Lexington: Lexington Books, 1979. 151 pp. \$16.00. Proposes an alternative to the goal model of interorganizational relations. The "technostructural" model suggests that interorganizational relations may be explained by the technology and composition of organizations. Empirical results suggest that interorganizational relations are likely to appear for organizations with similar technologies, but dissimilar technostructures. Reviews previous work on interorganizational relations, comments on problems with the goal concept, and discusses problems in the conceptualization and measurement of technology.

**Grenberg, Norman.** *THE MAN WITH A STEEL GUITAR: A PORTRAIT OF AMBITION, DESPERATION, AND CRIME.* Hanover, N.H.: University Press of New England, 1981. 165 pp. \$10.95. An account, in the form of a biography, of how the failed ambitions of a country musician led him to engage in armed robbery.

**Halem, Lynne Carol.** *DIVORCE REFORM: CHANGING LEGAL AND SOCIAL PERSPECTIVES.* New York: Free Press, 1980. 340 pp. \$17.95. An examination of divorce law in America from colonial times to the present, linking changes in laws of divorce, custody, and alimony to changing perspectives on marriage, resulting from intellectual-social currents of the times.

**Held, David.** *INTRODUCTION TO CRITICAL THEORY, HORKHEIMER TO HABERMAS.* Berkeley: University of California Press, 1980. 511 pp. Cloth, \$32.50; paper, \$12.75. A lucid and perceptive study of the persons and ideas identified with the Institute of Social Research established at Frankfurt in 1923. These thinkers—chiefly Horkheimer, Adorno, Marcuse and Habermas—rejected the orthodoxies of both

Marxism and Western social science, and were a dominant influence in reshaping Marxism. Held reviews the contributions of this group: the analysis of conflict and capitalism, studies in the sociology of knowledge (criticisms of social science and its methodology—above all, positivism), the study of the arts and the culture industry, of psychoanalysis and their attack on the problem of self-actualization under the constraints imposed by contemporary society. A useful bibliography is appended.

**Hobbs, Albert H.** *MAN IS MORAL CHOICE*. New Rochelle: Arlington House, 1979. 446 pp. \$9.95. Taking it as an article of faith that one can choose between good and evil by act of will, Hobbs marshalls supporting evidence, and aggressively questions the degree to which behavior is predictable, as maintained by the behaviorists, in favor of free, hence unpredictable, choice.

**Karatnycky, Adrian, Alexander Motyl, and Adolph Stuomthal.** *WORKERS' RIGHTS, EAST AND WEST*. New Brunswick: League for Industrial Democracy and Transaction Books, 1980. 150 pp. \$4.95. An examination of workers' rights in eleven countries that signed the Helsinki Accords: seven Western countries, the USSR, Poland, Romania, and Czechoslovakia. The findings of violations of workers' rights in Eastern Europe will come as no surprise, but the details of the situation and of the attempts by workers to bring about change, provide us with much that is new. Valuable reading for those who want a more systematic background for understanding workers' movements in industrial societies in general, and in socialist societies in particular.

**Krotki, Karol (ed.).** *DEVELOPMENTS IN DUAL SYSTEM ESTIMATION OF POPULATION SIZE AND GROWTH*. Edmonton: University of Alberta Press, 1978. 260 pp. \$17.50. An examination of the techniques of dual systems of data collection, and of the practical problems involved in undertaking them. The method is illustrated using real cases, and its utility in underdeveloped countries is evaluated.

**Lunn, Kenneth (ed.).** *HOSTS, IMMIGRANTS AND MINORITIES: HISTORICAL RESPONSES TO NEWCOMERS IN BRITISH SOCIETY 1870-1914*. New York: St. Martin's Press, 1980. 377 pp. \$25.00. Papers originally presented at the 1978 conference of the Society for the Study of Labour History cover popular images of nonwhite races; the relations among ethnicity, class, and politics; working-class attitudes towards Jews; the place of Gypsies in British society; reactions to East European immigrants; and the role of trade unions in ethnic relations.

**McConnell-Ginet, Sally, Ruth Borker, and Nelly Furman (eds.).** *WOMEN AND LANGUAGE IN LITERATURE AND SOCIETY*. New York: Praeger, 1980. 352 pp. Linking the social study of language to the study of women in literature, this interdisciplinary volume examines a wide range of topics: linguistics and the feminist challenge, the image of women, women as writers, language in women's lives, and the relationship between social power and cultural conceptions.

**McDonough, Peter, and Amaury DeSonna.** *THE POLITICS OF POPULATION IN BRAZIL: ELITE AMBIVALENCE AND PUBLIC DEMAND*. Austin: University of Texas Press, 1981. 178 pp. \$19.95. Examines the attitudes of Brazilian government officials and various elite groups toward population planning, and compares them to attitudes of the general public, and concludes that elites seriously underestimate the desire for family planning services. Elites see population issues in the light of national power, but



others are more concerned with the private, economic aspects of family size. Apart from its findings, this study is of interest for its methodological innovations.

**Mausner, Bernard.** *A CITIZEN'S GUIDE TO THE SOCIAL SCIENCES.* Chicago: Nelson-Hall, 1979. 413 pp. Cloth, \$15.95; paper, \$7.95. Explores the basic ideas of interest to sociologists, ethnologists, sociobiologists, anthropologists, social psychologists, and political scientists, and catalogues them with reference to their sources.

**Murdoch, William W.** *THE POVERTY OF NATIONS: THE POLITICAL ECONOMY OF HUNGER AND POPULATION.* Baltimore: Johns Hopkins University Press, 1980. 382 pp. Cloth, \$22.50; paper, \$7.50. This wide-ranging study of the origins and supports of poverty in less developed societies covers not only intrasocietal factors but also the international position of poor countries, concluding that the best hope for reducing poverty lies in socialist rather than reformist solutions. But the author is less than sanguine about the likelihood of such programs, and so sees little chance of improvement in the foreseeable future.

**Nanda, Sachchida, and A. K. Lal.** *ELITE AND DEVELOPMENT.* Atlantic Highlands, N.J.: Humanities Press, 1980. 276 pp. \$15.75. Fourteen articles on elites and development in India treat elite recruitment, rural development, elite-mass relations, neighborhood elites, and bureaucratic elites. The introductory essay discusses the issues involved in applying the concept of elite to Indian society.

**National Research Council.** *DISASTERS AND THE MASS MEDIA.* Washington: National Academy of Sciences, 1980. 299 pp. \$9.75. Researchers and media professionals will find this study in cause and effect useful. The role of media in the preparing for, mitigating, and recovering from disasters is explored.

**Neale, R. S.** *CLASS IN ENGLISH HISTORY: 1680-1850.* Totowa, N.J.: Barnes & Noble, 1981. 250 pp. \$28.50. Ostensibly a survey of the use of "class" and "class consciousness" in social-historical accounts of the English transition to capitalism, a five-class model is also developed. However, the historical examples are used to buttress the case for a social history grounded in theory and thus epistemologically sound. Some Althusserian criticisms of Marxist historiography are accepted (i.e., attack on humanistic historicism), while an emphasis on praxis and the dialectic are retained.

**Nutini, Hugo G., and Betty Bell.** *RITUAL KINSHIP: THE STRUCTURE AND HISTORICAL DEVELOPMENT OF THE COMPADRAZGO SYSTEM IN RURAL TLAXCALA.* Princeton: Princeton University Press, 1981. 494 pp. Cloth, \$28.50; paper, \$11.50. Systems of fictive or artificial kinship, like the Latin American institution of compadrazgo, often play the same role of linking persons in peasant village societies that lineage-structured kinship plays in tribal societies, although the social origins and consequences, of course, are quite different. This study is a particularly important one, because the authors draw on both fieldwork and historical sources, and because they carefully study the relationship between ritual kinship seen as a social institution and seen as a religious institution, without reducing one to the other.

**Parkinson, C. Northcote, and Herman Le Compte.** *THE LAW OF LONGER LIFE.* Troy, Alabama: Troy State University Press, 1980. 253 pp. A British historian of wide-ranging interests and a Belgian physician investigate an intriguing problem: can life span,

and, therefore, possibly the life of our civilization, be extended beyond current expectations? An army of considerations, among them life styles, attitudes, politics, nutrition, medicine, human foibles, are evaluated for their influences on longevity. The authors present bold conclusions on how we all could achieve better tomorrows.

**Pearman, William A., and Robert Rotz.** *THE PROVINCE OF SOCIOLOGY: SELECTED PROFILES.* Chicago: Nelson-Hall, 1981. 208 pp. Cloth, \$15.95; paper, \$8.95. According to the somewhat pretentious introduction to this volume, "like love, sociology is not easily defined." Rather than try, the authors present 72 thumbnail sketches of famous sociologists and fellow-travellers from Auguste Comte to Thomas Szasz and Robin Williams. The book is full of fascinating "facts," such as the declaration that George Homans "is a functionalist theorist who has developed a conceptual scheme very much in the tradition of his Harvard colleague Talcott Parsons," and the curious demographic news that Kingsley Davis was born in 1980.

**Polinsensky, Josef V.** *ARISTOCRATS AND THE CROWD IN THE REVOLUTIONARY YEAR 1848: A CONTRIBUTION TO THE HISTORY OF REVOLUTION AND COUNTER-REVOLUTION.* Albany: State University of New York Press, 1980. 245 pp. Cloth, \$34.00; paper, \$12.95. Among the events of 1848, those in Bohemia are not well known. This detailed work, the last of a trilogy dealing with the relationship of Czech society to revolution, focuses on the relation between the retreating aristocracy and the amorphous, but emergent proletariat in Prague. The revolution is discussed in the context of the revolutionary wave that swept Europe, and particularly that of the relation between Prague and Vienna.

**Raven, Bertram H. (ed.).** *POLICY STUDIES: REVIEW ANNUAL, VOLUME 4.* Beverly Hills: Sage, 1980. 768 pp. Some fifty "policy scientists" contribute articles, without deciding whether "policy studies" is an academic discipline, a combination of the applied wings of extant academic disciplines, or just a matter of offering advice to governments or to the public in general. Chapters treat questions of ethics and identity; concepts and methods; critiques; problems of implementation; and a welter of specific practical, and occasionally empirical, problems. The last include: differential income, poverty, and fiscal containment; energy policy; our changing population; law and courtroom policies; social policy and race relations; medical and health policies; and public policy in the International Year of the Child. A piecemeal approach to social issues seems to be the norm among policy scientists; little is done to integrate the treatment of different problems. But a variety of those problems are interesting, which cannot be said of general musings on the alleged field.

**Richter, Maurice N.** *SOCIETY: A MACROSCOPIC VIEW.* Boston: Hall, 1980. 122 pp. \$15.95. An extended essay on deciding the appropriate level for defining system boundaries in the macrosociological analysis of change. The question is whether the nation-state is an appropriate unit, or whether some larger unit is required. The author, an adherent of social evaluation theory, pays considerable attention to the concept of social differentiation, and to the relations among culture, science, and technology. A useful summary of the conceptual problems facing an increasingly important theoretical enterprise.

**Rogers, Sandra M., and Charles E. King.** *THE ANALYSIS OF SOCIAL INTERACTION: USING THEORY OF ATTRIBUTION.* Washington: University Press of America, 1977. 138 pp. \$6.00. Argues that the explanation of social interaction requires understanding the attributions actors make about themselves and their interaction partners, and that this focus will help minimize biased interpretations by observers. May be useful for supplementary reading at the undergraduate level, although more for the points of view expressed than for its somewhat limited review of sources.

**Rushton, J. Phillipe.** *ALTRUISM, SOCIALIZATION AND SOCIETY.* Englewood Cliffs: Prentice-Hall, 1980. 244 pp. \$16.95. Using a social learning perspective, Rushton surveys and organizes the literature on altruism, and attempts to integrate sociological, anthropological, psychological and biological analyses. Included are considerations of sociobiological foundations of altruism, and the contributions of the family, the mass media and the educational system. Concludes with some comments on the need for greater social supports for altruism, given decreasingly effective socialization by the family.

**Scarritt, James R. (ed.).** *ANALYZING POLITICAL CHANGE IN AFRICA: APPLICATIONS OF A NEW MULTIDIMENSIONAL FRAMEWORK.* Boulder, Colo.: Westview Press, 1980. 360 pp. \$20.00. A series of essays analyzing political change in various parts of contemporary Africa. An extremely complex, abstract model of political change draws very heavily on an old multidimensional framework of social change developed by Talcott Parsons. This model's emphasis on culture and values as the "controlling" factors in the process of change makes the emphases of these articles quite different from that now fashionable.

**Shelley, Louise I.** *CRIME AND MODERNIZATION: THE IMPACT OF INDUSTRIALIZATION AND URBANIZATION ON CRIME.* Carbondale: Southern Illinois University Press, 1981. 186 pp. \$22.50. A historical and cross-national study of the impact of modernization on crime during the last two hundred years. Especially valuable is the inclusion of socialist societies, as well as capitalist and developing societies. The thesis is that modernization has a predictable influence on crime patterns, regardless of the political and economic regime. A bold and imaginative study which will undoubtedly stimulate much debate and further research.

**Shepherd, John, Phil Virden, Graham Vulliamy, and Trevor Wishart.** *WHOSE MUSIC? A SOCIOLOGY OF MUSICAL LANGUAGES.* New Brunswick: Transaction Books, 1977. 300 pp. Cloth, \$19.95; paper, \$7.95. Eight essays united in their effort to reconsider music as an aspect of a wider social reality. Issues include: (1) theoretical work on how music might be considered "meaningful," (2) sophisticated musicological analyses of music's relation to ideology and social stratification, (3) music's definition within formal education, and (4) a piece by Wishart evaluating the cultural critiques of Adorno and Lukacs.

**Sher, Jonathan (ed.).** *RURAL EDUCATION IN URBANIZED NATIONS: ISSUES AND INNOVATIONS.* Boulder, Colo.: Westview Press, 1981. 346 pp. \$30.00. Twelve essays on aspects of rural education in Portugal, Scotland, Australia, Alaska, New Zealand, Finland, Norway, and the U.S. contribute to a neglected area of research. Especially important in light of current re-evaluations of the future of rural populations in urban societies.

**Shevasunt, Somphong, and Dennis Hogan.** FERTILITY AND FAMILY PLANNING IN RURAL NORTHERN THAILAND. *Chicago: The Community and Family Study Center, University of Chicago, 1979. 210 pp.* This report of the Northern Thailand Fertility Study, initiated in 1976, presents data on factors facilitating contraceptive behavior. Comparing couples who have successfully controlled fertility with those who have not, the authors find that organized family planning programs can be effective independent of level of social and economic development.

**Shin, Kilman.** DEATH PENALTY AND CRIME. *Fairfax, Virginia: Center for Economic Analysis, George Mason University, 1978. 279 pp. \$20.00.* Regression and factor analytic techniques applied to the study of murder rates in the U.S. lead the author to conclude that the death penalty is ineffective in reducing murder rates. He suggests that changes in the wider society, and especially gun control, would have a greater effect than reintroducing capital punishment.

**Silverman, Sydel (ed.).** TOTEMS AND TEACHERS: PERSPECTIVES ON THE HISTORY OF ANTHROPOLOGY. *New York: Columbia University Press, 1981. 322 pp. Cloth, \$22.50; paper, \$8.00.* Sociology writes its own history often, if not always well, but anthropologists have given little attention to the history of their discipline; no first-rate study exists. These eight short intellectual biographies help remedy the lack. Each originated as a lecture, and some discussion is transcribed and included. The selection is somewhat arbitrary: Boas, Kroeber, Radin, Malinowski, Benedict, Steward, White and Redfield are here; but not Radcliffe-Brown, Mead, or Evans-Pritchard.

**Slawski, Carl J.** SOCIAL PSYCHOLOGICAL THEORIES: A COMPARATIVE HANDBOOK FOR STUDENTS. *Glenview: Scott, Foresman, 1981. 188 pp. \$8.95.* This concise guide focuses on comparison and application of theories. Each chapter includes a brief presentation, application, and critique of several related theories or theorists, together with study questions, a list of additional readings, and outlines for projects. The author encourages use of the handbook at the undergraduate level, recognizing the need for superior class preparation by students and instructor to profit fully from it.

**Stinnett, Nick, Barbara Chessner, John De Frain, and Patricia Knaub (eds.).** FAMILY STRENGTHS: POSITIVE MODELS FOR FAMILY LIFE. *Lincoln: University of Nebraska Press, 1980. 518 pp. Cloth, \$21.00; paper, \$9.95.* This second book in the continuing series of National Symposia on Building Family Strengths is written by family life educators, practitioners, and researchers who present diverse approaches to building family strengths and include descriptions of existing programs, as well as proposed model programs.

**Tomasson, Richard F. (ed.).** COMPARATIVE STUDIES IN SOCIOLOGY: AN ANNUAL COMPILATION OF RESEARCH. VOL. I. *Greenwich: JAI Press, 1978. 336 pp. \$25.00.* Thirteen essays organized into three groups: on modernization, on social indicators and quality of life, and case studies of two societies. Areas covered include population and modernization, individual aspects of modernity, homicide, revolution, ethnic systems, pension systems, genocide, and patterns of development.

**Tomasson, Richard F. (ed.).** COMPARATIVE STUDIES IN SOCIOLOGY: AN ANNUAL COMPILATION OF RESEARCH. VOL. II. *Greenwich: JAI Press, 1979. 345 pp. \$25.00.* Under the major headings of developed societies, underdeveloped societies, and theories

in comparative studies, thirteen articles cover such issues as public opinion, education and social equality, migrant workers, politics and economic growth, religion, the world system, elites and population, social welfare, and inter-organizational relations.

**Tucker, Frank H.** *THE FRONTIER SPIRIT AND PROGRESS*. Chicago: Nelson-Hall, 1980. 371 pp. Cloth, \$23.95; paper, \$11.95. This interesting exercise in comparative social history looks at the cultural components of frontier society in the U.S., Germany, Japan, and the USSR. The imaginative use of a diverse array of sources of data (including literature and the content of mass communications) is especially instructive, while the author's conclusions regarding basic similarities in the development of frontiers should stimulate further work in this area by sociologists.

**Wartella, Ellen (ed.).** *CHILDREN COMMUNICATING: MEDIA AND DEVELOPMENT OF THOUGHT, SPEECH, UNDERSTANDING*. Beverly Hills: Sage, 1979. 280 pp. Cloth, \$18.50; paper, \$8.95. Developmental psychology and communications theory meet in this collection of eight essays. Half are on the general issues, half are specifically on television.

**Wilson, Peter J.** *MAN, THE PROMISING PRIMATE: THE CONDITIONS OF HUMAN EVALUATION*. New Haven: Yale University Press, 1980. 185 pp. \$12.95. Speculates on the conditions that made possible human society and its development. Beginning with the Plio-Pleistocene era, in which the primate order emerged and diverged, this study aims to go beyond inquiries that look at human beings as if people, animals, plants, and machines are things of the same sort.


**Weinstein, Fred.** *THE DYNAMICS OF NAZISM: LEADERSHIP, IDEOLOGY, AND THE HOLOCAUST*. New York: Academic Press, 1980. 168 pp. \$13.50. Nazism was not a class phenomenon, according to Weinstein; nor do any of the other "common sense" explanations of the event hold. Instead, we must understand Nazism subjectively, in terms of the kind of leadership and ideology which mobilized the population at large. When we do so, Weinstein argues, we discover that the key to Nazism was Aryan racism.

**Wynne, Edward A.** *LOOKING AT SCHOOLS: GOOD, BAD, AND INDIFFERENT*. Lexington, Ma.: Lexington Books, 1980. 235 pp. \$22.95. An investigation of 167 schools—public and private, elementary and secondary, urban and suburban—looking at attitudes of students, parents, and educators. The significant effect on academic progress and character development is traced to such factors as: the imagination, determination, and stability of the staff; school size; and parent and staff aspirations.

**Yeates, Maurice.** *NORTH AMERICAN URBAN PATTERNS*. New York: Wiley, 1980. 168 pp. \$27.95. Focusing on differences among the major urban regions of the U.S. and Canada, this book attempts to discern causes and consequences of changing urban patterns. Yeates writes with an eye throughout to the policy implications of physical growth and population shifts. As a Canadian geographer, Yeates is particularly sensitive to national differences, but these turn out to be a very minor theme in the book.

**Youniss, James.** *PARENTS AND PEERS IN SOCIAL DEVELOPMENT: A SULLIVAN-PIAGET PERSPECTIVE*. Chicago: University of Chicago Press, 1980. 301 pp. \$23.00. A synthesis of

the views of Harry Stack Sullivan, psychiatric theorist, and Jean Piaget, psychologist and philosopher of knowledge, together with a wealth of empirical data, combine to persuade that children's relations with their friends and peers make distinctive and critically important contributions to sensitivity, self-understanding, and interpersonal cooperation.

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## Notes on the Authors

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Paul **Allison**, along with J. Scott Long, is investigating the relationship between university department prestige and the productivity of individual scientists. He is also developing new methods for the analysis of event history data.

His interest in the culture of organizations has its counterpart in Thomas **Dimieri's** study (with Carol Mueller) of the ideology informing a social movement. Recently he has investigated the process of innovation in police agencies. He is studying the structure of decision-making in public agencies and the means of achieving accountability.

George **Dowdall** ("Employment and Mental Hospitalization . . .") studies social and personal aspects of total institutions. His recent research approaches the psychiatric hospital as a system deploying resources directed toward social control.

At the time of his study with co-author Parker Frisbie, Isaac **Eberstein** was working with the State Data Center program in Mississippi. He continues to examine inter-area commodity flows to uncover the structure of the urban system in the United States. He is also working on the demography of minority groups, levels of marital stability, and mortality.

William **Falk's** chief research interest is in the sociology of education. He is studying school teacher strikes, attrition among cohorts of high school students, and social and economic attainment among young adults in the United States.

Harold **Fallding** is at work on a book that reviews the contributions of the social process theorists; a book that will introduce a new theory about conditions under which people move toward cooperation through making allies. Its likely title is *The Social Process: Achieving Human Interests Through Alliance and Opposition*.

With co-author Thomas Gannon, Elizabeth **Freidheim** comments on two articles by Bruce Mayhew (*Social Forces* 59:335–75, 627–48). Her special interests lie in the sociology of institutions, particularly science and law.

W. Parker **Frisbie's** "Metropolitan Function and Interdependence . . ." reflects his continuing research interest in urban ecology. His present work includes studies of ecological models of migration, household-family demography among Hispanics, and the measurement of technological change. (See his recent papers with Clifford Clarke in *SF*, December 1979 and December 1980.)

Thomas **Gannon's** chief interests lie in organization theory and the sociology of religion. He is working on a study of values and beliefs among college students and faculty; and on a study of cross-cultural convergences in the sociology of religion.

Linda **George** has been doing research on social stress and means of coping with it; variation in the use of health services; and the effect of role transitions on personal identity. On her agenda for future research are studies of the social epidemiology of mental illness.

His paper with Ronald Kessler hardly reflects David **Greenberg's** wide range of research interests which include the origins of written legal codes in antiquity, social sources of intolerance of homosexuality, patterns of black migration in the U.S., and economic growth in the Third World.

Ronald **Kessler** is doing work on the social epidemiology of mental illness, in particular, differential response to stress. He is studying the relationship between socioeconomic status and mild forms of anxiety and depression; and, in collaboration with James McRae, the relationship between gender and depression over the life cycle.

Joseph **Lawrence** ("Fear of Crime . . .") is studying changes in crime control bureaucracies—size of urban police forces and variations in arrest rates in recent years—as a function of various structural features. He is also completing an experimental study of methods of teaching sociology, testing the effectiveness of a modified personalized system of instruction (PSI).

Lawrence's co-author, Allen **Liska**, is studying social control systems in major United States cities, and the capacity of models generated by conflict theory to explain variations in social control across these cities.

His article in this issue joins Charles **Logan's** principal specialties, criminology and research methods. His current work touches problems of deterrence, parole, criminal justice philosophy, and evaluation research.

J. Scott **Long's** current research follows two paths: the process of stratification in scientists' careers; and multivariate statistical techniques.

Once a graduate student at Louisiana State University, **George Lord, III** specializes in problems of stratification, particularly as they emerge in a dual economy. He has also collaborated with William Falk in research on teacher strikes.

The article on "Compliance Under Threat . . ." reveals David **Luckenbill's** special interest in the dynamics of interpersonal coercion. He is also doing research on social organization and deviance; and deviant career mobility.

**Robert McGinnis** directs the Research Program on Social Analyses of Science Systems at Cornell. He is doing research on networks of basic and applied research, researchers, and research organizations in agricultural science.

**James Marshall** studies the way social life causes or prevents mental disorder, suicide and chronic diseases. He is engaged in a long-term study of the social epidemiology of cancer.

**Armand Mauss** comments on Rooney's "Organizational Success Through Program Failure" (SF 58:3). He writes and teaches mainly in the fields of the sociology of religion, the sociology of deviance, and social movements/collective behavior. He is presently completing a federal project evaluating an alcohol abuse prevention program for school children.

**Marietta Morrissey** is interested in Latin American development and international stratification, including the integration of Third World immigrants into United States labor markets.

Her work in this issue (with Thomas Dimieri) is part of a clutch of Carol **Mueller's** studies which include a description of the impact of the women's movement on the changing characteristics of women in public office and a typology of collective responses to tokenism among women in non-traditional work. Her future plans include an analysis of the polarization of public opinion, over the course of the seventies between feminist and "pro-family" positions.

**Dr. H. B. M. Murphy**, a well-known figure in the field of social and trans-cultural psychiatry, has just retired from the Department of Psychiatry at McGill.

Some of her general interests are represented in Elizabeth **Mutran's** paper in this issue (with Linda George). She is conducting research on the meaning of identity in old age and its relationship to psychological well-being

—including the effects of retirement and widowhood on permutations of self-conception.

At the Institute for Social Research, Willard **Rodgers** is completing projects on the quality of life in a metropolitan area; and on the quality of life of older people. He is also examining the adequacy of interview data from older respondents, the validity and reliability of such data compared to that from younger respondents; and compared to that obtained through telephone interviews.

James **Rooney** teaches in the School of Social Service and is a Fellow at the Boys Town Center for the Study of Youth Development at Catholic University.

Andrew **Sanchirico** is a Ph.D. candidate who, for his dissertation research, is examining social psychological factors underlying the relationship between the SESs of families of orientation and procreation.

His paper in this issue is part of Ralph **Sell's** larger program of investigating the impact of occupational and industrial structure on migration. He is currently collecting data from several corporations to determine how policies and practices condition the use of local or national labor markets for employee allocation and socialization.

Joel **Smith** (with John Zipp) is studying the processes by which Canadians in and out of Quebec formed their interpretations of the meaning for Canada in last years referendum in Quebec. He is also involved in a study of the impact of local physical and social environments on vital processes in American metropolitan communities.

Michel **Tousignant** is in the Department of Psychology at the Université du Québec à Montréal. He is working on a study of sex biases in epidemiological surveys of mental health. During 1981–82 he will spend a sabbatical year among the Highlands Indian tribes of Ecuador to study their notions of emotion.

In describing his current and projected research, John **Zipp** writes that he is working on a project to examine support for economic democracy in the United States; and is beginning a study to assess the impact of a plant closing on the workers and communities involved.



# The Structure of Belief Systems Among Contending ERA Activists\*

CAROL MUELLER, Wellesley College

THOMAS DIMIERI, Wellesley College

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## ABSTRACT

*The structures of beliefs on feminist policy preferences are compared for activists who led campaigns supporting and opposing a Massachusetts referendum on equal rights for women (ERA) in 1976. Beliefs of opposing activists demonstrate asymmetric structures. The preferences for feminist policies of ERA proponents show significantly greater constraint, consensus, and position extremity than do those of ERA opponents. These findings are explained in terms of differences in the group processes among activists within social movements that assert new claims and responsibilities compared with those that defend old rights and privileges.*

Are there systematic differences in the patterns of policy preferences held by social movement activists supporting and opposing change? For almost twenty years, Tilly has elaborated systematic differences at the structural level which distinguish proactive movements supporting change from reactive movements opposing. This paper extends that discussion by arguing that the organization of proactive and reactive belief systems differs systematically as well. Data to test this thesis are preferences on feminist policies held by activists working for and against an Equal Rights Amendment (ERA) to the state constitution of Massachusetts in 1976.

\*An earlier version of this paper was presented at the annual meeting of the American Sociological Association, 1980. Preparation of the paper was made possible by a postdoctoral grant, #1F32 MHO7811-01 from the National Institute of Mental Health to the first author. Data collection and analysis were funded by a faculty grant to both authors from Wellesley College and continued support from its Center for Research on Women and the Wellesley College Computer Center. We thank Henry Noerenberg, our collaborator on the Massachusetts state survey, and Andrea Levere, who conducted most of the interviews with Massachusetts activists. We have benefited from the comments of Myra Ferree, Charles Judd, Robert Mitchell, Tahi Mottl, and Frances Fox Piven.

## THE ORGANIZATION OF BELIEFS

Before addressing the central thesis of this paper, it is necessary to indicate what we mean by the organization or structuring of belief systems.<sup>1</sup> Although beliefs can be highly complex systems of thought and meaning, we have followed the lead of political scientists in simplifying our subject by concentrating on the public policies that are proposed by a proactive movement and, in turn, opposed by a reactive movement. The belief systems approach which we employ is adapted for analysis of social movement ideologies from a rich tradition of research which has developed in response to Converse's classic paper, "The Nature of Belief Systems in Mass Publics."

The belief systems described by political scientists consist primarily of expressed preferences for candidates, issues, and parties (Campbell et al.). The consistency of preferences is evaluated over a policy domain such as domestic or foreign affairs. Consistent positions usually fall along a dimension ranging from liberal to conservative. Consistency of preferences may then be assessed for different groups in order to compare the coherence or integration of their belief systems and their correspondence with a liberal or conservative orientation. A previous paper (Mueller and Judd) discusses the appropriateness of this paradigm for the study of three important characteristics of social movement beliefs—constraint, consensus, and position extremity.

Constraint is a property of individuals describing the degree to which one belief or preference can be predicted from another. Consensus is a group property that describes the average variability on a set of specific beliefs within a group. Position extremity is the difference between average beliefs of two or more groups or populations where one average is taken as the standard. It is, thus, a property of the relationship between the two groups. Mueller and Judd found, for instance, that the sex role beliefs for members of a social movement organization (the Boston Chapter of the National Organization of Women) were more highly constrained for individual members and showed more consensus within the group than the same beliefs for women who belonged to a nonfeminist political organization (the Massachusetts Federation of Republican Women—MFRW). Not surprisingly, the average belief for NOW women was significantly more feminist on a 9-point scale than MFRW women. Thus, these three dimensions may be used to compare the structure and coherence of the policy preferences for activists supporting and opposing change.

Our thesis that proactive and reactive belief systems will differ systematically along these three dimensions is based on the ideas developed by Ferree and Miller for applying attribution theory to the study of social movement ideology. Following Tilly, they maintain that there are inherent differences in the degree and source of consensus in social movement

belief systems depending on the type of demands or claims made by the movement. Reactive movements are those which seek to protect old rights; proactive movements demand new rights. Basic to the Ferree–Miller argument is the assumption that reactive demands are still legitimated by the prevailing culture while proactive demands are not. Because of this difference in legitimacy, the proactive movement asserts new rights which have not previously been accepted within the dominant culture as when unions sought the right to bargain collectively. Alternatively, the proactive movement may try to endow new groups with rights previously legitimated for others as when suffragists sought to obtain the vote for women after it had been extended to ever wider categories of men. In this challenge phase of its life cycle, the proactive movement must define and provide symbols for legitimating new, more desirable social arrangements. Consensus on the desirability of the new arrangements develops out of concrete social interaction within a homogeneous, intensively interacting group. In contrast, the demands of the reactive movement are legitimated by traditional definitions of rights and obligations which provide a taken-for-granted basis of consensus. Legitimacy is gained by reference to time-honored slogans or symbols. When specific traditional rights are challenged, reactive demands are very specific also—stop busing or Stop ERA (see Mottl). Rather than develop a new ideology, a reactive movement seeks to attach an additional concern to an existing belief structure without denying its overall legitimacy.

If we examine the differences between reactive and proactive beliefs in terms of consensus, constraint, and position extremity, several propositions are suggested. On issues related to the grievances of social movement members, a proactive group's beliefs should demonstrate greater consensus among group members, greater constraint among the beliefs of individual members, and greater position extremity from the general public than the beliefs of the reactive group. These differences in the patterning of beliefs are primarily accounted for by two factors. Proactive groups have a greater dependence on social interaction to establish and to legitimate the new beliefs. In addition, the development of the proactive belief system arises as a challenge to older beliefs and the groups protected by them. These two conditions, high levels of interaction and a situation of group antagonism, are similar to conditions identified by political scientists as the basis for higher levels of belief constraint among different segments of the electorate (Aberbach and Walker; Nie et al.). Consensus seems to arise similarly on the group level (McClosky et al.). The average distance of proactive beliefs from those of the general public should be greater because of the innovative and initially illegitimate nature of proactive beliefs and the fact that these arise and are disseminated through concrete social interaction, a slow process similar to conversion (for a general discussion, see Gerlach and Hine; and, on the women's movement, Cassell).

The beliefs of reactive groups should show less constraint, less consensus, and less position extremity than the beliefs of the proactive group. This is due to the reactive group's essential adherence to the prevailing ideology. Because they only seek to reaffirm former rights, their beliefs should have a good deal of the looseness and contradictions associated with everyday definitions of the social world (see Bittner for this contrast). Grievances of the reactive group arise out of ad hoc situations created by changing conditions or a threat from a proactive movement. Thus, inadequate time is available to invest in the prolonged social interactions necessary for closely integrating the beliefs within either individuals or groups. The reactive group accepts most of the dominant ideology and seeks only minor restitution. As a result, it should be closer to the general public than a proactive group on most beliefs.

#### HYPOTHESES

These propositions are tested with interview data collected from opposing leaders of campaigns working for and against a Massachusetts referendum on a state Equal Rights endment (ERA) in 1976. This campaign is selected to represent Tilly's distinction between proactive and reactive social movements. He uses the proactive—reactive distinction to characterize the opposing political groups which are mobilized in response to major structural transformations in society (see Tilly et al.). Proactive groups mobilize to take advantage of structural changes; reactive groups mobilize to defend the old order. Such is the nature of the campaigns for the federal ERA.<sup>2</sup>

A major transformation is occurring in the roles of women. These changes center around women's increasing entry into the paid labor force, the decline in fertility, and the increase in divorce and marital instability (see Klein for a comprehensive discussion of these trends). Some women are structurally positioned to experience these changes as the basis of new opportunities, rights, and privileges. Others occupy positions from which these changes are viewed as a threat to the traditional rights and privileges of wives and mothers. Women on both sides have organized politically. The ERA is the issue which has most dramatically represented these changes and what is being won and lost for many women. Since its material implications remain ambiguous, the focus on the ERA is probably due to its symbolic importance (Mueller). If proponents win, the amendment will lend the weight of Constitutional legitimacy to changes that are presently occurring in women's roles. If opponents defeat it, they will have won at least a temporary symbolic victory by denying Constitutional sanction to macrolevel sources of change which are less easily influenced by grassroots mobilization.

In stating the previous propositions more specifically as hypotheses, we are assuming that the ERA referendum of 1976 in Massachusetts oc-

curred during the challenge phase of a social movement cycle. It seems likely to us that the ERA was one issue in a highly integrated set of feminist beliefs for proponents. This would be consistent with other reports that early ideological differences within the contemporary woman's movement had been resolved on most issues by the mid-seventies (Carden; Freeman). For opponents, however, the existence of a tightly constrained set of anti-feminist beliefs in 1976 was less likely. Reactive movements in defense of family and neighborhood that opposed busing (Mottl), abortion (Leahy), and the ERA (Arrington and Kyle; Brady and Tedin) had shown little evidence of coordination by 1976.

Thus the belief system framework applied to campaign activists working for and against the ERA suggests the following hypotheses:

1. *Policy preferences on feminist issues will be significantly more constrained for individual members of the leadership supporting the ERA than for leaders opposing.*
2. *Consensus on policy preferences will be significantly greater within the leadership group supporting the ERA than within the opposing leadership group.*
3. *The average policy preference of leaders opposing the ERA will be less polarized from the views of women in the general population than that of leaders supporting the amendment.*

Hypotheses 1 and 2 are tested with data from interviews with the leaders of groups working for and against the ERA in a 1976 Massachusetts referendum. For Hypothesis 3, a survey of Massachusetts adults conducted immediately after the 1976 election serves as baseline data for the measure of position extremity.

## The Study

### CAMPAIGN ACTIVISTS IN MASSACHUSETTS

Frustrated with repeated failures to pass the federal ERA, NOW members submitted a bill in the 1971-72 legislative session to guarantee equal rights on the basis of race, creed, and national origin as well as sex. The state bill was allowed to die, however, after the 1972 passage of the federal amendment by Congress and 26 states, including Massachusetts. As momentum for the federal amendment slowed in 1973, the state bill was reintroduced. Lobbying for the bill was directed by a coalition including the Governor's Commission on the Status of Women, the (Boston) Mayor's Commission to Improve the Status of Women, the Massachusetts Women's Political Caucus, the Women's Equity Action League, and the Women's Lobby. It passed unanimously and without debate in August 1973.

In the two years before the bill was submitted to the legislature for a

second and final vote, a national organization opposing the federal amendment, STOP ERA, had spread to 40 states. Arguments against the amendment were based on the general theme that the sex neutral guarantees of the ERA would destroy the family. In addition, the ERA would promote sexual promiscuity by bringing men and women together unnaturally in police cars, in prisons, on sports teams, and in combat units. It would also legalize abortion and homosexual marriages.

The impact of the national controversy generated by the arguments and organizing efforts of STOP ERA and its chief spokesperson, Phyllis Schlafly, became clear when hearings were called for the final vote on the Massachusetts ERA in February 1975. In a crowded hearing room, speakers from the conservative Women for Constitutional Government, who had affiliated with the national STOP ERA, surprised legislators as well as proponents. The hearings marked the beginning of a heated public controversy accompanied by intensive lobbying and letter writing. Only intensive lobbying and parliamentary maneuvering by the proponents achieved final approval in May by a vote of 217-55. The final step was a state referendum set for November 1976.

Immediately after the May 1975 vote, the ERA Coalition began to mobilize for a statewide campaign to reach voters. A highly centralized structure was developed with task forces for education, outreach, organization, fundraising, and media relations. The controversy of the previous spring and the defeats of state referenda in New York and New Jersey in the fall of 1975 led the Committee to expect a difficult campaign. In January of 1976, it launched its campaign with 23 committees located across the state. By summer, the Committee was able to hire a full-time campaign worker and open a downtown Boston headquarters. During the ten months of the campaign, the Committee was able to draw on a list of 5,000 identified volunteers throughout the state and recruited 75 speakers. Campaign funds of \$66,000 were raised.

The opposition presented a very different picture. Despite their strong showing with the legislature in the spring of 1975, organized efforts against the ERA referendum did not begin until the summer of 1976. The campaign developed around two small and largely independent groups from the north and south of Boston—a STOP ERA affiliate in the north and an ad hoc group called ALERT on the South Shore. While the North Shore group recruited volunteers through the John Birch Society, opposition to abortion was the common link with the South Shore group. There was little role specialization or formal organization. Most of the half dozen core members of each group performed tasks encompassing the full range of campaign activities. The two groups not only failed to coordinate their activities, but the chief public speaker against the ERA disassociated herself from the opposition leadership on several occasions. In their interviews, respondents noted continual difficulty in recruiting volunteers and raising

money. The total campaign funds spent by the opposition were less than \$5,000.

#### SAMPLES

The study is based on interviews with ERA campaign leaders and a state-wide sample of Massachusetts residents. Leaders were initially defined as individuals who were influential in shaping policy for the two campaigns. In the process of identifying leaders, it was soon discovered that the Committee to Ratify was considerably larger and more formally organized than either STOP ERA or ALERT. Given the lack of comparability in the opposing campaign organizations, all members of STOP ERA and ALERT but only key decision-makers in the Committee to Ratify were identified and interviewed.<sup>3</sup> In both cases, identification began with those who were publicly known. They, in turn, identified others and the process continued until no new persons were named. The total number interviewed was 17 proponents and 13 opponents. Of the 13 opposition leaders, 2 were men; all proponents were women. Only one leader, an opponent, refused to participate. Interviews of most proponents took place before the November election. All opposition leaders and key leaders of the Committee to Ratify were interviewed after the election because of their apprehensions that sensitive information might be released which would adversely affect their respective campaigns.

Interviews with Massachusetts residents were based on a stratified cluster sample of residents selected from telephone listings. Respondents were interviewed by students from Wellesley College over a two-week period immediately following the 1976 election. Based on U.S. Census data for Massachusetts minor adjustments by sex, age, and education were made in the sample. After weighting, the sample size was 407 of whom 306 had voted on the ERA.<sup>4</sup> Because most discussion of the ERA has focused on the opposition of women, the comparison here between activists and voters is confined to the 167 women voters.<sup>5</sup>

#### FEMINIST POLICY PREFERENCES

Data for describing campaign leaders' preferences on feminist policies consist of thirteen statements which were described to respondents as "specific demands that have been made in the name of changing women's status for the better" (see Appendix). Respondents were asked to judge whether each demand would help improve women's place in society, neither help nor hurt, or hurt. Items considered "helpful" were scored as one, "hurt" as three, and "neither help nor hurt" as two. The items range from those which clearly require government action such as "government support for day care centers" to those which are almost entirely matters of

private morality such as "giving women the same sexual freedom as men." Despite this range, most of the demands require government funding, tax incentives, executive action, judicial or legislative support. The items and question wording are from Ferree. Six of the items were also asked on the state survey of Massachusetts residents (see items with asterisks in the Appendix).

## Results

Like other studies of ERA activists (Arrington and Kyle; Brady and Tedin; Tedin et al.), proponents and opponents differed significantly on most social characteristics relevant to feminist beliefs (see Table 1). Opponents

**Table 1.** SOCIAL CHARACTERISTICS OF ERA PROponents AND OPPONENTS (IN PERCENT)

	Proponents (N=17)	Opponents (N=13)	r
<u>Marital Status</u>			
Married	47.1	92.3	-.407**
Separated	11.8	0.0	
Divorced	11.8	0.0	
Never married	29.4	7.7	
<u>Age</u>			
20-30	23.5	8.3	.312*
31-40	52.9	25.0	
41-50	11.8	41.7	
51-60	5.9	25.0	
61-70	5.9	0.0	
<u>Employment Status</u>			
Full time	64.7	30.3	.336*
Part time/no outside employment	35.3	69.2	
<u>Type of Employment</u>			
Professional, technical	83.3	28.6	.582**
Management, administration	8.3	14.3	
Sales	8.3	42.9	
Service	0.0	14.3	
<u>Education</u>			
High school or trade graduate	5.9	23.1	-.224
Some college	5.9	7.1	
College graduate	41.2	38.5	
Some graduate school	17.6	7.7	
Graduate degree	29.4	23.1	

\*p ≤ .05

\*\*p ≤ .01



were older and more likely to be married and housewives. Full-time employment, usually in a professional or technical job, was significantly more characteristic of proponents. Most striking, however, were differences in the importance of religion (see Table 2). All of the opponents were currently members of a church or parish; all stated that religion was very important to them personally; and, two-thirds had received some part of their education in a religious school.

In contrast to opponents in southern states, however, the women who led the opposition to the ERA referendum in Massachusetts were predominantly Catholic. They also differed from ERA opponents studied elsewhere in their high level of education and involvement in electoral politics.

Despite unexpected similarities in levels of education and political

**Table 2.** RELIGIOUS AND POLITICAL PARTICIPATION CHARACTERISTICS OF ERA PROPONENTS AND OPPONENTS (IN PERCENT)

	Proponents (N=17)	Opponents (N=13)	r
<u>Current Religious Membership</u>			
Yes	31.3	100.0	-.705***
No	68.8		
<u>Personal Importance of Religion</u>			
Very important	15.4	100.0	-.745***
Somewhat important	30.8		
Not very important	15.4		
Not at all important	38.5		
<u>Religious Education</u>			
Yes	23.5	61.5	-.385*
No	76.5	38.5	
<u>Religious Preference</u>			
None	31.3	0.0	.549***
Jew	6.3	0.0	
Protestant	37.5	23.1	
Catholic	25.0	76.9	
<u>Political Participation</u> (percents refer to affirmative answers)			
Appointed to office	41.2	23.1	.190
Run for office	23.5	15.4	.101
Speak for candidate	41.2	53.8	-.126
Influence friends for candidate	94.1	100.0	-.162
Attend political meeting for candidate	94.1	92.3	.036
Write to public official	100.0	92.3	.212
Talk to elected official	94.1	92.3	.036
Give campaign money	82.4	92.3	-.145
Registered to vote	100.0	100.0	.000
<u>Political Orientation</u>			
Conservatism score ( $\bar{X}$ )	14.06	21.31	.661***

\*p < .05

\*\*p < .01

\*\*\*p < .001

participation, the general political orientations of the contending leaders differed in the expected direction. A conservatism score was based on level of concern for five issues (crime, morality in government, sexual morality, size of federal budget, and the military position of the U.S.) which had high loadings on the first factor in a single component factor analysis of eight concern items. Opponents scored almost twice as high as proponents on the conservatism score (see Table 3).

Organization affiliations reinforce differences in structural characteristics and in political orientation. The inner core of decision-makers for the Committee to Ratify consisted largely of the women who had organized the Boston Chapter of NOW in the early seventies and were consid-

**Table 3.** GROUP MEANS, VARIANCES, AND VALUE OF *t* ON THIRTEEN FEMINIST POLICY QUESTIONS FOR ERA CAMPAIGN LEADERS

Feminist Policy Item <sup>a</sup>	Group Mean		Variance		<i>t</i> values
	Pro-ERA (N=17)	Anti-ERA (N=13)	Pro-ERA	Anti-ERA	
1. Equal pay for equal work <sup>b</sup>	1.000	1.154	.000	.308	-1.00
2. Elect more women <sup>b</sup>	1.000	1.308	.000	.231	-2.31*
3. Keep maiden names <sup>b</sup>	1.000	2.692	.000	.231	-12.70***
4. Equal school sports funding	1.000	2.000	.000	.833	-3.95**
5. Use of term Ms.	1.177	2.615	.154	.423	-7.05***
6. Allowing girls on boys' teams	1.235	2.692	.441	.231	-6.97***
7. Governmental support for day care <sup>b</sup>	1.118	2.539	.110	.603	-6.18***
8. Show more competent women on TV	1.000	1.462	.000	.436	-2.52*
9. Encourage girls to enter professions	1.000	1.231	.000	.192	-1.90
10. Same sexual freedom for women	1.000	2.462	.000	.603	-6.79***
11. Right to abortion <sup>b</sup>	1.000	2.923	.000	.077	-25.00***
12. Quotas in executive jobs and professional schools	1.353	3.000	.618	.000	-8.64***
13. Paternity leave <sup>b</sup>	1.118	2.615	.110	.590	-6.58***
Group Averages	1.077	2.207			-15.69***
			.110	.366	2.97**

<sup>a</sup>See Appendix for exact wording of items.

<sup>b</sup>Items used in the state survey.

\**p* ≤ .05

\*\**p* ≤ .01

\*\*\**p* ≤ .001

ered by more peripheral members of the Committee to be founders of the women's movement in Massachusetts. Eleven of the 17 proponent leaders belonged to NOW; 3 of the remaining 6 belonged to other feminist organizations. Affiliations with schools or professions predominated over social service and strictly civic concerns. Politically, affiliations with the Civil Liberties Union, Common Cause, the Congress of Racial Equality and Planned Parenthood reflected a consistent array of liberal concerns.

Over half of the opposition leaders from both the North Shore STOP ERA and the South Shore ALERT were recruited into ERA opposition through their affiliations with pro-life groups. Most of the others were members of conservative political organizations such as the John Birch Society, Eagle Forum, Women for Constitutional Government, and the Daughters of the American Revolution. Although a few belonged to professional organizations, they were more likely to be leaders of groups concerned with children—ranging from Scouts and the YMCA to the local school committee and auxiliary groups within their local churches.

Because the two campaigns were very different in both their duration and their degree of formal organization, it is also important in evaluating the hypotheses to consider whether the individual leaders in the two campaigns differed significantly in their campaign participation. Despite the earlier start of the Committee to Ratify, the proponent leaders we have interviewed did not differ significantly from opposition leaders in the number of years they had worked on the issue. Although one proponent had begun working for the state amendment before the federal ERA passed Congress in 1972, 85 percent of both proponents and opponents had been involved for no more than two years. In terms of the intensity of their participation, we also found no significant differences between the two groups of leaders in either the number of campaign activities attended weekly or the number of hours spent on the campaign per week.

#### CONSTRAINT ON FEMINIST POLICY PREFERENCES

From Hypothesis 1, we expect to find that constraint on feminist policy preferences is higher among proponents than opponents. Traditionally, constraint has been assessed in political science by computing correlations between pairs of policy items across respondents within a group, averaging within groups and then comparing across groups (Campbell et al.; Converse). Previous research on belief systems has shown, however, that high levels of group consensus (and, thus, low variance) artificially reduce the degree of association among items (Barton and Parsons). A similar problem is found here among proponent activists on their policy preferences (see variances in Table 3). Instead of correlational techniques, we will follow the procedure outlined in Mueller and Judd of measuring constraint by calculating for each individual the variance of responses across the

thirteen items. This measure of an individual's belief constraint is then averaged for each group.

Comparing the average within-individual variances on the thirteen policy items, we find an average variance of .098 for proponents and .710 for opponents. These two mean values are significantly different ( $t$  with 28 *d.f.* 8.63;  $p < .0001$ ) indicating the greater consistency (lower average variance) of policy preferences among proponents.

#### CONSENSUS ON FEMINIST POLICY PREFERENCES

While constraint provides a measure of homogeneity within the individual's belief system, consensus measures homogeneity of belief within each of the two groups of campaign leaders. Thus, from<sup>4</sup> Hypothesis 2, we expect that within-group consensus will be greater among proponents than among opponents. Consensus is measured by comparing the variance across respondents on each of the thirteen items for the two groups separately. As indicated in Table 3, no variance is found among supporters of ERA on eight of the thirteen policy items. On these items, all proponents gave the identical answer. Of the five remaining items, only two (six and twelve) have a lower variance for opponents than for proponents. Thus, it is not surprising that the average variance over the thirteen items for opponents (.366) is more than three times that of proponents (.110). Using a  $t$ -test for matched samples, these average group variances are found to differ significantly ( $t$  with 12 *d.f.* 2.9655,  $p < .01$ ). We find, then, that not only are ERA proponents more consistent than opponents in their policy preferences as individuals, but they also show a much higher level of consensus within the group.

#### POLARIZATION ON FEMINIST POLICY PREFERENCES

For Hypothesis 3, we expect to find that the average policy position (support or opposition) of ERA opposition leaders is closer to that of women in the electorate than to the average position of the proponent leaders. To make this comparison, women in the electorate are divided into those who reported a favorable vote on the ERA ( $N=124$ ) and those who reported a negative vote ( $N=43$ ). We then calculate an individual's average score over all preference items and compare the average individual differences between the four groups—proponent and opponent activists and voters. For these tests on position extremity, the six policy questions are used for which we have comparable data from both leaders and voters (see items with asterisks in the Appendix).

As indicated in Table 4, average individual preferences on feminist policies range from ERA proponents at the most favorable end (lowest value) to ERA opponents at the opposite end (highest value). Mean values

**Table 4.** AVERAGE INDIVIDUAL PREFERENCES ON SIX FEMINIST POLICIES FOR WOMEN VOTERS AND CAMPAIGN LEADERS

Policy Preferences	Proponents		Opponents		t value
	Mean	Variance	Mean	Variance	
<b>Leaders</b>	(N = 17)		(N = 13)		
Individual Averages (over six items)	1.039	.009	2.205	.089	-15.26***
<b>Women Voters</b>	(N = 124)		(N = 43)		
Individual Averages (over six items)	1.521	.177	1.732	.143	-2.43*
<b>Group Averages</b>					
Equal pay for equal work	1.049	.072	1.098	.172	-.88
Elect more women	1.248	.355	1.408	.563	-1.42
Keep maiden names	1.933	.812	2.248	.707	-2.03*
Government support for day care	1.351	.457	1.892	.783	-2.00*
Right to abortion	1.737	.773	1.608	.903	-.99
Paternity leave	1.807	.810	2.139	.865	-2.05*

\*p ≤ .05

\*\*\*p ≤ .001

for the two groups of voters fall in order between the two extremes. The same ranking on support for feminist policies holds among the four groups on the six individual items as well. In terms of individual averages over the six policy questions, the positions of proponents and opponents are significantly different for both leaders and voters. We see in Table 4, however, that a woman's vote on the ERA predicts her position on only half of the individual policy questions. This is because most women voters, regardless of their vote on the ERA, accept the demand of equal pay for equal work as well as the value of electing more women to public office. On the abortion issue, both groups of voters are very near the neutral position of 2.0, although the variances indicate considerable difference of opinion in both groups of voters. For the other three issues, the ERA vote predicts the policy position.

If Hypothesis 3 is correct, the difference between the policy preference position of women voters and proponent leaders will be significant while that between voters and opponents will not. The *t* values in Table 5 indicate that this is not the case. The position of opponent as well as proponent leaders is significantly different from that of women voters as a whole (row three). In addition, we find that both groups of leaders not only hold policy positions significantly different from women who voted against them on the ERA, but also from those who voted with them. Thus, women voters on both sides of the ERA issue hold moderate positions on other feminist policies in comparison with the campaign leaders. Contrary to our expectations, they are no closer to opponents than to proponents.

**Table 5.** VALUES OF *t* FOR DIFFERENCE OF MEANS TESTS BETWEEN CAMPAIGN LEADERS AND VOTERS ON AVERAGE FEMINIST POLICY PREFERENCES

Women Voters	Leaders	
	Proponents	Opponents
Proponents	32.113***	15.204***
Opponents	20.379***	18.192***
All	59.556***	9.333***

\*\*\**p* ≤ .001

Before discussing the implications of these results, an obvious caveat is in order. This extensive discussion is based primarily on interviews with a small number of campaign leaders. Such a careful examination of the patterning of their policy preferences seemed justified for several reasons. Most importantly, there are seldom more than a handful of people directing a political campaign despite, as in the case of the proponents, a volunteer army of 5,000. Also, by comparing the patterning of preferences between opposing leadership groups with those of women in the general population, we have also provided a context for evaluating the unique type of structures which distinguish activists from nonactivists. Finally, our approach has the added advantage of collecting data during and immediately following a campaign.

**Conclusions**

Previous theories of social movement interaction have drawn on the conflict theories of social psychologists to argue that beliefs polarize symmetrically in a situation of conflict. Oberschall, for instance, has argued that belief polarization is the psychological counterpart of social polarization between opposing social movements. The two are considered mutually supporting processes in which conciliatory individuals are driven out of leadership positions in opposing movements and moderate or inconsistent attitudes are eliminated from the beliefs of participants on both sides of the conflict. Drawing on the work in social psychology of Sherif and Deutsch, Oberschall implies that conflict produces two sets of consistent but opposing beliefs within each of the contending movements.

The finding here that policy preferences on feminist issues are more

fragmented among reactive opponents than proactive supporters of the ERA does not appear to support this assumption. We have found that the beliefs of a reactive counter-movement are not the mirror image of proactive beliefs. Anti-ERA activists are not as consistently anti-feminist as pro-ERA activists are consistently pro-feminist. As a group, the ERA opponents support feminist policies which would encourage young women to enter the professions as well as policies guaranteeing women's right to equal pay for equal work. The fragmentation of their beliefs is even more strongly demonstrated in the greater variation of their individual responses compared to the activists supporting the ERA. Thus, they disagree more among themselves than proponents on eleven of the thirteen issues. Even on the issue of abortion, they are more divided than proponents.

There are several ways of accounting for the discrepancy between our findings and Oberschall's theory of attitudinal polarization, but we have found two most promising for further exploration. The first approach would give Oberschall the benefit of the doubt by suggesting that the conflict between opposing movements observed here has not been sustained long enough for symmetrical belief polarization to occur. There is recent evidence to support this alternative. In the four years following collection of our data in the 1976 Massachusetts ERA campaign, a national, right wing political movement has coalesced at the organizational level around "pro-family" issues. Along with many issues concerning the public schools, the pro-family program includes opposition to the feminist policies of abortion, homosexual rights, and the ERA (Crawford). This coalition of economic conservatives, evangelicals, and single-issue groups dominated the Republican Platform Committee in the summer of 1980 just as it had demonstrated increasing political effectiveness in the electoral campaigns of the late seventies. Despite their increased importance in electoral politics, however, it is not known whether this coalition has developed among its activists a level of consensus in defense of traditional norms and values comparable to the belief consensus of the proactive feminist movement which preceded it. Comparable levels of consensus and constraint in the pro-family movement would support Oberschall's use of a belief polarization model adapted from research on conflict in small groups and require a different level of explanation.

An alternative interpretation raises the question whether the social psychological model of polarization used by Oberschall is appropriate for the structural level to which it is applied. It seems plausible that the polarization which occurs between social movements may be quite different from that in small groups. For instance, the greater scale of social movements makes it much more likely that communication will be indirect rather than face-to-face. The mediation of the media as the means of communication also introduces a third party to the conflict and the possibility of coalition (see Lipsky). The importance of media support is accentuated by

the fact that social movements struggle for control of the historic symbols conveying legitimacy to rights and privileges as well as for tangible rewards.

Most importantly, however, the emphasis of Ferree and Miller's work on the divergent processes by which consensus and legitimacy are achieved in proactive and reactive movements suggests that asymmetry rather than symmetry of belief structure is more likely at any given time. The presumption in favor of asymmetry is based on the premise that the belief system of a reactive movement always enjoys more cultural legitimacy than that of a proactive movement. From this assumption follows the challenge posture of the proactivists; the guarantee of opposition; a mobilization strategy of conversion through the intense interaction of homogeneous groups; and the greater constraint, consensus, and extremity of beliefs described here.

Equally important to the presumption of asymmetry, however, is the sequencing of life cycles of proactive and reactive movements. There is the strong likelihood that proactive movements will achieve initial successes before reactive movements mobilize (Mottl). In the past two decades, for instance, reactive movements have not mobilized until a proactive movement had won the support of one or more major institutions. The ERA had passed Congress with overwhelming approval before Phyllis Schlafly began nationwide mobilization for STOP ERA in late 1972. Supreme Court decisions favorable to proactive claims for busing and abortion were also handed down before the anti-busing and "right-to-life" movements became widespread. Thus, recent U.S. history suggests that reactive groups may not even appear until their claims have already lost the support of some major institutions. Because of this precedence of the proactive movement, asymmetry in belief structure seems more likely than symmetry at any given point in the rise of the proactive movement.

Life cycle phasing may also reverse the direction of asymmetry found here. There is obviously a stage in the development of a successful proactive movement when the legitimacy of some portion of its program is established and these claims become part of the taken-for-granted assumptions of its beneficiaries as well as powerful institutions in society. Under these circumstances, reactive belief systems may retain their legitimacy at the local or regional level but take on a defensive stance at the level of national culture as the temperance movement did in the period after World War I (Gusfield). To the extent that a reactive movement withdraws from the national culture and formulates its own world view, the structural characteristics of its beliefs may come to approximate those of the proactivists described here with high levels of consensus, constraint, and position extremity. To discover whether such structure exists, however, will require that investigators frame their questions within the context of the reactivists' world view.

One last comment is in order. Tilly's distinction between reactive



and proactive movements has been an essential ingredient in this study of social movement beliefs. This may at first appear incongruous because Tilly is best known for his contributions to the resource mobilization perspective which has generally ignored the part played by beliefs. Tilly's theoretical work, however, has generally accorded more influence to beliefs than that of others in the resource mobilization tradition. While Oberschall and McCarthy and Zald (a, b) have emphasized rational decision-making and the allocation of tangible resources to the pursuit of collective goals, Tilly has insisted that the analysis of collective action take into account "... a group's conception of its aims and rights" (c, 42-3). Although Tilly's empirical work emphasizes the origins of competing claims in macro-structural changes, the proactive-reactive distinction is, in fact, made at the cultural level in terms of a collectively shared sense of the legitimacy of claims both propounded and defended. We find Tilly's proactive-reactive distinction a critical juncture for joining structural and ideological analyses of social movements.

## Notes

- 1 We follow Converse in defining a "belief system [as] a configuration of ideas and attitudes in which the elements are bound together by some form of constraint or functional interdependence" (207). This definition is consistent with usage in social psychology where attitudes are considered a subcategory of beliefs. Beliefs are "relationships between two things or between something and a characteristic of it . . ." (Bem, 4). An attitude is an evaluative belief that expresses a preference.
- 2 Although the Massachusetts ERA campaign concerned an amendment to the state constitution rather than the federal, we did not find this difference significant for either the issues raised or the level of resources mobilized. Despite disclaimers by proponents that the Massachusetts ERA would not affect federal issues like the draft, arguments raised by both sides were virtually indistinguishable from those used in ratification debates of the federal amendment. The high level of organized opposition in successful campaigns which defeated state ERAs in New York and New Jersey in 1975 also indicates that resource mobilization is not directly tied to the level of government for which an equal rights amendment is intended.
- 3 Despite these differences in role specialization within the contending leadership groups, the groups did not differ significantly on characteristics likely to influence our hypotheses. The groups show similar levels of campaign involvement on three separate measures—number of activities attended per week, number of days worked per week on the state ERA referendum, and number of total years involved in the ERA campaign (see Table 2).
4. Although the actual vote was 60.4 percent for the ERA and 39.6 percent against, comparable proportions in the weighted sample were 76.5 and 23.5. Clearly, some respondents inaccurately reported a favorable vote. Comparison with nine other referenda items—which were all reported accurately in the survey—suggests two principal reasons why respondents might have misrepresented their ERA vote. Some negative voters may have wanted to make a favorable impression on interviewers affiliated with a women's college. Also, "soft" opponents of the amendment may have wanted to associate with the winning side. The inclusion among voters for the ERA of those who actually voted against it should, however, have a conservative effect on the relevant hypotheses. Inflating the number of favorable voters would tend to "purify" the opposition and "contaminate" the supporters.
- 5 The original number of all respondents in the telephone survey was 439. Blacks were removed from this completed sample because of their low response rate in the study and because of their small proportion in the state population. The final weighted sample was then 416. Of these, there were 306 who had actually voted on the ERA of whom 167 were women.

## Appendix

Respondents were read the following statement: "There are many specific demands that have been made in the name of changing women's status for the better. I'm going to read you a list of demands and I'd like you to tell me which ones, if any, you think will help improve women's place in society, which ones, if any, will hurt, and which ones will neither help nor hurt women's status."

Respondents were then asked whether each of the following demands would help, hurt, or have no effect on women's status.

- \*1. Giving women equal pay for doing equal work?
- \*2. Electing more women to Congress and other high political offices?
- \*3. Allowing married women to keep their maiden names?
- 4. Providing equal funding for male and female sports in schools and colleges?
- 5. Using the term Ms. rather than Miss or Mrs.?
- 6. Allowing girls on boy's teams, like the Little League?
- \*7. Providing government support for day care centers?
- 8. Showing more competent women on TV and in the movies?
- 9. Encouraging girls to enter the professions like medicine and engineering?
- 10. Giving women the same sexual freedom as men?
- \*11. The right to abortions in the first trimester of pregnancy?
- 12. Establishing quotas for the proportion of women in executive jobs and professional schools?
- \*13. Paternity leave for fathers?

\* Items used on the state survey.

Scoring for all items is as follows: help=1; neither help nor hurt=2; hurt=3.

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# Metropolitan Function and Interdependence in the U.S. Urban System\*

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## ABSTRACT

*Central to the ecological theory of urban systems is the notion of a territorial division of labor based on differentiation and interdependence and mediated by large urban centers performing metropolitan, i.e., control and coordination, functions. The hypothesis of a positive relationship between performance of metropolitan functions and extensiveness of involvement in the web of interdependence (the latter measured by volume and variety of trade flows and the number of exchange linkages), is here tested directly for the first time. The vertical (metropolitan) dimension of functional differentiation is found to be closely associated with the extensiveness of trade interdependence, but the relationship varies by product type and city size.*

It is widely recognized that in heavily urbanized societies, characterized by well-developed transportation and communications networks, individual communities do not exist in isolation, but are involved in a larger system of functionally differentiated local populations (Duncan et al.; Hawley, a). The functional aspects of urban systems have often been studied in connection with the development of the metropolitan economy (Bean et al.; Duncan and Lieberman; Duncan et al.; Sutker) and in developing typologies of local functional specializations (Duncan and Reiss; Kass, a). But the structural differentiation of populations as they mediate intercommunity trade has been the subject of only limited investigation—even though evidence suggests that hierarchical position is a useful explanatory tool with respect to various social, economic, and demographic characteristics

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of communities (Galle; Stern and Galle; Winsborough). Moreover, even when of primary concern, patterns of metropolitan interdependence have not been measured directly in terms of sustenance exchange flows. Rather they have been assumed or inferred indirectly through the examination of functional correlates (e.g., Vance and Sutker) or operationalized in terms of size and/or distance from a larger city (e.g., Bogue, a; Butler and Fuguitt; Lincoln and Friedland; Stoeckel and Beegle; Tarver).

Although interarea trade linkages have received some attention in analyses of hierarchical differentiation (Duncan and Lieberman; Duncan et al.; Galle and Stern; Winsborough et al.), this research has been hampered by sparse data and, therefore, has often been more suggestive than definitive. Consequently, "[d]ata inadequacies, conceptual vagueness, and analytical difficulties have thus far permitted us to make only fragmentary descriptions and even fewer explanations of interurban interaction patterns" (Bourne and Simmons, 260).

The problem posed in this study stems from these propositions. The ecological theory of metropolitan systems postulates the existence of a territorial division of labor, dominated by large urban centers, that is based on interdependence among functionally differentiated activities. At the core of this theory is the hypothesis of a close positive association between the degree to which a community is engaged in key administrative and coordinative activities and its centrality in the web of trade interdependencies (Hawley, a; Vance and Sutker). However, the latter proposition has not heretofore been adequately tested because no direct measure of the functional interdependence dimension of the division of labor has been available (Gibbs and Poston).

Taking advantage of a little known (and even less frequently used) data source, the present research evaluates an assumption central to ecological models of urban systems, in general, and to the concept of metropolitan dominance in particular. We want to test the hypothesis that the position of communities in the *hierarchy of urban interdependence*, as measured by exchange flows and linkages, varies positively with the *vertical functional differentiation* of communities, as measured by degree of involvement in management and coordinative functions.<sup>1</sup> This is accomplished by combining data on the range, scope, and volume of interarea exchanges of both unprocessed and manufactured commodities with information on the economic and organizational characteristics of communities in order to examine the extent to which populations which rank high on a functional basis are also dominant in mediating extensive trade with other areas.

## Functional Differentiation and the Structure of Urban Systems

In this section we present the theoretical rationale underlying this research, beginning with a discussion of the general conceptual framework, followed by a statement of the specific substantive issues involved.

### GENERAL CONCEPTUAL FRAMEWORK

Urban systems are comprised of interdependent communities with complementary functions which originate in, and are tied together by, a web of trade relationships. In addition to maintenance, or nonbasic functions which all cities<sup>2</sup> provide their local populations, communities specialize, in varying degree, in certain sustenance activities that are performed not only for their local populations but for other communities as well (Kass). As a result of differentiation among areas in these export or basic functions, populations vary in their patterns of trade with other communities (Duncan et al.). The position of communities within a territorial division of labor reflects community involvement in the network of interarea sustenance exchange and the extent to which each mediates trade among other populations (cf. Duncan et al., 46). The mechanisms underlying the differentiation of communities in mediating interarea trade are differences among areas in the functional significance of their sustenance activities for the greater national economy (Bogue, a; Gras; McKenzie). Coordinative activities (e.g., banking/finance, wholesaling, communication, and transportation) function primarily to control or determine the conditions under which the exchange of goods and services occur. Similarly, the influence of a population is not necessarily limited to a contiguous hinterland, because communities are linked through their relationships with others in a larger ecological field that may be regional, national, or international in scope (Pappenfort).

### FUNCTIONAL DIFFERENTIATION

Functional differentiation of urban communities may be conceived in terms of two dimensions which, although related, can be treated as distinct on both theoretical and empirical levels. The first, *specialization*, implies community differences in the type and degree of activity across the full range of functions in which communities are involved (e.g., Kass, a). The second, here denoted *metropolitan function* (or metropolitanism), reflects the variation between communities in absolute levels of involvement in the higher-order management and coordinative activities that govern the conditions under which exchange occurs (e.g., Vance and Sutker).<sup>3</sup>

1 *Specialization*. Specific functions characterizing individual cities have typi-

cally been distinguished according to the configuration of labor inputs to speciality industries. The result has been a sequence of functional typologies (Alexandersson; Duncan and Reiss; Hadden and Borgatta; Harris; Kass, a; Nelson), that have often relied on rather arbitrary classification criteria (see Kass' [a] comment). Specialization of cities, whether "cast as discrete taxonomies" or as a continuous variable, represents the horizontal dimension of urban differentiation (Wanner, 519, 520). Classification of communities by functional type can be accomplished without taking into account either exchange linkages or the role of cities in the hierarchy of control and coordination. Partly for this reason, by far the most common use of functional classification schemes has been for description, and the most common criticism of them has been their failure either to explain or predict (Duncan et al.; Hadden and Borgatta; Wanner). (As always, there are exceptions, e.g., see Kass, b; Stern and Galle.) Hence we turn to a consideration of the second (metropolitan function) dimension.

**2. Metropolitan function.** Two approaches can be identified in analyses of metropolitan dominance, both of which reflect the *vertical* dimension of urban differentiation. The first, prominent in the works of Bogue (a), Gras, and McKenzie, emphasizes the organizational hegemony exercised by a metropolis over its *adjacent* hinterlands. In other words, the focus is on the *internal* organization of an extended metropolitan community: "The metropolitan community thus appears to be an organization of many mutually interdependent and interfunkioning subcommunities . . ." (Bogue, a, 59).

The second emphasis in this tradition shifts the focus from *intra*-metropolitan to *inter*metropolitan organization. That is, "On the one hand a city mediates in the realtionships of its subdominants with the rest of the system or subsystems. On the other hand, some cities seem . . . to mediate in the relationship between the major subcomponents of the system as a whole" (Winsborough et al., 9). Metropolitan populations occupy the apex of an urban hierarchy because the coordinative and management activities performed within these communities enable mediation and thus control over the flow of capital, goods, and services among subdominant areas of settlement (Smith and Weller).

However, metropolises differ in the niche occupied in the intermetropolitan hierarchy. Some may be subdominants controlling and serving primarily their immediate hinterlands. Others which have benefitted from the advantage of location at the confluence of major transporation routes, (especially port status) and which are heavily engaged in higher-order financial and commercial functions, will wield greater influence over wider areas. Indeed, some of the latter mediate sustenance exchange on a society-wide basis thereby integrating lower-strata communities into a functional whole (Duncan et al.; Vance and Sutker). Through metropolitan-based institutions, entire areas are integrated into a territorial division of labor.

## INTERDEPENDENCE AND EXCHANGE FLOWS

Each of the perspectives reviewed above recognizes that the system of cities rests on a dual foundation of intercity functional differentiation and a concomitant network of trade linkages. Although the outline of the urban hierarchy in the United States and the functional bases of dominance have been rather clearly established, relatively little research has gone beyond the question of functional differentiation to consider the equally (or more) crucial question of *functional interdependence* as indexed by the web of trade relationships.

Certain related types of intercommunity relationships have, of course, been examined. Studies of communications linkages (newspaper circulation) between cities have discovered an asymmetric system of relationships that varies with city size (Winsborough et al.). Financial transaction flows have been used to distinguish cities at various positions in the metropolitan hierarchy (Duncan et al.). Other studies of financial activities have not used data on flows or trade linkages per se, but instead have relied on surrogate measures based on information about loans and correspondent banking arrangements that indicate the direction of financial flows (Duncan and Lieberman; Lieberman; Lieberman and Schwirian). One recent investigation replicating the work of Vance and Sutker does provide information on commodity shipments, but for only three cities (Galle and Stern).

The lack of research dealing with the relationship between performance of metropolitan functions and patterns of exchange is particularly unfortunate. In our view, measurement of the degree of involvement in higher-order (financial, commercial, administrative, distributive) functions may accurately rank cities according to metropolitanism, but it is through the exchange flows themselves that dominance is ultimately established.

One reasonable argument is that mediation of system sustenance flows (Duncan et al.) and performance of coordinative/integrative functions (Vance and Sutker), along with population size and spatial location, are all part and parcel of the meaning of dominance. Another is that the degree to which a community is engaged in higher-order metropolitan functions is, along with size and location, a determinant of the ability of urban-based institutions to mediate trade (Hawley, c). Central to either interpretation is the assumption of a positive relationship between position along the vertical dimension of metropolitan function and the degree of interdependence with other units in the system.

## HYPOTHESES

It will be useful to summarize the foregoing in terms of specific hypotheses:



1. The hypothesis of primary concern here should by now be clear: communities with higher levels of involvement in metropolitan activities (i.e., control and coordination functions) are expected to exhibit more extensive linkages of interdependence than communities less engaged in higher-order activities. By extensiveness of interdependence we mean *volume* and *variety* of exchange flows as well as the *number of linkages* through which exchanges take place.
2. Based on previous research (Bogue, a; Hawley, c; Vance and Sutker), it is anticipated that both population size and locational advantages will be positively associated with both metropolitan function and the extensiveness of trade linkages. Nevertheless, the relationships between the variables mentioned in the first hypothesis are expected to persist net of controls for size and location.

The purpose thus far has been to place the present research in the framework of urban ecology, first by sketching the outlines of the concept of functional differentiation as related to dominance, and then by noting specific theoretical points of departure in this research tradition. One of these points of departure, horizontal specialization, will not be pursued further both because it is already the subject of an extensive literature and because it is not of direct relevance to this investigation. By contrast, the notion of the performance of metropolitan functions (the vertical dimension of functional differentiation), leads directly to consideration of dominance at both an intra- and intermural level. Of particular interest is the latter since it provides a conceptual basis for study of urban functional interdependence which, although crucial to the entire theory of urban systems, has heretofore received virtually no detailed empirical attention. (Indeed, it could be argued that the concept of dominance has no meaning apart from the exchange flows that metropolitan areas are assumed to mediate and control.) In the following sections are presented a review of analytic strategy as well as findings which emerge from the hypotheses above.

## Data and Methods

### UNITS OF ANALYSIS

The units of analysis are all 171 Business Economic Areas (BEAs) comprising the contiguous United States *circa* 1970. Developed from county level data by the Bureau of Economic Analysis to enable the projection of regional economic activity, these functional economic areas (also called "daily Urban Systems" [Berry and Kasarda, 283]) were marked out to approximate,

in operational terms, the ecological conceptualization of "community" (Bogue, b; Hawley, a). Each BEA was constructed to consist of a city-oriented area "with its hinterland in which there is a definite interaction of the various parts with the center and in which the establishments, both business and households, are functionally related" (U.S. Department of Commerce, 22). Through the use of commuting patterns as well as other evidence of the linkage of center with periphery, these economic areas were developed so that "each combines its labor market and labor supply—the place of work and place of residence of the labor force. There is, therefore, a minimum of commuting across economic area boundaries." (U.S. Department of Commerce, 22). These units divide the United States into mutually exclusive community areas. Although based on 1960 commuting patterns, the relatively high functional integrity of the BEAs has recently been reaffirmed (Berry and Kasarda, 282–304).<sup>4</sup> Thus, BEAs or Daily Urban Systems provide a useful approximation to the ideal of "unit character."

#### CONCEPTUALIZATION AND MEASUREMENT OF INTERDEPENDENCE

The degree of interdependence characterizing a community in the urban hierarchy reflects the importance of institutions within its borders in mediating interarea trade relationships among less dominant populations; hence the need for information on the actual sustenance linkages between areas for direct operationalization. Thus we exploit information in inter-BEA commodity flows during 1972, data compiled by the U.S. Department of Transportation in developing a national transportation policy. Consisting of the gross tonnage of nineteen groups of bulk and manufactured commodities<sup>5</sup> moving between all 171 BEAs by origin and destination, these data permit the most complete coverage of interarea commodity transactions available to date (Kaufman and Faucett; U.S. Department of Transportation).<sup>6</sup>

There are several potential limitations of the commodity flow data as a measure of interdependence. First, *intra*-BEA truck movements are not included for bulk commodities. This underestimates the amount of trade *within* each community but has only limited ramifications for the relative size or range of *inter*-BEA flows on which we focus. Second, where "two or more segments of a single multimodal shipment are captured in the bulk commodity flow data base, they are captured as separate, not integrated, flows" (U.S. Department of Transportation, 12). Thus, the ultimate origin and destination of each bulk commodity shipment are not necessarily known. Third, the census Commodity Transportation Survey of manufactured goods "only includes shipments from U.S. manufacturing facilities, excluding imports and shipments from other non-production facilities such as warehouses, agricultural assemblers, merchandisers, or mineral pro-

cessing plants" (U.S. Department of Transportation, 12). This also points to a general characteristic of the *bulk* portion of these data, *viz.*, both imports and exports from international trade, to the extent that these goods are included in the data base, are allocated to their port of entry/exit, which may overestimate the volume of commodity shipments and receipts in these BEAs. However, since this allocation of trade to ports of entry/exit accurately reflects their role as gateway centers involved in trans-shipment and break-in-bulk activity, and since port status is explicitly included in the present analysis (for reasons discussed below), any biases engendered by this aspect of these data are considered minimal.

Three dimensions of interarea trade are examined for each BEA. Since communities at the apex of the hierarchy are expected to exchange both a *larger variety* and *volume* of sustenance with *more places* than are communities lower in the hierarchy, the volume of trade (in logged short tons), the number of linkages, *i.e.*, trading partners (BEAs), and the number of product groupings are combined into a summary index of total trade activity through summation of standard scores.<sup>7</sup> This procedure generates, for each BEA, a summary rank-score of high reliability ( $\Omega = .94$ )<sup>8</sup> which indicates the position of the community relative to all other BEAs in the country. Communities with higher scores on this index evidence more extensive interarea trade linkages than do BEAs with lower scores.

It might be argued that the gain in parsimony achieved by use of the summary index of interdependence is more than offset by loss of analytic detail about the complex network of trade flows. Our response would be that an adequate operationalization of the concept of functional interdependence must take into account at least the three dimensions (number of trade linkages, variety of exchange, and number of linkages) described above. On the other hand, although we will devote most attention to the summary index, there is no reason not to add as much specificity as possible to the analysis. Therefore, each of the three components of the index *is also examined separately* for both receipts and shipments within both the bulk and manufactured commodity categories. (Since all BEAs received the same *number* of product groups, however, only two components can be analyzed for manufactured commodity receipts.)<sup>9</sup>

Implicit in the measurement procedure described above is an important assumption, *viz.*, that each community can be given an unambiguous rank along the single dimension of the extent of interarea trade. This issue is raised by empirical investigations employing multiple criteria in which a weak or partial hierarchical ordering of communities is inferred instead of the strong ordering required above (Duncan et al., 46–7). Although the problem of unidimensional versus multidimensional measures of hierarchical position cannot be adjudicated at this time, for all practical purposes the issue is moot, since to evaluate more precisely the extent to which the pattern of external community trade varies by commodity type, the sum-

mary index is broken into its components for more detailed investigation in portions of the analysis.

#### CONCEPTUALIZATION AND MEASUREMENT OF METROPOLITAN FUNCTION

There are several ways of operationalizing the degree of community involvement in metropolitan functions (cf. Duncan, et al.; Vance and Sutker; Wanner). Our procedure is based primarily on the indicator developed by Vance and Sutker to rank southern cities, and later used by Galle and Stern in their replication. The functions of metropolitan areas of concern are those that can theoretically be expected to form a basis for the exercise of dominance. In other words, it is those activities which can be characterized as key functions—those which enable an ecological unit to mediate and channel exchange flows (Hawley, b, 332; c, 220)—that are expected to relate positively to the measure of interdependence described above.

There are at least four general classes of functions which are key in this sense (Hawley, b; Vance and Sutker). These are listed immediately below along with specific indicators of each:

1. *Industrial Development* is the major source of population concentration and accumulation of wealth. This component is measured by *retail sales* and *value added by manufacturing* which reflect the "gross underpinnings a city has for building its market and amassing wealth" (Vance and Sutker, 127).
2. *Organization of the Market* refers to the extent to which metropolitan centers supply specialized services and products not produced by lower-order cities. The first measure in this category is the *dollar value of wholesale sales*, long considered an important indicator of metropolitan status. The second is *gross receipts of selected service establishments*, a measure of numerous specialized services supplied to hinterland populations (Vance and Sutker).
3. *Financial Services*, in the sense of both banking activity and corporate control, is the third key function. While it would not be unreasonable to classify such functions under either (or both) of the first two categories, the integrative significance of this activity warrants separate attention (though as might be expected, all the measures of metropolitan function described in this section are highly intercorrelated). Operationalization is first in terms of *banking and savings association deposits* reflecting the wealth of a community and also its degree of involvement in this coordinative function. Also indicating the degree of organizational control centered in a community (Lincoln, a) is the *number of FORTUNE 1000 manufacturing corporations with home offices in the BEA in 1969* (Fortune, a, b).
4. *Transportation and Communications* facilities may be the most crucial feature in the development of metropolitan dominance. To this point we have

relied heavily on Vance and Sutker's measurement procedures. However, these authors failed to include a measure of the transportation and communications dimension that is clearly requisite to the expansion of urban organization and integration of the system (Hawley, c). Thus, we have added to the original Vance and Sutker index the *number of transportation and communications establishments with fifty or more employees (in 1967)* thereby incorporating involvement in distributive activity (U.S. Bureau of the Census, a).

Factor analysis of these variables confirms that each taps the same underlying dimension, and item-analysis indicates that the index of metropolitan function is characterized by high reliability ( $\Omega = .98$ ). Thus each of the variables was converted to standard scores and summed to form an index analogous to the Vance and Sutker measure.<sup>10</sup> Communities with high scores on this index are more involved in metropolitan functions than communities with lower scores.

Several additional comments are necessary about the index of metropolitan function. First, the use of absolute values almost insures, in advance, that the summary index will be strongly related to community population size. This was examined in the original work by Vance and Sutker in graphic fashion, and recalculation from their tables shows that while the correlation between the population size of a city and its metropolitan function is high ( $r^2 = .72$ ), it is not unity. This suggests that size per se is not sufficient to describe the relative position of a city in the metropolitan system, which is consistent with an earlier statement by Gras that the

. . . mere agglomeration of individuals . . . does not constitute a metropolis . . . What counts most is commercial dominance over a wide area, a dominance which such large centers as Pittsburgh and Detroit do not possess, for they are manufacturing towns . . . but relatively insignificant as marketing centers for the adjoining district (184).

As Vance and Sutker point out, "while all metropolises are large cities, not all large cities are metropolises. Population size is a concomitant; function is the keynote" (115). Thus, while size might be reasonably taken as a surrogate for the hierarchical aspect of urban systems (Berry and Kasarda, 327-34; Duncan et al., 69; Lincoln, b, 917), it is functional relationships and degree of interdependence that are of theoretical import.

It is important to note that the decision to employ absolute rather than relative (per capita) measures stems from conceptual, not statistical, concerns (Fuguitt and Lieberman; Kasarda and Nolan). Wanner, in commenting on the substantive issues of principal interest here, argues that when interest is in the "'urban hierarchy' in the sense of a ranking in terms of importance or dominance from the point of view of the system," what is required are measures of "extensiveness of activity." Measures of *exten-*

*siveness* must, in turn, be based on absolute values, rather than on relative values which indicate *intensiveness* of activity (525). Hence, in this case, absolute measures are more appropriate since the theoretical scheme centers on the metropolitan system and on the extensiveness of communities' influence over intercommunity exchange.

#### CONTROLS

This is not to say, however, that population size is extraneous in the present context. Indeed, the indexes of both metropolitan function and interdependence are based on absolute levels of their component variables; and, obviously, larger communities will exhibit higher levels of both trade and associated economic activity than smaller communities *ceteris paribus*, simply as a result of differences in population size. Of greater substantive interest is the fact that large local populations, both as labor resource and market, create economies of scale that may stimulate a community's economic development and enhance its position in the metropolitan hierarchy (Frisbie). Thus, population size must be controlled when the hypothesis concerning the relationship between metropolitan function and intercommunity exchange linkages is tested.

Finally, historical advantages of location are also likely important for the range and volume of trade interconnections. Of particular significance is whether the ecological unit of analysis has within its boundaries major port facilities, since BEAs with such facilities are primary import-export gateways linking the U.S. system of cities to international commodity flows. Hence, port status, operationalized as a dummy variable coded 1 for presence of port facilities handling more than 10 million tons annually as of 1976 according to *The World Almanac* (Newspaper Enterprise Association, 134), is included as a control along with population size. (See note 12 for further comments on the application of these controls.)

#### ANALYTIC STRATEGY: TWO STAGES

A basic assumption of this research is that metropolitan communities constitute a system—"that what cities are like depends at least in part on what cities do (their functions), and that the functions of cities are in some measure a reflection of intercommunity relationships" (Duncan et al., 46). Indeed, the hypothesis being examined concerns the form of the relationship between function (conceptualized in terms of "metropolitanism") and interdependence in the network of exchange in light of predictions derived from ecological models of dominance. Since this relationship is considered a property of the urban system per se, the present analysis examines all interacting BEAs in the contiguous United States, rather than a subsample

selected on the basis of specific characteristics (E.g., size, cf. Duncan et al.; region, cf. Vance and Sutker).

Including the entire range of U.S. BEAs in the present investigation is obviously appropriate given both the systemic nature of urban interaction and evidence that the urban system in the U.S. is national in scope (Duncan et al., 69, 118–23). However, the obviousness of this decision does not imply that it is trivial. Regarding size and/or regional location of the communities examined, it is possible that conclusions pertaining to the system as a whole may not be applicable to segments of the system. The implication here is that conclusions of prior studies of metropolitan function, based in the main on the empirical examination of portions of the greater U.S. urban system such as larger cities or those within a single region, may vary from conclusions based on an examination of the entire system. The possibility that the relationship between trade and metropolitan status varies according to regional location is beyond the scope of the present research,<sup>11</sup> but the existence of interaction involving population size is explored in the present investigation as an elaboration on the hypothesis of major interest.

Specifically, then, data analysis is conducted in two stages, dealing first with the relationship between extensiveness of trade linkages and metropolitanism for the U.S. urban system as a whole and second with the relationship among large (populations greater than 2 million) and small BEAs separately. Although grouped into size categories on the basis of an arbitrary cutpoint, the 22 large BEAs (which together contain 50.6 percent of the total population of all 171 areas) seem to correspond roughly to what others have considered to be areas with metropolitan characteristics. In both stages of the analysis, the primary concern is the correlation between the extensiveness of trade interdependence characterizing a community and its absolute involvement in financial–commercial (metropolitan) functions, both in bivariate form and with controls. The bivariate correlation reflects the extent to which trade and metropolitan activity hang together, in general, while the partial coefficients identify that portion of the relationship which is independent of both size<sup>12</sup> and port status.<sup>13</sup>

## Findings

### STAGE I: ANALYSIS OF THE ENTIRE NATIONAL SYSTEM OF CITIES

For descriptive purposes, the detailed trade linkages between all 171 BEAs were examined, and these data (not shown) suggest an image of the urban system both consistent with the conclusions of other studies (e.g., Duncan et al.) and relevant to the conceptual issues discussed above. First, some

BEAs are specialized in the production or processing of a limited range of product groupings. For example, Meridian, Mississippi is the source of a large amount of lumber and furniture but does not have a high level of trade in any other commodity. A like situation obtains in a number of areas, including Greenville, South Carolina and Chattanooga, Tennessee for shipping textiles and apparel; Redding, California and Eugene, Oregon for lumber and furniture; Davenport–Rock Island–Moline, Iowa–Illinois for non-electrical machinery, and Appleton–Oshkosh, Wisconsin for paper and allied products, among many others. These BEAs are not major commercial or industrial centers involved in extensive exchange in a wide range of goods, but are instead highly specialized in a few products exported to the larger urban system.

In contrast, other BEAs evidence trade (both shipping and receiving) in numerous products, suggesting extensive involvement in the network of interdependencies since they constitute general points of trans-shipment for commodities between origin and destination. For example, Chicago is among the five largest receivers for 13 of the 19 different commodity groups examined, and is among the five largest shippers for 8. Likewise, the New York City BEA exhibits at least the fifth largest volume of receipts in 13 commodities and is among the largest shippers of 4. Similar patterns are observed for Philadelphia, Detroit, and several other areas.

These two trading patterns, which may be regarded as extremes on a continuum of extra-local exchange, correspond well with the basic outline of the territorial division of labor suggested by ecological models (Duncan et al.; Galle and Stern; Wanner). Specifically, the former pattern (a high volume of trade, primarily shipping, in a *few* products) characterizes communities with low levels of involvement in metropolitan functions, while the latter (a high volume of shipments *and* receipts in many products) appears characteristic of areas at the apex of the vertical dimension of metropolitanism.

To illustrate the association between degree of community interdependence and involvement in metropolitan functions, Table 1 presents the 10 BEAs with the highest and lowest scores on the summary index of these two constructs. The table clearly reveals the similarity between the distribution of areas on both indexes as well as the resemblance to the pattern of intercommunity differentiation suggested by prior research. New York, Chicago, and other large BEAs found in prior analyses (e.g., Duncan et al., 271) to be near the top of the urban hierarchy have high scores on both indexes, while smaller BEAs which score low on the index of metropolitanism exhibit the lowest values for extensiveness of trade interdependencies. The relationship is apparently strong, judging from the fact that 7 of the 10 BEAs with highest positions in the trade hierarchy, as well as 7 of the 10 lowest, appear in similar positions on the index of metropolitan function.<sup>14</sup> This is confirmed by a product-moment correlation of .83 ( $p < .001$ ) be-



**Table 1.** EXTREME SCORES ON THE INDEX OF METROPOLITAN FUNCTION AND INDEX OF TRADE INTERDEPENDENCE FOR ALL U.S. BEAS, 1970

High		Low	
BEA	Score	BEA	Score
<u>Metropolitan Function Index*</u>			
New York, N.Y.	3.88	Aberdeen, S.D.	-1.69
Chicago, Ill.	2.99	San Angelo, Tx.	-1.63
Los Angeles-Long Beach, Cal.	2.87	Scottsbluff, Neb.	-1.63
Philadelphia, Pa.-N.J.	2.55	Bismark, N.D.	-1.60
Boston, Mass.	2.42	Eureka, Cal.	-1.55
San Francisco-Oakland, Cal.	2.29	Minot, N.D.	-1.52
Detroit, Mich.	2.23	Grand Forks, N.D.	-1.30
Cleveland, Ohio	2.16	Rapid City, S.D.	-1.24
Pittsburgh, Pa.	1.85	Redding, Cal.	-1.21
St. Louis, Mo.-Ill.	1.81	Grand Junction, Col.	-1.18
<u>Index of Interdependence*</u>			
Chicago, Ill.	2.63	Eureka, Cal.	-2.86
Houston, Tx.	2.24	Aberdeen, S.D.	-2.22
St. Louis, Mo.-Ill.	2.17	San Angelo, Tx.	-2.19
Cleveland, Ohio	2.15	Redding, Cal.	-1.75
Philadelphia, Pa.-N.J.	2.05	Rapid City, S.D.	-1.70
New Orleans, La.	1.99	Reno, Nev.	-1.59
Pittsburgh, Pa.	1.96	Bismark, N.D.	-1.56
Los Angeles-Long Beach, Cal.	1.89	Scottsbluff, Neb.	-1.52
Birmingham, Ala.	1.72	Billings, Mont.	-1.46
New York, N.Y.	1.68	Cheyenne, Wyo.	-1.46

\*Indexes are in standard form.

tween these variables, as shown in Table 2.<sup>15</sup> Thus, the zero-order relationships for the urban system taken as a whole support the hypothesis of primary interest. That is, the greater the involvement of a community in metropolitan functions, the more extensive its interdependence with other units in the system as indexed by the range, scope and volume of its external trade linkages. In accordance with the second hypothesis, population size and port status are strongly correlated with both trade and function (Table 2). However, when the former variables are controlled, the association between the overall index of interdependence and metropolitan function is erased, primarily due to the influence of population size alone. This is shown in the first row of Table 3.<sup>16</sup> This finding clearly contradicts the pattern of association hypothesized, and suggests, that, for the system as a whole, the relationship between the position of a community in the web of interarea sustenance exchange and its performance of metropolitan functions may be explained by the joint dependence of these variables on the factors of size (primarily) and the locational advantage of port status (to a lesser extent).

However, data in Table 3 also help us account, at least partially, for the failure of the broad hypothesis; for we find an interesting and impor-

**Table 2.** DESCRIPTIVE STATISTICS AND PRODUCT-MOMENT CORRELATION MATRIX OF VARIABLES FOR ALL U.S. BEAS, 1970

	Descriptive Statistics			
	Interdependence*	Metropolitan Function*	Population Size	Port Status
$\bar{X}$	.000	.000	13,460	.216
S	1.000	1.000	.916	.413
Md	-.093	-.244	13,313	---

Correlation Matrix**				
	(1)	(2)	(3)	(4)
(1) Interdependence		.83	.85	.58
(2) Metropolitan function			.98	.58
(3) Population size				.58
(4) Port status				

\*Indexes in standard form.

\*\*For all correlations,  $p < .01$

tant pattern of relationships between particular types of interarea trade and the index of metropolitan function. These coefficients indicate that, net of population size and port status, areas with greater involvement in metropolitan functions are characterized by a greater range of extra-local trade in manufactured commodities, but a lesser range of trade in bulk commodities. BEAs with higher scores on the index of metropolitan function receive bulk commodities from a larger number of other areas, but ship a smaller quantity and a more limited range of bulk goods than do other BEAs. Conversely, these communities receive a larger quantity of manufactured goods, and ship a greater number of manufactured product groupings in larger quantities to more areas. When considered together in the summary index of interdependence, the opposite net relationships for these two types of goods balance, so eliminating an overall association. In addition to helping to explain the apparent failure of the major hypothesis, the divergent relationships of trade in bulk and manufactured commodities with the index of metropolitan function have other implications which are discussed following the presentation of findings from an analysis of the pattern of association within size groups.

**STAGE II: ANALYSIS WITHIN SIZE CATEGORIES**

The second stage of the research involves the analysis of small (less than 2 million in population) and large BEAs considered separately. Although not

**Table 3.** BIVARIATE AND PARTIAL CORRELATIONS OF THE OVERALL INDEX OF INTERDEPENDENCE AND ITS SEPARATE COMPONENTS WITH METROPOLITAN FUNCTION FOR ALL U.S. BEAS, 1970

	Bivariate Correlation	Controlling		
		Population Size	Port	Population and Port
Index of Interdependence	.83***	.02	.75***	-.001
<u>Components of the Index of Interdependence</u>				
<u>Bulk Commodities</u>	.65***	-.12	.47***	-.15*
<u>Receipts</u>	.74***	-.01	.60***	-.03
BEAs	.81***	.20**	.73***	.19***
Products	.57***	-.09	.39***	-.12
Tons	.69***	-.09	.54***	-.12
<u>Shipments</u>	.45***	-.18**	.23***	-.21**
BEAs	.40***	-.02	.20*	-.04
Products	.38***	-.23***	.17**	-.26***
Tons	.43***	-.22***	.22**	-.26***
<u>Manufactured Commodities</u>	.88***	.24***	.85***	.25***
<u>Receipts</u>	.90***	.01	.87***	.02
BEAs	.82***	-.10	.78***	-.08
Tons	.94***	.16***	.90***	.17*
<u>Shipments</u>	.80***	.29***	.74***	.30***
BEAs	.60***	.12	.57***	.13*
Products	.81***	.33***	.74***	.33***
Tons	.83***	.36***	.76***	.36***

\*p ≤ .05  
 \*\*p ≤ .01  
 \*\*\*p ≤ .001

precisely coextensive with the universe of areas most likely to manifest distinctively metropolitan characteristics, the 22 large BEAs seem to correspond roughly to the analytical units employed in many analyses of the metropolitan hierarchy.

Descriptive statistics and bivariate correlations of the overall degree of interdependence, metropolitan function, population size and the presence of port facilities are presented by size category in Table 4. As expected from the relationships between these variables observed above for the system as a whole, large BEAs are more likely to be ports and are characterized both by greater involvement in metropolitan functions and higher levels of interdependence than are small BEAs. Although levels of statistical significance are partially a function of the number of cases, it is interesting to note that the magnitude of the association between trade interdependencies and metropolitanism is almost twice as high among the smaller BEAs as among larger ones. With these qualifications, the zero-order corre-

**Table 4.** DESCRIPTIVE STATISTICS AND PRODUCT-MOMENT CORRELATION MATRIX OF VARIABLES FOR SMALL AND LARGE BEAS SEPARATELY, 1970

Variables	Descriptive Statistics	
	Small BEAs <sup>†</sup>	Large BEAs
Interdependence		
$\bar{X}$	-.209	1.416
S	.852	.751
Md	-.207	1.443
Metropolitan function		
$\bar{X}$	-.281	1.903
S	.678	.710
Md	-.410	1.682
Population size		
$\bar{X}$	13.210	15.153
S	.652	.588
Md	13.143	14.903
Port status		
$\bar{X}$	.134	.773
S	.342	.429

Intercorrelations				
(large BEAs [N = 22] above diagonal, small BEAs [N = 149] below diagonal)				
	(1)	(2)	(3)	(4)
(1) Interdependence	--	.42*	.29	.32
(2) Metropolitan function	.80***	--	.97***	.35
(3) Population size	.83***	.95***	--	.35
(4) Port status	.44***	.34***	.35***	--

\*p ≤ .05  
\*\*\*p ≤ .001

<sup>†</sup>Small BEAs are less than two million in size.

lations suggest the same conclusion as that noted above for the system as a whole, viz., among both small and large BEAs, communities which rank high in metropolitan functions have a more extensive network of trade linkages.

However, when the variables of population size and port status are controlled within each group, as in Table 5, striking differences between

**Table 5.** BIVARIATE CORRELATION AND PARTIAL CORRELATIONS OF THE OVERALL INDEX OF INTERDEPENDENCE AND ITS SEPARATE COMPONENTS WITH METROPOLITAN FUNCTION FOR SMALL AND LARGE BEAS SEPARATELY, 1970

Variables	Small BEAs (N = 149)		Large BEAs (N = 22)	
	Bivariate Correlation	Controlling Population & Port	Bivariate Correlation	Controlling Population & Port
Index of Interdependence	.80***	.02	.42*	.60**
<u>Components of the Index of Interdependence</u>				
<u>Bulk Commodities</u>	.55***	-.21**	.26	.41*
<u>Receipts</u>	.62***	-.05	.48*	.50*
BEAs	.70***	.11	.48*	.56*
Products	.49***	-.10	.38*	.38*
Tons	.60***	-.11	.43*	.39*
<u>Shipments</u>	.35***	-.27***	.10	.34
BEAs	.26***	-.14*	.16	.46*
Products	.31***	-.27***	.06	.20
Tons	.36***	-.28***	.02	.25
<u>Manufactured Commodities</u>	.89***	.39***	.63***	.82***
<u>Receipts</u>	.89***	.18*	.88***	.55**
BEAs	.84***	.09	.59**	.34
Tons	.91***	.26***	.87***	.52**
<u>Shipments</u>	.80***	.37***	.49**	.80***
BEAs	.65***	.22**	.42*	.74***
Products	.80***	.39***	.43*	.79***
Tons	.79***	.39***	.54*	.80***

\*p ≤ .05

\*\*p ≤ .01

\*\*\*p ≤ .001

large and small BEAs are apparent in the relationship between interdependence and metropolitan function. In brief, small BEAs exhibit a net pattern of covariation between these constructs nearly identical to that for the system as a whole. That is, among smaller communities, high metropolitan rank, net of population size and port status, is associated with a less extensive network of trade in bulk commodities but a more extensive network of trade in manufactured commodities.

Among large BEAs, a different picture emerges from Table 5, supporting the hypothesis of major interest. After controlling for size and presence of port facilities, the greater the level of involvement of large BEAs in metropolitan functions the more extensive are their trade interdependencies. This relationship holds for *both* bulk and manufactured commodities. Moreover, the vast majority of the net relationships are statistically significant and most are stronger than the unadjusted (bivariate) correlations. These data convey an image of the structure of the upper

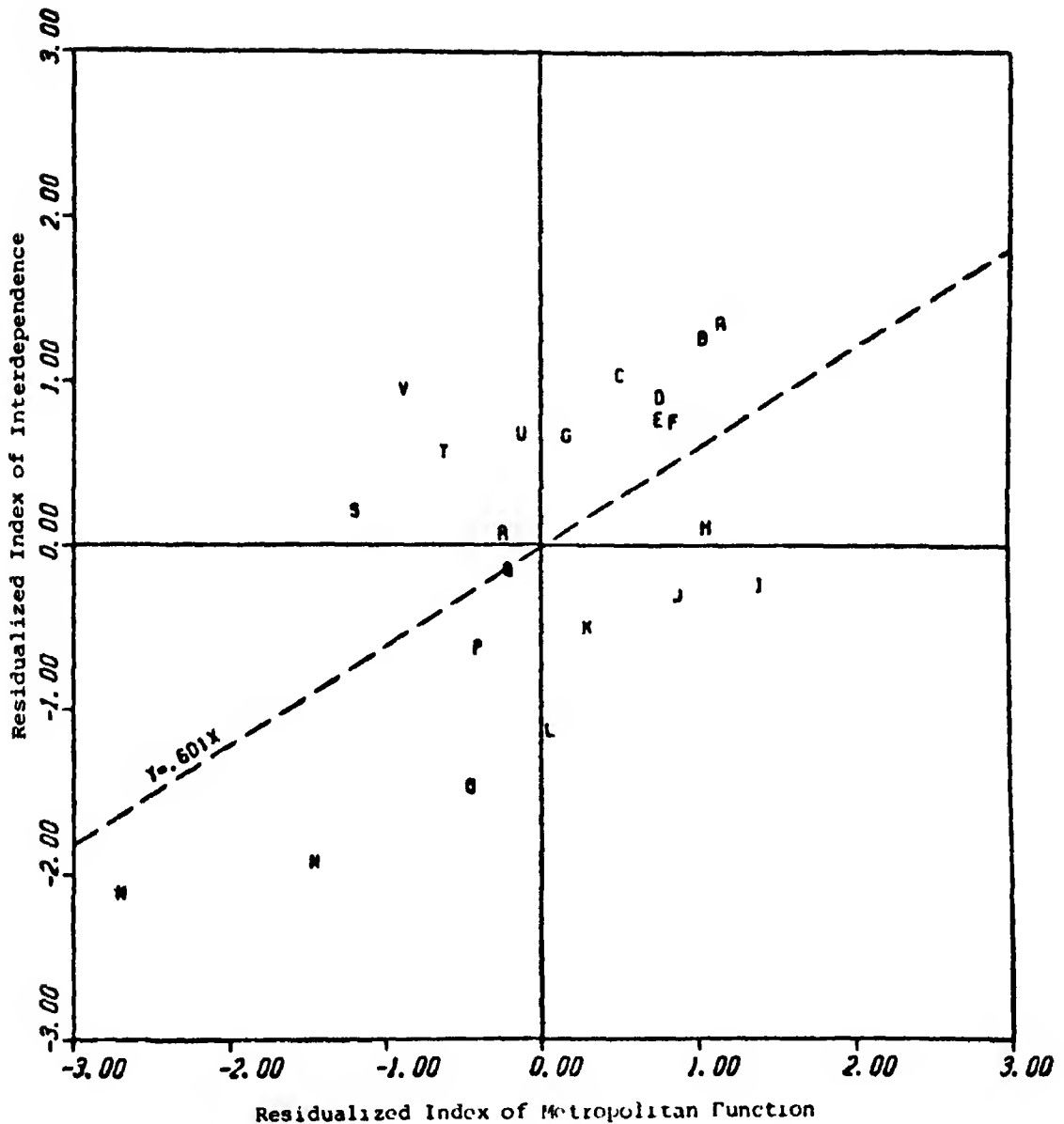
stratum of the urban system that accords with the conclusions of other studies of metropolitan dominance. That is, the position of a community in the web of interarea sustenance exchange is closely related with its pattern of internal economic activities, independent of population size (e.g., Vance and Sutker). But in addition, it is clear that there are important limitations on the applicability of this conclusion to either smaller communities or to the urban system as a whole.

### Discussion and Summary

Several specific conclusions about structural properties of urban systems seem warranted. First, large urban communities are more likely to be characterized by a high level of performance of metropolitan function and an extensive involvement in the network of extra-local exchange than their smaller counterparts. However, the specific proposition that higher levels of activity in distinctively metropolitan functions are associated with the increased ability to mediate extra-local trade, independent of population size and locational advantages, is generally applicable only to the largest (most-metropolitan) communities, not to smaller areas or to the system as a whole. Instead, smaller communities vary in their degree of trade in different commodity types according to their metropolitan rank. Controlling for size and port status, the higher the involvement in metropolitan functions among these areas, the less their trade in bulk commodities and the greater their trade in manufactured commodities.

The divergent relationships of trade in bulk and manufactured commodities with the index of metropolitan function among smaller communities indicate an important characteristic of the urban system, *viz.*, the existence of a nationwide intercommunity division of labor. When coupled with the findings that the largest communities do exhibit the predicted positive relationship between metropolitan function and extra-local trade net of size and port status, the often hypothesized nodal character of the intercommunity division of labor is clearly implied. That is, it is the largest metropolitan communities which mediate the flow of sustenance among the smaller subdominant and functionally specialized areas of settlement within their ecological fields (Duncan et al.; Gras; Vance and Sutker).

To illustrate more fully the nature of the observed relationship between interdependence and metropolitan function for the 22 large communities that are of primary concern to students of ecological dominance, Figure 1 displays a plot showing the relative position of each large BEA on both dimensions simultaneously, after residualizing (additively) for the influence of both size and port status and adjusting each axis to standard form.<sup>17</sup> As is shown in the figure, 8 of the 22 large communities (e.g., Chicago in the upper right quadrant) are characterized by higher than



**Figure 1.** Plot of the Standardized Relationship between Extensiveness of Interdependence and Metropolitan Function, both Residualized for Population Size and Port Status, among the Twenty-two Largest U S BEAs 1970

The BEAs corresponding to the letters on the plot are (A) Chicago, (B) Houston, (C) St. Louis, (D) Cleveland, (E) Minneapolis, (F) Kansas City, (G) Atlanta, (H) Dallas, (I) Milwaukee, (J) New York, (K) San Francisco, (L) Hartford, (M) Washington, D.C., (N) Miami, (O) Boston, (P) Seattle, (Q) Detroit, (R) Baltimore, (S) Los Angeles, (T) Philadelphia, (U) Pittsburgh, and (V) New Orleans.

average levels of both metropolitanism and extensiveness of interdependence while 5 areas (e.g., Washington in the lower left quadrant) have lower than average net scores on both. Five BEAs (e.g., New Orleans in the upper left quadrant) are characterized by above average levels of interdependence and below average scores on the index of metropolitan function, and four communities (e.g., Milwaukee in the lower right quadrant) follow the opposite pattern.

Although informative, the data presented in Figure 1 depict only a partial outline of the characteristics of communities at the apex of the urban system. Including horizontal functional specialization (e.g., different city "types" [Kass, a]) would necessitate adding at least a third dimension to the graph. Perhaps as a result of the limited nature of the linkage data (see note 14), the position of New York on the index of trade interdependence is below the average for the 22 large BEAs.<sup>18</sup> Nonetheless, Figure 1 provides, independently of population size and port status, an insight into the relative involvement of the largest U.S. communities in the metropolitan activities and regional relationships which integrate diverse local populations into a coherent urban system.

In summary, this study has examined a basic assumption of ecological models of urban systems, the relationship between community involvement in metropolitan functions and position in the web of interurban sustenance exchange. In this regard, the vertical (metropolitanism) dimension of functional differentiation was found to be closely associated with the extensiveness of the network of trade interdependencies, but the form of this relationship varies by both product type and city size. Although more complex than initially anticipated, these findings support the urban ecological perspective and point to the utility of directly examining trade flows to reflect the association between the functional aspects of communities and the properties of the broader territorial division of labor.

## Notes

1. The vertical dimension is to be distinguished from horizontal functional differentiation, as measured by specialization in certain types of industry (Kass, a). Wanner provides theoretical and empirical evidence contrasting the vertical and horizontal dimensions of functional differentiation. We develop this point in somewhat greater detail below.

2. To avoid redundancy, the term, city, is used throughout as a synonym for community, although it is recognized that the latter is an ecological construct while the former is determined by political decisions. Similarly, although obviously it is neither cities nor populations but rather city-based *organizations* which exercise influence over subordinate territory, the convenient and conventional term, metropolitan dominance, is employed throughout (cf. Lincoln, b, 927).

3. This observation has found expression in other schemes distinguishing between general and specific functions (Stern and Galle, 259-64), metropolitan specialization versus "gross levels of metropolitanism per se" (Bean et al., 32), and between "financial-commercial hierarchy" and "industry structure" (Wanner, 535), each of which suggests a distinction between the functional characteristics of communities in both horizontal and vertical terms.

4. However, due to some coalescing of the commuting patterns of residents of adjacent BEAs between 1960 and 1970, certain modifications will be necessary for 1980.

5. The bulk commodities include field crops, forestry and fisheries products, metallic ores, coal, crude petroleum, nonmetallic minerals, chemicals and allied products, and petroleum and coal products. The manufactured commodity groupings are food and kindred products, textile mill products, apparel and other fabric products, lumber and furniture, paper and allied products, primary metal products, fabricated metal products, nonelectrical machinery, electrical machinery, transportation equipment, miscellaneous manufactures, and a final group not identified by the two-digit STCC code to protect the confidentiality of information on individual shippers.

6. Bulk data were gathered from various sources by DOT, depending both on the product



type and mode of transit. Sources include: the Waybill Sample and Quarterly Commodity Statistics provided by the ICC, the Army Corps of Engineers complete Census of Water Carrier Traffic, and estimates of pipeline movements of crude oil and petroleum products by the Federal Energy Administration and the National Petroleum Council in combination with origin-destination data published by the Bureau of Mines

Information on processed or manufactured commodities was taken directly from the 1972 Commodity Transportation Survey of the Bureau of the Census (Kaufman and Faucett; U S. Department of Transportation, 9-11).

7. A formula representing this procedure is as follows:  $T = \Sigma(Z_1 + Z_2 + Z_3)$ ; where  $Z_1$  is the standard score of each variable,  $Z_1$  is the volume of manufactured commodities (in logged short tons) shipped from a community ( $i$ ), after the weight of each specific commodity shipped is converted to standard form and summed (thereby controlling for the difference between product groupings by weight);  $Z_2$  is the number of manufactured commodity groups shipped from  $i$ , and  $Z_3$  is the number of BEAs to which  $i$  ships manufactured commodities. These three variables are also computed separately for the bulk commodity shipments, manufactured commodity receipts, and bulk commodity receipts of each BEA. The summary index is a sum of 11 of these 12 Z-scores adjusted to standard form. Because each BEA received the same number of manufactured product groupings (11), this particular variable was omitted from the final summary index.

8. This index is actually comprised of two positively correlated dimensions—one for the shipment of bulk commodities and the other for trade involving shipments/receipts of manufactured goods and the receipt of bulk commodities ( $r = .49$ ). However, the index is retained in its present form because the distribution of variables across a plot of these two factors indicates that they are distributed in essentially a unidimensional manner. Also, a forced single factor solution generates a nearly identical pattern of communalities as the two-factor solution although each is slightly lower in magnitude. The high internal consistency of the summary index also supports the decision to retain its unidimensional form.

9. It is possible to disaggregate further by dealing with the trade flows on an industry by industry basis. Although this would certainly increase the degree of descriptive detail, it would not be possible to fit a tabular presentation, much less a meaningful discussion, of such an exhaustive description into a paper of appropriate journal length. Also, other analytic techniques such as blockmodels (e.g., Snyder and Kick) could have been applied in the study of the network of urban interdependencies. However, the primary purpose of testing fundamental ecological assumptions is at least as well served by the conventional methods described in this section as by more exotic techniques.

10. Preliminary analysis revealed the existence of a number of extreme values on the metropolitan functions index. Hence, each of the variables comprising the index was converted to natural logarithmic form prior to standardization and summation for the final measure. For the same reason, population size and volume of trade were similarly transformed.

11. Research currently in progress focuses on this question.

12. Notice that controls for size and port-status are utilized *within* each of the broad city size groups in the second phase of the analysis.

13. In view of nonlinear relationships which developed involving both the index of interdependence and the measure of metropolitan function with population size, size was converted to natural logarithmic form. The other variable of interest, port status, applied to 27 BEAs with port facilities handling over 10 million tons annually. Although the measure of port status could exhibit a partly artifactual relationship with the index of external trade (since 4 of the 11 components of the trade index involve the tonnage of BEA commodity shipments/receipts), for several reasons this is not considered likely. First, the volume of commodity-specific shipments/receipts is not included in the index of interdependence in gross form, but in comparative (Z-score) fashion. Thus, the absolute tonnage of any specific product grouping traded by a BEA is important for the index only insofar as a high absolute volume also indicates a high level compared to the volume of this commodity traded by every other BEA. Moreover, any artifactual relationship is further attenuated because differences between product groupings in weight are controlled in constructing the index. That is, although extensive trade in a single, high-density commodity (e.g., coal) may result in a high absolute level of tonnage, a high score on the volume component of the index of interdependence results from high relative levels of trade in numerous commodities. Since not all products exhibit the

same weight per unit (e.g., textile mill products weigh less than coal), major ports handling a limited number of heavy commodities may have lower scores on the trade index than do non-ports with more extensive trade in numerous products but a lesser overall volume of absolute tonnage. Therefore, both the likelihood and magnitude of any artifactual relationship are clearly minimal. In any case, the association of port status with other variables is only of secondary interest. The major concern is that, whatever its effects, they be controlled while examining the relationship between interdependence and metropolitan function.

14 The position of New York on the index of interdependence probably reflects the limitation of the data to bulk and manufactured commodities and the concomitant exclusion of such high-order linkages as finance in which New York is extensively involved (Duncan et al., 153)

15 Using ordinal-level statistics,  $r_s = .63$  ( $p < .001$ ).

16. Although it is convenient to report the findings in terms of zero-order and partial correlations, results based on partial regression coefficients parallel those presented here. Also, levels of statistical significance are reported even though the entire universe of BEAs is examined. As Blalock points out, "it makes sense to make significance tests even when one has data for the entire population . . . in order to rule out the simple 'change processes' alternative" (242).

17. In other words, this is a plot of the second-order partial correlation between extra-local trade and metropolitan functions for these BEAs, net of population size and port status

18 Also, Figure 1 is based on the adjusted (residualized) relationship. It will be recalled that New York ranked highest on the *unadjusted* index of metropolitan function.

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# Postdoctoral Training in Bioscience: Allocation and Outcomes\*

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## ABSTRACT

*The careers of 557 biochemists are studied in order to answer the following questions: Who gets postdoctoral training and why? How does such training affect subsequent employment opportunities? Does postdoctoral training increase later research productivity? Results show that predoctoral research productivity has no effect on who gets postdoctoral training or where one gets it. Getting postdoctoral training does not seem to affect one's chances of getting a prestigious job, but where the training occurred has a major impact on the prestige of subsequent jobs. In contrast, having had postdoctoral training seems to result in substantial increases in later citation rates, but where the training occurred makes little difference in citation rates. The modest effect of postdoctoral training on publication rates disappears when employment sector is held constant.*

During the past two decades, postdoctoral training has become a significant component of science education in the U.S., especially in the biomedical sciences. Precise estimates of the number of scientists engaged in postdoctoral training are hard to come by, in part because the status, postdoctoral trainee, is so poorly defined and institutionalized in most universities (Curtis). Nevertheless, one recent study reported that between 1971 and 1975 over 55 percent of Ph.D. recipients in biomedical specialties took postdoctoral appointments within a year after receiving their doctorate. Moreover, by 1977 there were more than 6,000 postdoctoral trainees in biomedical fields, a number that grew at an annual rate of 12.5 percent since 1973 (Coggeshall et al.). This rapid growth has prompted concern that large numbers of new doctorates may be settling for temporary re-

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search positions in the face of diminishing opportunities for tenure-track appointments (National Research Council, d), and there is persuasive evidence that this is indeed the case (Coggeshall et al.).

A more fundamental concern is whether an investment of more than one hundred million dollars annually (National Research Council, d) can be justified on the basis of its outcomes. Unless it can be shown that postdoctoral training appointments are made wisely, that they are of some later value to the recipients, and that they generate measurable benefits to science, then it would be difficult to justify the investment as more than a remote and expensive form of unemployment compensation.

Aside from its importance for public policy, the postdoctoral training position provides a strategic site for the study of stratification and productivity in science (Reskin). Many postdoctoral positions—especially those that are called fellowships—confer prestige on their recipients. Even those with little prestige value may have important career benefits in the form of additional research experience and valuable credentials. Hence, to the extent that the norm of universalism obtains in science (Merton), one would expect these positions to be allocated on the basis of assessed ability or previous performance.

A related question is whether the university department in which a scientist receives postdoctoral training has an impact comparable to that of the doctoral department on later career chances. Many studies have documented the strong association between prestige of the doctoral department and prestige of subsequent academic affiliation (Berelson; Caplow and McGee; Cole and Cole; Crane; Hagstrom and Hargens; Hargens and Hagstrom), and recent studies have suggested that this effect is not explained by greater ability and/or performance of a department's graduates (Long; Long et al.). Does the same pattern hold for the postdoctoral department and, if so, does the postdoctoral department partially or entirely mediate the effect of the doctoral department? Finally, an examination of the relationship between postdoctoral training and later publication and citation patterns may help to answer more general questions about the effects of social contexts on scientific productivity.

To provide some tentative answers to these and other questions, we have studied the career histories of two cohorts of biochemists who received their doctorates in the late fifties and early sixties. The analyses reported here turn on three events in the early postdoctoral career. The first of these, getting a postdoctoral appointment, occurs within a year after receiving the Ph.D. degree. The second, entry into the regular labor force, ordinarily occurs during the same year for those who do not take postdoctoral training, but anywhere from one to four years later for those who do. The third consists of research productivity during the interval from eight to ten years following receipt of the doctoral degree.

## Data

We collected biographic and bibliographic information, entirely from public data sources, about recipients of the Ph.D. in biochemistry from U.S. universities in fiscal years 1957, 1958, 1962, and 1963. Since no systematic differences among these cohorts were found, they are combined in all tables reported here. The list of doctorates was obtained from commencement rosters of individual universities and verified from separate sources.<sup>1</sup> Female biochemists were eliminated from the sample because their numbers were few and because biographic coverage was poor. The following analyses are thus restricted to 557 male biochemists for whom sufficient information could be obtained.<sup>2</sup>

Information about postdoctoral training was part of a career history gathered for each biochemist through 1972. Data were obtained from *American Men and Women of Science* (Cattel) and, where necessary, from a range of supplementary sources such as *American Doctoral Dissertations* (Xerox, a). No small problem in the study of postdoctoral appointments is their sheer identification. As our starting point, we accepted the National Research Council's (b) definition of a postdoctoral appointment as

... a temporary appointment, the primary purpose of which is to provide for continued education or experience in research, usually, though not necessarily under the supervision of a senior mentor. Included are appointments in government and industrial laboratories which resemble in their character and objectives postdoctoral appointments in universities.

This definition corresponds almost precisely with that used by Curtis and Coggeshall et al.

Applying this definition to job titles listed in *American Men and Women of Science* led to certain ambiguities. The job titles we found fell into two classes:

1. There were 177 biochemists whose first jobs after receiving their doctorates were unambiguously described as "fellow," "postdoctoral fellow," or "trainee". Throughout the remainder of the paper we will refer to all such positions as simply fellowships and to the incumbents as fellows.
2. Another group had job titles such as "research associate," "research assistant," or "scientist." When jobs with these titles were held for less than five years, they were taken to be postdoctoral training appointments. There were 94 biochemists who held such appointments. Hereafter we will refer to these positions as research associateships.

In most of the analyses reported below, we combined these two groups into a single class described synonymously as postdoctoral trainees, trainees, appointees, or postdoctorals. Nevertheless, for each analysis we

determined whether, in fact, it was appropriate to treat the two classes as functionally equivalent. Any differences will be noted below.

The prestige of the doctoral department was measured by the complete three-digit rating of faculty quality of biochemistry departments, a partial listing of which appeared in Cartter. These scores ranged from 1.00 for the least prestigious to 5.00 for the most prestigious. The prestige of the postdoctoral department and the department of the first subsequent job was more difficult to measure because the biochemists often held positions in departments other than biochemistry.<sup>3</sup> Accordingly, a "bioscience" prestige score for each university was constructed by taking a weighted average of the Roose and Andersen ratings of the departments of biochemistry (1/2), chemistry (1/4), physiology (1/12), microbiology/bacteriology (1/12), and pharmacology (1/12).<sup>4</sup> These scores also ranged from 1.00 to 5.00.

For all but 8 of the biochemists, the name of the dissertation supervisor (hereafter referred to as the mentor) was obtained from *Dissertation Abstracts* (Xerox, b), *Directory of Graduate Research* (American Chemical Society), or a mail survey of graduate deans. A measure of the mentor's research accomplishments was obtained by counting citations to his or her first-authored publications in the 1961 *Science Citation Index* (Institute for Scientific Information). While these counts will be interpreted as a measure of eminence, it should be kept in mind that they may reflect both the performance of the scientist and his or her standing in the scientific community.

Biochemists' research productivity was measured by counts of published articles and citations to them. *Chemical Abstracts* was used to locate the articles, and counts of citations were obtained from several volumes of *Science Citation Index*. By counting citations to individual articles rather than to authors, we were able to obtain counts for *all* publications, not just those of which our biochemists were first author. For the analyses reported here, we used citation and publication counts for the three years ending in the year the doctorate was awarded (predoctoral productivity), and counts for the eighth through tenth years after receipt of the Ph.D. (For a detailed discussion of how these measures were constructed, see the appendix in Long). All of the analyses reported here used the natural logarithm of publication or citation counts. Before taking the logarithm, 0.5 was added to each count (Allison).

Beyond these key variables, we used Astin's measure of selectivity for each biochemist's undergraduate institution. This index has values ranging from 1 to 7, with 7 being the most selective. It has been interpreted by some to be a crude indicator of academic ability of baccalaureate recipients and by others as a measure of the quality of the undergraduate education. In any case, a number of studies have shown it to be a moderately good predictor of future success in science (e.g., Hagstrom and Hargens; Reskin).



## Results

### DETERMINANTS OF POSTDOCTORAL TRAINING

Relative to doctorate recipients in most scientific fields, biochemists are intensely committed to postdoctoral study. Among the biochemists studied, 49 percent pursued postdoctoral training within a year of earning the Ph.D. In this section, we examine those factors which had an impact in determining who did and who did not pursue postdoctoral training. The choice of variables for this analysis was guided, in part, by the expectation of universalism in science—that rewards should be allocated on the basis of meritorious behavior, or in anticipation of meritorious behavior at some time in the future. Accordingly, it was expected that predoctoral research productivity would have a substantial effect on the allocation of postdoctoral positions. We also anticipated that characteristics of the biochemists which could be interpreted as indicators of scientific ability might also have some impact. Such indicators include undergraduate selectivity, prestige of doctoral department, mentor's prestige, and age at which the biochemist received his doctorate.

Of course, to the extent that these characteristics are *not* valid indicators of ability, their influence can be interpreted as a failure of universalistic processes. In fact, they could also be seen as evidence for ascription. An additional ambiguity arises from the fact that, in order to have been a postdoctoral trainee, biochemists must have both applied for and accepted such positions. Thus, whatever factors appear to affect the allocation of postdoctoral positions may be more indicative of the preferences of new doctorates than the preferences of those who select among them. This is especially important in view of the fact that postdoctoral positions are not an unmitigated good: not only must the trainee incur the cost of temporarily forgoing a more permanent and usually better paid job, but there is also evidence that trainees lag behind non-trainees in salary for several years thereafter (Coggeshall et al.).

With these cautions in mind, let us turn to the results in Table 1. We first estimated a linear regression equation in which the dependent variable was a dummy variable coded 1 if the person was a postdoctoral trainee and 0 if not. As expected, biochemists from prestigious departments and with prestigious mentors were more likely to receive postdoctoral training, net of other variables. These effects are modest, however, and are dominated by the negative effect of age at the receipt of the doctorate, and by the fact that those who received their degree from schools of agriculture were much less likely to undertake postdoctoral training than those with degrees from arts colleges or medical colleges. Furthermore, those who were married when they received their doctorates were significantly less likely to have been postdoctoral trainees. Undergraduate selectivity has a positive

**Table 1.** ESTIMATES FOR LINEAR AND LOGIT MODELS PREDICTING ENTRY INTO POSTDOCTORAL TRAINING

Independent Variables	Linear Model			Logit Model	
	Metric Coeff.	Stand Coeff.	t	Coeff.	t
Ph.D. in ag. school	-.226	-.22	-5.06	-1.017	-4.81
Married at Ph.D.	-.128	-.09	-2.25	-.594	-2.21
Ph.D. prestige	.0457	.09	2.21	.210	2.19
Age at Ph.D.	-.0200	-.15	-3.79	-.096	-3.76
Undergrad. selectivity	.0217	.08	1.89	.100	1.89
Mentor's citations	.0327	.10	2.19	.149	2.15
Publications	.0219	.03	.65	.087	.58
Citations	-.0213	-.04	-.82	-.096	-.81
R <sup>2</sup>	.139				

effect that is statistically significant at the .05 level with a one-tailed test but not with a two-tailed test.

Most surprisingly, the effects of the two predoctoral productivity measures are far from significant and, in the case of citations, the sign is opposite to that anticipated. To be sure that these null results were not a consequence of multicollinearity (the correlation between publication and citation counts exceeds .60), we reestimated the equation omitting one or the other of the two productivity measures. The coefficients were still far from reaching statistical significance. In a similar analysis for 231 male chemists, Reskin also failed to find a significant effect of predoctoral publications. Contrary to the results reported here, however, she also found no significant effects of undergraduate selectivity, prestige of doctoral department, counts of mentor's publications, and length of time between bachelor's degree and doctorate (which is highly correlated with age at Ph.D. in most samples).

These results could be called into question by the dubious legitimacy of using a dichotomous dependent variable with ordinary least-squares multiple regression. In particular, it is now well known that the use of a dichotomous dependent variable may lead to inconsistent standard error estimates and, hence, biased *t*-tests of the hypothesis that each coefficient is equal to zero (Hanushek and Jackson). To rule out this possibility, we estimated a logit model by the method of maximum likelihood. As seen in the last two columns of Table 1, the results are almost identical to those from the linear regression. (The coefficients are substantially higher, but that is an inherent feature of the logit specification.) The most meaningful comparison is the *t*-test for the hypothesis that a coefficient is equal to

zero, and from Table 1 it can be seen that the logit and linear models produce remarkably similar *t*-statistics.

The most striking feature of these results is the absence of any effects of predoctoral publication and citation counts, a finding which clearly runs contrary to the hypothesis that postdoctoral training positions are awarded for meritorious behavior. This negative result cannot be dismissed on grounds of insufficient research productivity by the time of receipt of the doctorate. These biochemists had published as many as 16 papers by this stage in their careers, with a mean of 1.14 papers, and had received as many as 74 citations, with a mean of 2.64. So there was research productivity—some of it quite visible—which could have been used as at least one of the criteria for awarding postdoctoral positions. Moreover, in analyses reported elsewhere (Long et al.), we have shown that predoctoral publications are the single best predictor of citation counts made ten years later. These results are corroborated in Tables 6 and 7 of this paper. Thus, it cannot be argued that predoctoral productivity is a poor indicator of future productivity.

The positive effects of prestige of doctoral department, prestige of mentor, and undergraduate selectivity may be explained in at least three ways:

1. These origin statuses were used as indicators of ability in awarding postdoctoral positions.
2. Purely ascriptive processes determined which scientists got access to valued resources.
3. Those who scored high on all three measures were more motivated than others to seek out and accept postdoctoral training positions.

Without further data, there is no way to distinguish among these explanations. Age at Ph.D. can also be interpreted as an indicator of ability and, thus, its strong negative effect can be seen as consistent with the hypothesis of universalism. Precisely because the effect is so strong, however, we suspect that it is best explained in terms of individual motivations. Older Ph.D.s are undoubtedly more concerned about getting their careers started and have more to lose from investing additional years in poorly paid research training. The effect of marital status can be explained in the same way: those with responsibilities for family support may be less willing to take a monetary loss.

The much lower rates of postdoctoral training among agricultural biochemists may be due to the fact that agricultural science is generally oriented more toward mission and application than to basic research (Mayer and Mayer). As shown below, biochemists with agricultural doctorates were much more likely to take jobs outside of academia. The route of a postdoctoral training position, on the other hand, seems geared more toward basic research in an academic setting. A second explanation is that

more postdoctoral positions were available to those whose research is medically rather than agriculturally oriented. Curtis (235) reported that over 60 percent of the postdoctoral fellowships in biochemistry were funded by the U.S. Public Health Service.

There is still one criticism that might be leveled at the analysis reported in Table 1. Reskin has argued that, for some new doctorates, a postdoctoral training position is an honor given for work well done and a stepping stone to a better job in the future. For other persons, however, such a position is just a job taken because no better job is available. She hypothesized that the processes leading people into postdoctoral positions and the consequences of having held such positions are very different depending on which of these two functions is paramount.

This argument suggests that lumping all temporary postdoctoral positions together may produce misleading results. To examine this possibility we divided the 271 postdoctorals into the two groups discussed earlier: 177 fellows and 94 research associates (RAs). We reasoned that fellowships would be more likely to emphasize the honorific functions while research associateships would have more of the character of jobs. Also relevant is the fact that fellowships were more likely to be awarded in nationwide competition while research associates were typically hired by sponsoring scientists who controlled a research grant. If there were any differences between these two groups, it was expected that fellowships would be allocated according to more universalistic criteria.

To test this hypothesis, we estimated a *multinomial* logit model (Hanushek and Jackson, 210-14) predicting whether a biochemist would fall into one of three categories: fellowship, research associateship, or no postdoctoral training. To make for easy comparison with Table 1, the results in Table 2 are presented as contrasts between each of the two postdoctoral training categories and the residual category of no training.

Only two of the differences between the two postdoctoral categories reach statistical significance. Age at Ph.D. has a very strong negative effect on getting a fellowship instead of no postdoctoral training; but virtually no effect on getting a research associateship rather than no postdoctoral training. This result may also be expressed somewhat differently: given that a person had postdoctoral training, age at Ph.D. had a significant effect ( $t = 4.08$ ) on whether the training took the form of a fellowship or a research associateship. The other difference between the two training categories is in the effect of undergraduate selectivity: it is non-existent for RAs in contrast to non-trainees and positive for fellows compared to non-trainees. Although marital status appears to have a somewhat stronger negative effect for the fellow non-trainee contrast than for RAs versus non-trainees, the difference is not statistically significant. The effects of the other five variables are similar to those reported in Table 1. In particular, there is still no effect of predoctoral productivity for either of the two contrasts.

**Table 2.** MULTINOMIAL LOGIT MODEL DISTINGUISHING TWO TYPES OF POSTDOCTORAL TRAINING

Independent Variables	RAship vs. No Training		Fellowship vs. No Training		t test for Difference
	Coeff.	t	Coeff.	t	
Ph.D. in ag. school	-.990	-3.39	-1.03	-4.21	-.13
Married at Ph.D.	-.244	-.66	-.728	-2.49	-1.34
Ph.D. prestige	.191	1.49	.221	1.96	.21
Age at Ph.D.	-.0095	-.30	-.169	-5.04	-4.08
Undergrad. selectivity	.0002	.00	.167	2.64	2.21
Mentor's citations	.169	1.82	.140	1.73	-.28
Publications	.129	.65	.065	.38	-.30
Citations	-.068	-.44	-.124	-.91	-.33

While the differences in the two types of postdoctoral training are consistent with Reskin's claims, they do not undermine the conclusions drawn from Table 1. The fact that both age and marriage have substantial negative effects on getting a fellowship and very small negative effects on getting a research associateship seems quite reasonable in light of the economic explanation advanced earlier. To get a fellowship, one must typically enter a nationwide competition several months in advance. Thus, getting such a position requires effort and planning that would be expected only of someone who really wanted a fellowship and thought he would be qualified to get one. On the other hand, it seems plausible that the selection of research associateships would take place at a local level shortly before the position had to be filled. Then, when a new doctorate had exhausted all other possibilities, he might turn to a research associateship; older and married doctorates might be just as likely to do so as younger, single doctorates.

Of course, to the extent that age at Ph.D. is an indicator of scientific ability, these results could also be taken as evidence that merit is more important in the allocation of fellowships than in the allocation of research associateships. Adding weight to this interpretation is the fact that undergraduate selectivity—a less ambiguous indicator of ability than age at Ph.D.—affects the likelihood of getting a fellowship but not the likelihood of getting a research associateship. There does not appear to be any way to choose between these two interpretations with the data in hand. Nevertheless, the major finding holds: predoctoral productivity does not affect the probability of getting either type of postdoctoral position.

## DETERMINANTS OF POSTDOCTORAL PRESTIGE

Postdoctoral training positions also vary in other ways. While nearly all of the postdoctoral positions were in academic settings (a few were at NIH), there was substantial variation in the prestige of the academic institutions. Since prestige is both rewarding in itself and is also an indicator of other desirable features of departments (Cartter; Hagstrom), it is natural to ask whether it was allocated among postdoctoral trainees according to universalistic criteria. In other words, one might expect to find that the most able and productive Ph.D.s get their postdoctoral training in the most desirable places.

In some respects, this is a better site to examine the operation of universalism than the mere receipt or non-receipt of a postdoctoral position. Once a new biochemist has decided to seek postdoctoral training, it makes sense to aim for a position in the most desirable location in terms of career advancement. And since there are few costs in choosing a prestigious institution over one that is not so prestigious, it is reasonable to assume that candidates will seek out and choose the most prestigious institutions. As a consequence, those factors which determine postdoctoral prestige ought to be more indicative of the preferences of those awarding the positions than the preferences of those seeking the positions.

The evidence for those preferences is shown in Table 3, which gives the results of regressing postdoctoral prestige on the same independent variables found in previous tables. Only two of the variables attain significance at the .05 level, prestige of the doctoral department and the dummy variable indicating a degree from an agricultural school.<sup>5</sup> Again, predoctoral productivity has virtually no impact. This differs somewhat from Reskin's results. She found that not only doctoral prestige but also undergraduate selectivity and predoctoral publications had significant effects on postdoctoral prestige. However, Reskin's measure of prestige was based on the title and funding source of the postdoctoral position itself rather than on the department in which the position was located.

It appears then that many more factors influence whether a biochemist had a postdoctoral position than influence the prestige of that position. Moreover, Long found that for a subgroup of 136 biochemists (those who went on to tenure-track positions) the effect of doctoral prestige on postdoctoral prestige was largely explained by the fact that many individuals had their doctoral and postdoctoral training in the same institution. When these inbred scientists were excluded from the sample, the correlation virtually disappeared. In short, the allocation of postdoctorals among different institutions seems almost random, following neither universalistic nor ascriptive criteria. Nevertheless, as we shall see shortly, that allocation had major consequences for subsequent career chances.

**Table 3.** REGRESSION PREDICTING PRESTIGE OF POSTDOCTORAL INSTITUTION (N=200)

Independent Variables	Standardized Coefficient	Metric Coefficient
Ph.D. in ag. school	-.17*	-.334 <sup>a</sup>
Married at Ph.D.	-.06	-.129
Ph.D. prestige	.16*	.129 <sup>a</sup>
Age at Ph.D.	-.04	-.010
Undergrad. selectivity	.08	.040
Mentor's citations	.08	.048
Publications	-.06	-.068
Citations	.09	.075
R <sup>2</sup>		.098

\*Coefficient significant at .05 level

#### IMPACT OF POSTDOCTORAL TRAINING ON THE FIRST JOB

As previously noted, there is evidence that postdoctoral training is not a sound financial investment for the individual scientist, that the opportunity cost is not cancelled out by greater subsequent earnings than those of non-trainees. Is it possible that there is some form of non-monetary payoff from postdoctoral training, beyond the opportunity to improve one's research skills? One hypothesis is that a postdoctoral position increases the chances of getting a desirable job, where desirable refers to non-monetary rewards. According to the National Science Foundation, "Postdoctoral appointments have been relied on traditionally by Ph.D. holders as stepping stones to permanent research appointments in academic institutions." Table 4 provides some evidence for this claim. For all 557 biochemists, we estimated a linear regression in which the dependent variable was a dummy variable coded 1 if the first "real" job (immediately after the doctorate or after postdoctoral training) was in academia and 0 if it was not (63 percent of the jobs were academic).<sup>6</sup> Results are shown in the left-hand panel of Table 4. The linear regression was also corroborated with a maximum likelihood logit analysis (*t*-statistics from both analyses are shown for comparison).

Only two variables had statistically significant effects on the probability of an academic job, and both these effects were quite strong. Those with agricultural degrees were much less likely to enter academia while those with postdoctoral training were much more likely to do so, a result consistent with previous studies (Folger et al.; Reskin). In another regression (not shown) there was no difference between those with postdoctoral

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Table 4. ESTIMATES FOR LINEAR AND LOGIT MODELS PREDICTING FIRST JOB IN ACADEMIA

Independent Variables	All Biochemists (N=557)				Postdoctoral Trainees (N=200)			
	Stand. Coeff.	Metric Coeff.	t	Logit t	Stand. Coeff.	Metric Coeff.	t	Logit t
Ph.D. in ag. school	-.21	-.209	-4.68	-4.50	-.16	-.160	-2.08	-2.08
Married at Ph.D.	-.03	-.036	-.63	-.63	-.05	-.054	-.71	-.73
Ph.D. prestige	.06	.027	1.33	1.33	.00	.0018	.05	.09
Mentor's citations	-.08	-.027	-1.82	-1.79	.07	-.023	-.87	-.90
Publications	.05	.029	.92	.96	.04	.024	.47	.56
Citations	.03	.012	.49	.48	.09	.041	.99	1.04
Postdoctoral training	.30	.193	4.67	4.55	--	--	--	--
Postdoctoral prestige	--	--	--	--	.03	.013	.34	.34
R <sup>2</sup>		.104				.045		

titles of fellow or trainee and those with titles of research associate or assistant; both were equally likely to enter academia.

The interpretation of these effects remains ambiguous. On the one hand, it could be that academic departments give preference in their hiring to individuals with postdoctoral training or to those with non-agricultural degrees. An alternative explanation is simply that those with agricultural degrees are less interested in academic positions and do not bother to apply. Similarly, one would expect that Ph.D.s who are not interested in an academic career would not seek out postdoctoral training—the opportunity cost would simply be too great. Once again we face the quandary whether the observed associations reflect candidates' preferences or employers' preferences.

For the 200 biochemists who did take postdoctoral training, we estimated a second regression to see if the *prestige* of the postdoctoral institution had any impact on whether the first subsequent job was in academia. The results in the right-hand panel of Table 4 show that this was not the case. Among postdoctorals, the only factor affecting the academic-nonacademic dichotomy was a degree from an agricultural school.

Of course, simply getting an academic job is not enough. Presumably the biochemists who got academic jobs were also interested in getting a job at a prestigious institution. Was postdoctoral training any advantage in getting a more prestigious job? The results in the left-hand panel of Table 5 provide an answer to that question. For the 348 biochemists whose first real job was in academia, we regressed the Roose-Andersen rating for that institution on several of the variables examined previously. Although those with postdoctoral training did slightly better than those without (a difference of .211 on the 5-point scale), the difference was not significant at

Table 5. REGRESSIONS PREDICTING PRESTIGE OF FIRST JOB

Independent Variables	All Those at Rated Institutions (N=348)		Former Postdoctorals at Rated Institutions (N=150)	
	Stand. Coeff.	Metric Coeff.	Stand. Coeff.	Metric Coeff.
Ph.D. at ag. school	-.15**	-.440**	-.16*	-.497*
Married at Ph.D.	-.04	-.147	-.12	-.359
Ph.D. prestige	.25**	.314**	.12	.151
Mentor's citations	.09	.082	.12	.114
Publications	.01	.015	-.11	-.185
Citations	.08	.106	.10	.125
Postdoctoral training	.08	.211	--	--
Postdoctoral prestige	--	--	.26**	.313**
R <sup>2</sup>	.14		.17	

\*Coefficient significant at .05 level.

\*\*Coefficient significant at .01 level.

the .05 level. The two variables which did have significant effects were prestige of the doctoral department and the dummy variable indicating a degree from an agricultural school. The two predoctoral productivity measures had little effect, corroborating results of Long et al. for a subset of this sample.

Despite this null result, it cannot be said that postdoctoral training was inconsequential in getting a good job. *Where* that training took place had a major impact. For the 150 postdoctorals whose first position was in a rated academic institution, we regressed institutional prestige on the usual variables *plus* the prestige of the postdoctoral institution. The results in the right-hand panel of Table 5 show that postdoctoral prestige is by far the most important determinant of the prestige of the first job. In fact, the magnitude of the effect (either standardized or unstandardized) is almost identical to that of *doctoral* prestige in the regression for the full sample. For postdoctorals, the effect of doctoral prestige declines to the point where it is no longer statistically significant. The only other variable which affects prestige of first job is, as before, the dummy variable indicating a degree from an agricultural school. It thus appears that when a biochemist takes a postdoctoral position, the prestige of his new institutional affiliation replaces the prestige of his doctoral department. The choice of a postdoctoral institution then becomes critically important in determining one's future career chances.

It is not yet clear just how postdoctoral prestige facilitates a biochemist's entry into the academic labor market, although it seems plausible

that the same processes operate as with doctoral prestige. Elsewhere (Long et al.) we have argued that neither the prestige of the mentor nor predoctoral productivity explain away the effect of doctoral prestige, and the same may be true for postdoctoral prestige. This cannot be tested for mentor prestige since we do not know the name of the postdoctoral supervisor. However, we do have counts of publications and citations for the postdoctoral years. When those are substituted for the measures of predoctoral productivity in the regression for the 150 postdoctorals, the results hardly change at all (regression not shown). It cannot be maintained, then, that postdoctorals at prestigious institutions got better jobs because they published more or better papers during the postdoctoral training period. Rather, it appears that hiring departments pay more attention to where one got postdoctoral as opposed to doctoral training.

#### EFFECTS ON LATER PRODUCTIVITY

From the perspective of science policy, the key issue is not whether postdoctoral training leads to desirable careers but whether it actually improves the research capabilities and contributions of trainees in measurable and lasting ways. A committee of the National Research Council (c) has characterized the postdoctoral appointment as "a combination of intensive research activity and an opportunity to enhance the research technique of the trainee under the guidance of an experienced investigator." Thus, a reasonable expectation is that returns from the national investment in postdoctoral training would take the form of greater research productivity—papers published and citations received—by those with postdoctoral training than by otherwise comparable doctoral scientists.

Several studies have found sizeable differences in counts of publications and citations between those with and those without postdoctoral training (Folger et al.; National Research Council, a; Reskin) and this study is no exception. Our dependent variables were the number of papers published by each biochemist in the interval from eight to ten years following receipt of the doctorate and the number of citations made to those papers. While strongly correlated ( $r = .62$ ), citations were far from being a simple multiple of publications.

On the average, former postdoctorals published 26 percent more papers than non-postdoctorals (means were 4.77 and 3.78 respectively) and received 75 percent more citations (means were 24.07 and 13.78 respectively). While these differences raise the possibility that postdoctoral training made for later productivity, they hardly demonstrate that fact. An alternative explanation is that postdoctoral training positions were given to the most able doctorates who would have published more than their fellow cohorts even without postdoctoral training. Without a randomized experiment, that explanation can never be entirely ruled out. Nevertheless, by

statistically controlling for other characteristics of biochemists—especially predoctoral productivity—we can get some better indication of the plausibility of these competing hypotheses.

Table 6, Panel A, presents the results of a linear regression in which the dependent variable was the natural logarithm of the number of publications,<sup>7</sup> and the independent variables were those seen in the previous analyses, including a dummy variable indicating postdoctoral training. The strongest effects are clearly those of doctoral prestige, age at Ph.D., and number of predoctoral publications, all in the expected direction. The effect of having held a postdoctoral position is marginally significant at the .05 level with a one-tailed test. Because of the logarithmic transformation of the dependent variable, the unstandardized coefficient for the dummy variable can be transformed into a percentage increase by the expression  $100[\exp(b) - 1]$  where  $b$  is the coefficient. Thus, the coefficient of .161 for the postdoctoral dummy corresponds to former postdoctorals publishing 17 percent more than non-postdoctorals, net of other variables. We see, then, that while the effect of postdoctoral training on later publication rates is reduced by the introduction of other variables, it is not entirely eliminated.

It is possible, however, that the effect of postdoctoral training on

Table 6. REGRESSIONS PREDICTING LATER PUBLICATION COUNTS

Independent Variables	All Biochemists (N=557)				Postdoctorals (N=200)	
	A. Stand Coeff.	Metric Coeff.	B. Stand Coeff.	Metric Coeff.	C. Stand. Coeff.	Metric Coeff.
Ph.D. in ag. school	-.08*	-.170*	-.03	-.056	-.05	-.122
Married at Ph.D.	-.03	-.090	.01	.032	.03	.086
Ph.D. prestige	.12**	.116**	.08*	.074*	.10	.095
Age at Ph.D.	-.13**	-.034**	-.16**	-.042**	-.13*	-.040*
Mentor's citations	.07	.049	.07	.047	.05	.036
Publications	.14**	.178**	.10*	.124*	.01	.019
Citations	.10*	.099*	.11*	.109*	.12	.129
Postdoctoral training	.03*	.161*	.05	.099	--	--
Postdoctoral prestige	--	--	--	--	.08	.100
High prest. academia	--	--		.389**	--	--
Research center	--	--		.344**	--	--
Business or industry	--	--		-.354**		
R <sup>2</sup>	.139		.199		.075	

\*Coefficient significant at .05 level.

\*\*Coefficient significant at .01 level.

publication rates is not a result of extended training and research experience but rather a consequence of the fact that former postdoctorals are more likely to be employed in settings where research is expected and supported. As seen in Table 4, for example, postdoctorals were much more likely than others to have their first subsequent job in an academic setting. To test this possibility, a set of dummy variables representing employment sector was added to the regression equation. The three variables were chosen to represent four sectors: high prestige academia, low prestige academia, non-academic research center, and business or industry.<sup>8</sup> The results in Panel B of Table 6 support this alternative explanation. Employment sector has a substantial effect on later productivity (the *R*-square increases from .139 to .199) while the effect of postdoctoral training declines substantially.<sup>9</sup> According to the new equation, postdoctorals publish only 10 percent more papers than others, a figure that is not significantly different from zero.

It appears, then, that the effect of postdoctoral training on later publication rates is explained by the fact that postdoctorals got jobs in research-intensive settings (Long and McGinnis). It could still be the case, however, that *where* one got postdoctoral training made a difference in later productivity. Panel C of Table 6 gives evidence against that hypothesis. For the 200 postdoctorals in rated universities, the logarithm of publication counts was regressed on the measures of predoctoral status plus the prestige of the postdoctoral institution. Although in the expected direction, the effect of postdoctoral prestige is far from statistically significant.

The final step was to replicate the preceding analysis with citations as the criterion variable. Panel A of Table 7 shows that several variables had sizable impact on the number of citations, all in the expected direction. As usual, the best predictor was the number of citations to predoctoral publications. The effect of postdoctoral training, however, was highly significant and represented a 37 percent edge over non-postdoctorals. While this is a substantial decline from the 75 percent advantage that appears in a simple comparison of means, it is still a large effect. We then added the three dummy variables for employment sector to see if the effect of postdoctoral training could be explained by its impact on the sector of the first job. Again employment sector makes a sizeable difference in citation rates and does explain a small part of the postdoctoral effect. That effect is still highly significant, however, and represents a 32 percent advantage on the part of postdoctorals. When postdoctorals were divided into fellows and research associates, there was no difference between the two groups in later citation rates (regression not shown). Last, we attempted to see if the prestige of the postdoctoral institution had any impact on later citation rates. The regression results in Panel C of Table 6 offer no support for that hypothesis.

Table 7. REGRESSIONS PREDICTING LATER CITATION COUNTS

Independent Variables	All Biochemists (N=557)				Postdoctorals (N=200)	
	A. Stand. Coeff.	Metric Coeff.	B. Stand. Coeff.	Metric Coeff.	C. Stand. Coeff.	Metric Coeff.
Ph.D. in ag. school	-.09*	-.221*	-.06	-.152	-.12*	-.316*
Married at Ph.D.	-.02	-.054	.01	.037	-.01	-.027
Ph.D. prestige	.15**	.162**	.12**	.124**	.12	.126
Age at Ph.D.	-.14**	-.042**	-.16**	-.048**	-.15*	-.050*
Mentor's citations	.08*	.058*	.07	.051	.07	.061
Publications	.02	.028	.00	.000	-.04	-.063
Citations	.24**	.273**	.24**	.271**	.20*	.222*
Postdoctoral training	.14**	.317**	.12**	.277**	--	--
Postdoctoral prestige	--	--	--	--	.02	.024
High prest. academia	--	--		.430**	--	--
Research center	--	--		.334**	--	--
Business or industry	--	--		-.073	--	--
R <sup>2</sup>	.210		.224		.113	

\*Coefficient significant at .05 level.  
\*\*Coefficient significant at .01 level.

Summary and Discussion

Here are the major conclusions we have drawn from the preceding analysis:

1. Predoctoral productivity had no effect on the likelihood of getting post-doctoral training. For those who got such training, predoctoral productivity had no effect on the prestige of the postdoctoral institution. Yet measures of predoctoral productivity were among the best predictors of productivity several years later.
2. Older doctorates and those with degrees from agricultural colleges were much less likely to get postdoctoral training, net of other variables. Prestige of doctoral department and prestige of the mentor also had moderate positive effects on the likelihood of getting postdoctoral training.
3. The prestige of the postdoctoral institution was modestly affected by the prestige of the doctoral department and the college in which the doctorate was earned.
4. Postdoctoral trainees were much more likely than others to move into academic jobs, net of other variables.
5. Of those who entered academia, former postdoctorals were no more likely than others to get jobs in prestigious departments. Among postdoc-

torals, however, the prestige of the postdoctoral institution replaced doctoral prestige as the best predictor of the prestige of the first academic job.  
 6. Postdoctoral training appears to have some positive effect on later publication rates, although this is largely a consequence of the fact that postdoctorals tend to get jobs in research-intensive sectors.

37. Net of other variables, postdoctorals get over thirty percent more citations to their later publications than non-postdoctorals. This difference is *not* attributable to the different employment patterns of former postdoctorals.

8. Prestige of postdoctoral institution appears to have no effect on later productivity.

9. Age at Ph.D. had a strong impact on both publication and citation rates, net of other variables.

These findings suggest some tentative answers to the three questions which guided this study:

Are postdoctoral training positions awarded according to universalistic criteria? The impact of doctoral prestige and mentor prestige is consistent with other studies and is ambiguous with respect to universalism. These variables could be interpreted either as indicators of scientific ability or as indicators of position in a social network that relies on ascriptive processes. The strong negative effect of age at Ph.D. on the probability of getting postdoctoral training is also ambiguous in its interpretation. A natural explanation is that older Ph.D.s would not want to further extend their educational careers in the absence of large payoffs. Yet others have used age at Ph.D. (or closely related measures) as an indicator of ability, and the strong negative effects of age on later productivity lends support to this view. Hence, the negative effect of age could also be seen as evidence for universalism.

*These ambiguous results are overshadowed, however, by the fact that established records of research productivity have no impact whatever on whether one gets a postdoctoral training position.* These are the least ambiguous indicators of scientific ability and, as we have seen, early productivity is a good predictor of productivity ten years into the career. It appears that those who make decisions about postdoctoral positions are either unconcerned with scientific ability or else they believe that predoctoral publication does not indicate a candidate's potential.<sup>10</sup>

Does postdoctoral training enhance one's position in the job market? Certainly postdoctorals are much more likely to get academic jobs, but it could be argued that this is simply because those who choose a non-academic career see no point in postdoctoral training. It must be remembered that when these doctorates were seeking jobs, academic positions were relatively plentiful. Thus, the lack of postdoctoral training was not a barrier to getting a job in academia. In fact, of those entering academic jobs, 42 percent had *not* had postdoctoral training.



The real question is whether, having entered the academic labor market, one could get a better job with postdoctoral training than without. If better means more prestigious, the answer is no. On the other hand, if a biochemist did get postdoctoral training, it made a big difference where that training took place. Postdoctorals at better schools got jobs at better schools.

If postdoctoral training per se did not enhance one's job opportunities, why then did new doctorates choose to participate in such training? Possible explanations include erroneous beliefs, tradition, and inability to find an acceptable academic job. Again, the last explanation seems implausible in view of the booming academic job market of the early 60s. It may be that new doctorates anticipated the answer to our third question: Does postdoctoral training enhance one's research productivity? Our results suggest that it has little if any effect on the volume of productivity, but may have important effects on the visibility or utilization of one's work. To the extent that new doctorates desire their work to have maximal impact on other scientists, choosing to get postdoctoral training would appear to be a rational decision. These results also suggest that the allocation of public monies to support postdoctoral training has at least some measurable impact on the quality of scientific work. Whether that investment is cost-efficient is a question well beyond our capabilities to answer.

Assuming that postdoctoral training really does increase the visibility or use of biochemists' work, it is natural to ask how that effect is produced. The conventional wisdom is that postdoctoral training enables one to gain additional specialized skills and knowledge under the direction of an experienced investigator. In short, it is essentially an extension of graduate training with a greater emphasis on research skills. If that were the case, then the quality of training should vary greatly across departments and mentors, and this variation should be reflected in later citation rates. Yet prestige of the postdoctoral institution (which is known to be a fairly good proxy for resources and faculty quality) has virtually no impact on later citation rates. Thus productivity outcomes show a pattern exactly opposite to that of job outcomes: getting postdoctoral training makes a difference but where one gets it makes no difference.

To explain this apparently anomalous result, we suggest that the key resource involved in postdoctoral training is something that does not vary much across institutions—time to do research without teaching responsibilities. We suspect that time is so important because it enables the postdoctoral person to put aside his dissertation topic and begin investigations of greater potential importance (Curtis). In contrast, the new doctorate who immediately begins his career with a tenure-track position must cope with his new responsibilities as a teaching faculty member. While that may not reduce the quantity of research, it may greatly inhibit one's ability to venture into new areas. In short, we believe that one of the chief functions of postdoctoral training is the facilitation of intellectual mobility.

Can these conclusions be generalized to other scientific fields? We see no reason to suspect that other fields would differ. Although biochemistry has long been a field with one of the highest rates of postdoctoral training, it has only been at the vanguard of a long-term trend. In our sample, 49 percent of the biochemists pursued postdoctoral training. Several years later, between 1971 and 1975, 78 percent of biochemists and 57 percent of all biomedical doctorates took postdoctoral training (National Research Council, b). Similar trends have been documented in chemistry and physics (Coggeshall et al.). In short, many other fields are now investing in postdoctoral training at levels comparable to those in biochemistry in the late fifties and early sixties.

But are the processes the same in different fields? Obviously more data from a variety of disciplines are necessary for answering this question. Yet biochemistry itself provides some suggestive evidence for the invariance of causes and consequences of postdoctoral training. One of the unique features of biochemistry is the split between agricultural and medical orientations to the field. In every one of the tables presented above, those who obtained their degrees from agricultural schools differed significantly from other biochemists: They were much less likely to get postdoctoral training, and those who did take postdoctoral training did so in less prestigious institutions. Similarly, they were much less likely to start their careers in academia, and those who did take academic jobs were in less prestigious institutions. Finally, their rates of publication and citation were significantly lower than those of other biochemists. In many respects, then, these two subdivisions of biochemistry behave almost as though they were two quite different fields.

Despite these differences, the processes operating *within* each of these two groups of biochemists were remarkably similar. For all of the linear and logit regression models presented above, we tested for interactions between a degree from an agricultural school and all of the other variables. This is equivalent to testing whether the regression models were the same for the two groups. In only one case was there a significant difference: the effect of marital status on getting postdoctoral training was stronger among those with an agricultural degree. In short, major differences in commitment to postdoctoral training, academic career lines, and publishable research do not add up to much difference in the causes and consequences of postdoctoral training.

## Notes

1. These sources included *National Faculty Directory* (Gale Research Company), *Directory of Graduate Research* (American Chemical Society), *American Doctoral Dissertations* (Xerox) and *Dissertation Abstracts* (Xerox).

2. We estimate that this number represents nearly 90 percent of all males who received biochemistry doctorates in those years and who remained in the U.S.

3. About 15 percent of the trainees held multiple postdoctoral appointments. For these, the rating of the last postdoctoral institution was used.
4. These weights are roughly proportional to the number of biochemists holding jobs in the different bioscience departments. The complete three-digit ratings of all rated institutions were kindly supplied to us by Charles J. Andersen.
5. In another regression (not shown) we added a dummy variable indicating whether the postdoctoral position was a fellowship or a research associateship. There was not a significant difference between the two types of position.
6. A job was defined as academic if the employer was an institution of higher education. No distinction was made between tenure-track or non-tenure-track positions, or between four-year and graduate institutions.
7. For a justification of the logarithmic transformation of productivity measures, see Allison. Before taking the logarithm, 0.5 was added to each individual's publication or citation count. Note also that the logarithmic transformation is very similar in form to the square-root transformation used in some other analyses of these data (Long, Long et al.). It is unlikely that different results would have been obtained if the square root transformation had been used here.
8. The median Roose-Andersen bioscience rating was taken as the dividing line between high and low prestige academia. Unrated institutions were classified as having low prestige. Most of those in non-academic research centers were employed by NIH or USDA. The numbers of biochemists in each employment sector were: 166 in high prestige academia, 185 in low prestige academia, 113 in non-academic research centers, and 93 in business or industry.
9. We also examined the possibility of interaction between the effects of postdoctoral training and employment sector. None was statistically significant.
10. It is often claimed that predoctoral publications are not good indicators of scientific potential because most predoctoral publications are co-authored with faculty mentors. The results in Tables 6 and 7 do not support this claim.

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# G. H. Mead's Orthodoxy

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## ABSTRACT

*Recent representations of George Mead's view of society have opposed it to the dominant view by emphasizing its dynamic aspect at the expense of the inertia to which Mead also gave full recognition. He saw that inertia exercised through culture. Without using the term, culture, Mead recognized its force in the notion of sociality, as well as in a whole complex of other interrelated concepts. In addition, Mead represented social evolution as a cultural development, viz., a growing universalism in the institutions. Finally, as further evidence that Mead should not be set apart from the mainstream, it is shown that his position does not require a special methodology.*

Sociology now offers at least six models for the analysis of the social process—the functionalist, conflict, symbolic interactionist, exchange, ecological, and evolutionary. While they scarcely present us with additional models, critical theory, phenomenology, and ethnomethodology add three further orientations. This diversity does not mean that we have simply to choose between these approaches just as one might choose between the plates on a cafeteria rack according to personal taste. The intellectual problem posed by a discipline with this diversity is to compare the approaches for their comprehensiveness and adequacy, and to ask whether they have common, complementary, or contradictory elements and whether a synthesis amongst them may be possible. My own view is that there is more of both commonality and compatibility between these approaches than stereotypic thinking would let us believe. Furthermore, sociology as a whole is already in process of consolidating a fairly standardized conceptualization in certain very broad outlines; yet stereotypic polemics over the approaches can obscure the fact.<sup>1</sup> My purpose in this paper is to show that George Herbert Mead's conceptualization of the social process sits well with a basic feature of this standardized conceptualization. Because it does so, it must be accepted as fundamentally in line with any other approach that does the same. In addition, I will show that, contrary to what some would have us believe, Mead's position does not demand a special methodology.

## Separation of Social Organization and Culture

One of the securely taken territories in sociological conceptualization that should never be surrendered is the depiction of society as an interplay of social organization and culture. Society has a foreground and background, social structure two levels: the superficial and deep. It may be easier for observers to focus on only one of these at a time, but the reporting we cherish most pulls them together. Precisely what two-way traffic occurs between these levels of social life has yet to be exhaustively spelled out.<sup>2</sup> But some of the most captivating analyses have led us directly to that interface. Think of Weber's<sup>3</sup> demonstrations of cultures being imposed as the outcome of power struggles between status groups; Marx's (a, 12–20, b, 19–23; Marx and Engels) notions of ideology emerging from the relations of production; Durkheim's (a, b) studies of legal systems varying with the division of labor, and suicide with the presence and absence as well as the type of cultural norms. Decision-making studies are positioned here, and studies made under the rubrics of both symbolic interactionism and ethnomethodology can bring us to this junction. At first glance, the products of these two latter approaches can seem so much alike as to be taken for the same thing. Typically, though, they are not exactly the same; even though, as I say, they do share a location. Typically, symbolic interactionist studies follow the outward journey from culture to social organization while ethnomethodological studies follow the return journey down the same track. By this I mean that ethnomethodological studies show people making their action meaningful and legitimate by referring it back to the culture—the world taken for granted—that silently governs their life. Symbolic interactionist studies, on the other hand, notice how people venture out, how they take initiative and solve problems by foreground adjustments; yet this is always from a launching pad in a world that is there and which is not, in itself, problematic for them.

Not all studies that distinguish between society's superficial and deep levels designate the levels as social organization and culture. This is a great pity, for it would have hastened the standardization of sociological analysis if they had. It would also, incidentally, have helped dispel the sense of jangling diversity that invariably afflicts the sociological novice today. There have been inhibiting misunderstandings about social organization and culture of course. Some writers avoid the notions, considering that an analysis made in terms of them is an empty reification that leaves the individual out. Yet these terms simply fix our attention on the emergent properties of inter-individual life, a life that—anyone would admit—utterly vanishes if the individuals enacting it are taken away. Every patterned interaction is social organization, even if it is only once-for-all, yet we may find the term being reserved for the repetition of such patterns. As

for culture, it seems it may have been the sociologists trained in anthropology who were best prepared to pin its own name on it. There have been others who have not named it culture, even when they were recognizing its presence and power. Durkheim (b, c) was recognizing it when he drew attention to collective representations, Ortega<sup>4</sup> when he spoke of usage, Sumner when he wrote of folkways, mores, and institutions. What became the unifying theme of Max Weber's<sup>5</sup> work was identifying what distinguished the culture of Western civilization from that of Eastern civilizations; yet that work was not explicitly presented as an exploration into culture. Ethnomethodologists might have avoided the false impression that their work is apart from traditional sociology, had they simply named as culture the deep structure that their actors grope for to experience meaning.<sup>6</sup> The phenomenologists, likewise, would have seemed less esoteric, had they intimated that things like typification, the intersubjective world and the-world-taken-for-granted are easily recognizable features of what we ordinarily name culture.<sup>7</sup>

### Cultural Order Recognized in Mead's Notion of Sociality

Now one of the clearest cases of a vivid appreciation of culture, without its being given the name, is found in the thought of George Herbert Mead. Mead's entire thinking about society rests on a conception of a fluid foreground of organization arising out of a relatively fixed background of culture and then fading into culture again, in the sense that it leaves a lasting impression on culture in the form of some modification. I will show that a variety of Mead's major concepts imply this, but the master principle that lays it down is the one he designates *sociality*.<sup>8</sup> There is an inert phase in sociality that puts a break on social change, even while it supplies a necessary condition for having that change occur, and we can equate it with culture. It is true that Mead was a master exponent of the movement of our life through time and of the evolution through time of our own life from that of simpler forms, and his sociality is instanced in these transformations. It is also true that Mead gave special recognition to the part played in these transformations by the initiative-taking individual. Yet none of this allows us to represent Mead's society as all motion, as if he depicted a society that is without statics, without its own indispensable inertia. I fear that Blumer's (a) polemic may have fostered that impression. For Blumer claims to derive a highly motile "symbolic interactionism" from Mead, then opposes that approach to "the dominant view of society in the social and psychological sciences" (a, 72).

In Mead's vision, sociality is a process pervading all of nature, and not simply human society. It is essentially a process in which novelty appears and is then digested. What is social about it is probably more like

socialization than anything else: an intruding newcomer is domesticated. First drawing an analogy from society to trace its operation in the physical and biological realms, Mead then follows its continuance into human society itself. Mead says there is nothing in the system in which they appear to explain new forms of organization in organisms and action: indeed if they could be explained by causes operative there they would scarcely be new. So totally unprecedented are they, they have to be called emergents. Any emergent is nevertheless an outgrowth from an existing system; it does not alight from the beyond. But neither does it belong to the system that fathered it; it does not fit and is therefore meaningless or a-rational for the time being. The system that is being referred to when it is human society that is involved, is the systematization of past experience—and this is Mead's clear allusion to culture. The problem demanding some sort of adjustment when novelty arises at this level is to fit the novelty into experience. That is effected by substituting a new and more inclusive systematization of past experience for the existing one; which means that the past is reformulated so that there will still be a past from which action can continue to venture out.

I think I am being faithful to Mead's account if I illustrate what happens by using the following analogy. This adjustment involved in sociality is like the separate peace, the splitting effect that an outside enemy may have on a group if some of its members discover they are more in sympathy with the enemy than with certain fellow-members, and consequently separate to form a new alliance that incorporates the enemy. The new system salvages from the old those elements that are in tune with the emergent, while it jettisons those that are not. There is something a bit dialectical about this, for it amounts to saying there were contradictions in the old system that the coming of the emergent served to expose. Mead is thus insisting that culture makes a system, that social action can only operate out of it and that it necessarily feeds back into it. There are two cultural systems, in fact, whenever novelty is mastered, since only a modified system can accommodate the novelty—yet there is no discontinuity between these systems. The new culture is only new in being a revision of the old. The magnitude of the revision must vary with the centrality of the emergent to the way of life: in many instances it would not be at all extensive.

### **Cultural Order Recognized in a Connected Network of Further Concepts**

One of the more specific concepts Mead uses to underline the presence of a culturally induced inertia in society is *the perspective* (Mead, a, c). This is also a concept that Mead applies to nature as well, and he makes it mean any *system of order* that can be brought into view by adopting a specific



*standpoint*. System and perspective are virtually equivalent terms for Mead and, for all their being disclosed by a standpoint taken, he believes them to be fully objective. When Mead applies the notion to human society, the order actually bared to our attention is, again, the cultural order, the standpoint for finding it in this instance being that shared by the members of the society in question. This perspective is also the same thing as the *attitude of the generalized other*, which attitude the member must *take* if he is to act in a socially coordinated way (Mead, b, 135–64). By that notion too, then, Mead refers quite directly to culture. The orderliness that characterizes the generalized other is sometimes indicated by Mead's saying that the generalized other's attitude is "*organized*." By this he means to suggest a disposition to engage in a whole range of activities that are coordinated and integrated with one another. In the attitude itself that ensemble of actions is endorsed and has, in fact, begun to unfold. Overtly each individual goes on to enact only the part assigned him, but covertly, in the attitude, he has a readiness for all the parts. Mead's picture of society is of a set of individuals poised thus on the brink of cooperation, ever ready to enact a particular play because *each* of them has memorized the *entire* script. "Memorized" is not the right word, but I use it to indicate that extensive learning is indeed involved for the individuals in getting themselves thus prepared, and that it is the same learning for them all—although in this matter Mead disappointingly short-circuits the account by simply using the word "take." Mead does more justice to the magnitude of the reward of that learning, in that the result is the chance to acquire *the self* itself. For when, in order to be available for effective cooperation with the others, the individual member does take this attitude that those others in any case have also taken, he is endowed with a *me* (Mead, b, 164–222). Though it has to be supplemented by an original initiative-taking *I*, the *me* is the same in all members. This *me* is their whole ensemble of shared *habits*, their habitual shared *responses* to the whole range of *situations* their society typically presents them with. We cannot designate it as anything less, then, than the culture internalized.

If from the whole ensemble of shared responses that a society's members make to their whole connected web of situations, we single out those shared responses that they make to any particular situation, then we have an *institution* (Mead, b, 167, 211, 229–30, 260–73). An institution supplies a relatively fixed—we may of course equally well say a cultural—anchorage for actions taken within its sphere, since decisions taken in that sphere have to be made with reference to it. Yet Mead insists that this does not mean that institutions must be altogether rigid and inflexible, for they are open to modification. What enables the actions of separate individuals to be coordinated in any orchestrated *social act* is the fact that that total, consummated act has the same *meaning* for them all—and *is*, in fact, meaning for them (Mead, b, 75–90). These *shared meanings* realized through their

completed social acts are the *universals* in the experience of human beings. Expressed attitudes or *gestures* are *indications* individuals make, simultaneously to themselves and others, that they should all proceed to make specified contributions to the construction of some such social act. There is definitely a "should" here; Mead presents indication as normative for there is a summons in it, even though this is another large matter that he tends to treat briefly. When these expressed attitudes or gestures are read and responded to by all who will be involved, and in the way that was intended, they are themselves made meaningful by transference—due to the fact that they thus *symbolize* meanings. For this reason they may be called *significant symbols* (Mead, b, 61–75, 181, 190–1).

But, of course, every bit of this talk of meanings and their symbols presupposes a certain depth of previous experience, a preexistent culture, on the part of Mead's actor. Meaning inheres in completed social acts, and no act is completed until it is behind us. And how could the beginning of an act indicate a specific denouement unless that beginning had led to that conclusion in previous experience? No beginning that is being made for the first time can be meaningful at that point. These symbolizations of shared meanings provide the elements of a common *language* for a set of individuals; assembled together they map out a common *universe of discourse* (Mead, b, 89–90, 268–9, 284). All the symbols of a culture may be taken to make a language in a metaphorical, and yet broader and more basic sense; the language comprised of spoken words becomes a special department of it. Human *communities* are constituted by the fact that certain sets of people are admitted to the same universe of discourse, they employ the same ensemble of symbols (Mead, b, 156–8, 195, 202). This is, of course, tantamount to saying that a community is that set of individuals who share a culture. Finally, the last thing for us to note is that it is only through membership in such a culture-owning community that the individual can *think* and *act* (Mead, b, 7, 68–75, 135–46, 181–2, 186–92, 259–60; c). For thinking is using the community's symbols to indicate meanings to oneself; acting is responding to those of its symbols that are directed to the common attention, responding, at the overt level, by making the contribution assigned to one.

Now that we have considered the set of more specific terms through which Mead alludes to culture, in a context supplied by his treatment of sociality, the evidence that he ascribed importance to culture seems overwhelming. In fact, one might be inclined to marvel that anyone could give so full an elucidation of the interconnections between a diversity of aspects of culture without naming it by name—and yet to do that would be anachronistic. For the term "culture" had not acquired the distinctness, richness, and salience in Mead's circle that it has for us (even though the word was, of course, in their vocabulary).

### Mead's Social Evolution Is a Transformation in Culture: A Growing Universalism in the Institutions

Since community is constituted by sharing, the boundaries of community can be extended if there is an increasing universalism in whatever is offered to be shared. For Mead, then, social evolution proceeds in the direction of widening cooperation, the widening of community by extending the eligibility for membership in this way—which means, in a sense, by making the generalized other as general as possible. It means by establishing rights; for rights are generalized entitlements. (That is, I may insist that something is my right only if I and the community uphold it as something to which everyone else is equally entitled.) It means as well that community is widened by finding out and bringing out what it is that everyone *can* respond to in the same way. Throughout the course of history Mead sees just such a development to have occurred through transformations in the institutions. His account of this in the "Society" section of *Mind, Self and Society* (b, 227–336) makes a climax to his sociological reflections; yet it has received comparatively little attention. That neglect is not easy to understand—unless it is simply due to the accident that the behavioral scientists who first found inspiration in Mead were people whose own preoccupations were already more psychological.

In Mead's account there were two early developments permitting this evolution toward universalism, one economic and one religious. The economic development occurred when everyone was freed to make exchanges in a market in the same way. The religious development occurred when religion defined everyone as standing in the same relation to others: all were neighbors to one another—or brothers. Here were two standpoints both of which defined a universal society open to the entire race. Furthermore, the sharing of a universe of discourse—a culture and its included spoken language—is carried with the religious sharing of brotherhood. For neighbors—brothers—are compelled to find a common communication; and this for Mead is their shared logical thinking rather than any conventional tongue, necessary though such a tongue will be to give that thinking voice. Understood in that way, then, language as the vehicle of logical thinking is a third door onto universal society. A political expression of the same broadening impulse also dawns. It takes the form of the consolidation and unification of originally separate political units. Realized at first through dominance, it is transformed in time into the acceptance of responsibility for centralized administration.

The marriage of the attitude of universal religion with this attitude of the inclusive administration is what found expression in the ideology of democracy. Rousseau's *Social Contract*, according to Mead, thus extended the universalism conferred on the individual by religion into a gospel for the political life. In Rousseau's vision, every individual would be able to

stand on the same ground as every other one—each might maintain himself as a citizen; yet he would only succeed in doing so by recognizing the right of everyone else to do the same. Doing this, each individual would be taking the attitudes of the subject and sovereign simultaneously; while, at the same time, everyone would be willing the same thing. So it would become possible to speak of a *general will*. (Encountering this in Mead at this point, we are suddenly made to appreciate the affinity between Rousseau's general will and Mead's generalized other.) Mead's point is that there is something in this accumulation—free market, universal religion, common language, and central administration—that has been furthering an evolution. It is an evolution that helps realize the ideal possibility that inheres in the very nature of society due to the fact that society is a cooperating community whose members cohere by taking a generalized other.

In the above way Mead follows into human history the evolution that he first notices in man's advance on the lower animals through the use of symbols. In result, it is a movement from a clumsy, closed society that admits and entitles only the privileged, to a universal society whose membership is open and whose entitlements are guaranteed to all by right. Since it is to this wider cooperation we are headed, Mead rounds out his treatment of society by examining two obstacles offered to that advancement now. Internal conflict is the first he considers. As is typical of the social process theorists, he posits both associative and dissociative impulses in the human being; and, typically also, he allows that the latter have been used constructively in society when they were curbed and controlled by the former or mobilized against an enemy without. Yet he insists there is a peculiar character to *modern* conflict that keeps that standard resolution from sufficing. It is occasioned by the fact that members of the highly developed and organized society share certain interests at the same time as they are in conflict over others. Furthermore, the conflicting interests are each generated from and supported by a definite social structure—so for that reason these conflicts are only to be settled or terminated by a social reconstruction. The typical outcome of this kind of conflict, then, is—heightened thinking. The minds of those involved are stabbed awake; their strained situation is defined as a problem demanding solution, and they are stirred up to furious speculation on the reconstruction possible. Finding a solution depends, first, on their bringing out explicitly the common interests they *can* affirm; and, second, on their imagining what larger social whole could feasibly substitute for the society they are in, and in which the conflicting interests would be accommodated. As such larger wholes are successively established they exhibit "greater and greater degrees of functional, behavioristic differentiation among the individuals who constitute them" (Mead, b, 310), these differentiations being the now transformed expression of the initial oppositions of interest.

Finally, Mead reminds us, besides having the features already as-

cribed to it, the ideal society to which we tend must be perfectly integrated in the sense that:

the meanings of any one individual's acts or gestures (as realized by him and expressed in the structure of his self, through his ability to take the social attitudes of other individuals toward himself and toward their common social ends or purposes) would be the same whatever for any other individual who responded to them (b, 310).

But in a complexly differentiated society achieving that integration imposes a prodigious challenge of communication, and the size of this challenge is the second of the obstacles to present advancement that Mead examines. First, in this integrated society, common attitudes will have to be induced in all individuals. This is no drab uniformity that stifles a fitting individuality and diversity but the eliciting at a deeper level of commonalities that are altogether indispensable. Here is another place where the scarcely submerged concept of culture is bobbing against the very surface of the discussion. For here we clearly have a call for the effective internalization of a culture, and we can see that appropriate socializing procedures must be put in place for it. But second, in this integrated society, all individuals will have to be able to take the attitudes of all those others whom their action affects. There is no communication at all without this feedback, for simply broadcasting messages is not communication in the Meadian sense. Mead writes about this requirement only at an abstract level; but he does say that in the democratic society of his day it was not yet realized. It would appear to be far from realized yet, and to wait on a multitude of highly ingenious innovations in the modes of social participation and response.

### **Mead Truncated then Falsely Labeled Different**

There is something quite sublime in Mead's panoramic vision of total societies and their historic transformation, which seems to have been quite lost sight of in his professed followers' preoccupation with individuals' adjustments and their interpersonal encounters. There is, of course, no reason why a developing tradition of social interactionism should stay squarely with Mead; yet it would seem that his followers have been greatly impoverished because of what they have neglected.

It is, indeed, cause for thought that Mead's followers have not only neglected a major aspect of his thought, but have not extended the part they acknowledged. For, as Kuhn has shown, the tendency has been to take off quickly from Mead along one of a number of tangents. He instances eight of these: role theory, reference group theory, social and personal perception, self theory, phenomenological theory, career description,

interpersonal theory, and language-and-culture analysis. On the other hand, we must also note that Blumer, who claims to have stayed with Mead, has not extended Mead's thought either, but has simply restated his view of it repetitiously and ritually. Kuhn thinks the ambiguities and imprecision in Mead's writing may account for the scattering among those who drew inspiration from it, particularly an ambiguity over the determinacy or indeterminacy Mead ascribed to individuals and their actions. This may well be true, and it may also account for the lack of any followers fired with a passion to develop Mead's thought along its own lines, yet beyond the point where Mead left it. Another thing inhibiting development from Mead could, of course, be his utterly schematic social thought. For, as we have seen, Mead can condense in a word—such as "take" or "indicate"—complex mechanisms that are already analyzed in detail now. But possibly there is a more ultimate source of the paralysis. Thinking in step with Mead can induce circularity; for in order to account for mind, self, and society Mead has always to place us in a situation where they are already present. Even that, however, need not deter us from building from his thought, if we are armed by drawing the appropriate inference that what we can accept from him is analytical rather than explanatory theory.<sup>9</sup>

In my own view, the greatest mischief resulting from the adoption of the truncated Mead is that it has emboldened Blumer to engage in his polemic, setting symbolic interactionism against the dominant sociological position. "Symbolic interactionism" is Blumer's term for what he found sociologically relevant in Mead's social behaviorism, and it seems quite an inspired choice. Even though Blumer's own symbolic interactionism gives no full reflection of Mead's, one would like to keep "symbolic interactionism" for the whole, comprehensive tradition. But, as I hope I have shown, *Mead's* symbolic interactionism is entirely in line with the standardized conceptualization crystallizing in sociology. I would add that it is very much in line with functionalism in particular. Indeed, the first step in any synthesis of the nine approaches listed at the beginning of this paper, could well be the marriage of symbolic interactionism with functionalism. (But, having said that, one is forced to add the marriage may be exposed as incestuous, when everyone realizes what close kin the partners were!) Turner has shown that Parsons was a symbolic interactionist, so much so that in developing his pattern variables and typology of objects he was doing the symbolic interactionists' work for them. How could these approaches be believed to be at odds when, as Turner has also shown, they have the following features common: voluntarism and action effected through orientation to situations, the indication of objects by symbols, the construction of acts, and personality as both the product and agent of social action! Moreover, the matter reviewed in this paper allows us to add another four common features to the list: the orderliness and boundedness of the cultural system, consensus as the ground of community, integration

as a functional imperative, and universalism and differentiation as indices of social evolution.

### **No Different Methodology, Either**

Although Blumer (a, 1–60, 127–39) accepts responsibility for the inferences drawn, he sees implications in Mead's thought that compel him to oppose the dominant methodology in sociology and psychology also. I find myself partly in sympathy with this particular revolt, yet mainly not. I am in sympathy insofar as Blumer is led to reject the artificiality in routinely resorting to a standard bag of research procedures, since that may sometimes distract from the empirical truth rather than bring it to light. But I do not think Blumer really offers viable alternatives to orthodox methodology; where he represents himself as doing so his divergence from orthodoxy is either only apparent or actually mistaken.

No exception can be taken to Blumer's insistence that we use methods that capture the real characteristics of the social world; methods, for example, that acquaint us with the meanings people give to their situation and the objects in it, that show them as initiative-taking in their interaction, or that recognize the place of this initiative in sustaining large-scale organization. As well as approving this, one can fully endorse Blumer's plea that researchers get a closer familiarity with the social worlds they venture to collect data on. This can be done, he says, through what he calls the naturalistic procedures of exploration and inspection. Yet there is nothing innovating here; for the fruit of exploration is description and the fruit of inspection is classification, and these are the standard first phases of all science. (Blumer defines inspection as picking out what one takes to be empirical instances of a concept, in order to better distill the essence of the concept by noting what it is that diverse instances have in common.) But not only does Blumer's wish for a true-to-life sociology detain him longer in these first phases of scientific investigation, there is a distinct possibility it may entrap him there. There is little evidence that he can move freely from that base into scientific generalization.

In an essay specially devoted to it Blumer (a, 127–39) gives recognition to variable analysis as a way for so moving. But he says that variable analysis cannot deal with certain central areas of social life and that these require a different scheme of analysis, although he assigns only a single paragraph to suggesting what the rudiments of such a scheme might be. One thing that had caused Blumer to demur is that variables in their pure abstractness do not capture the rich complexity of the social process in the way we need. Yet this simply shows him reluctant to accept that the isolation of factors is of the essence of scientific procedure, and the thing that makes it so rewarding. No one doubts that, in any science, the factors we

elect to consider are isolated from a complex context; but we do isolate them because of the handle that gives us. There was another matter. Reiterating his concern over the typical repression from sociological reporting of the actors' interpretations of meanings, Blumer says variable analysis cannot in any case deal with that. He is right to insist that this be reported; for the meaning to the actors of their action is as much a part of social action as their overt behavior. But he is wrong to conclude that it is unamenable to treatment as a variable; for one has simply to note in what ways—relevant to the research problem—the act of interpretation can vary. But what, in lieu of variable analysis, does he propose as our procedure here? The one he requires is specified as follows:

This procedure is to approach the study of group activity through the eyes and experience of the people who have developed the activity (a, 139).

But here, right at his climax, Blumer takes an atavistic dive into descriptive sociology; this is no procedure for moving into scientific generalization. This symbolic interactionist shibboleth of taking the role of the other must have seemed a great flag for the loyal symbolic interactionist to wave. But it doesn't really have anything to do with the case. If anyone made this his ultimate research procedure he would never rise above description. We look at group activity through the eyes of the actors in order to get a completer description of it, *not in order to get the explanation of it*. We get the explanation of it by looking at it with our own eyes from a different and more distant perspective; one in which we isolate as variables whatever *we* take to be the salient features of it. Remembering Mead's observations on multiple perspectives might have helped symbolic interactionists avoid a great and paralyzing confusion here. No one knew better than Mead that *our* science is *our* perspective on things that are intellectually problematic for *us*. Part of what may be problematic for us is the very interpretation that we will, as good observers, have observed the actors to make.

After careful scrutiny, then, we must conclude that the methodological implications Blumer draws from Mead are not at odds with the standard methodology, save that in mistakenly circumscribing variable analysis Blumer guillotines much inquiry at the point of description or classification. Yet beyond this again is the question of how faithfully Blumer received and transmitted Mead's methodological position at all. That whole question was recently ventilated in exchanges in the *American Sociological Review*.

McPhail and Rexroat (a) had charged that Blumer's methodological perspective really differed from Mead's; and in a later issue, which published Blumer's (b) reply and their final rejoinder (McPhail and Rexroat, b) we find them unwilling to retract. A basic objection they raise is that Blumer's narrowness—the very narrowness leading him to discount variables and the explanation made possible by hypothesizing on relations



between them—that this narrowness is contrary to Mead's more comprehensive and, in any case, more orthodox conception of science. For to Mead, as I've indicated, science is an activity engaged in to solve problems of puzzling relationship as these are experienced and formulated by the investigator. Consequently, for Mead, the methodology for investigating social behavior is not different from that to be used in any other field of science.<sup>10</sup>

## Conclusions

Neither in his view of the social world nor in his view of the science by which it may be studied did Mead diverge from the dominant view now held in sociology. Insofar as Blumer has represented him as doing so, in order to legitimate a separatist "symbolic interactionism," he has been unfair to Mead and would seem to have served the discipline poorly.

## Notes

- 1 For some observations on this see Fallding (c).
- 2 A conceptualization of the field of sociology designed to help accumulate knowledge of this kind is given in *The Sociological Task* (Fallding, b).
- 3 This is essentially the perspective from which Weber (a,b,c,d) analyzes the emergence of dominant systems of religion.
- 4 It is fascinating to follow Ortega's phenomenologist's quest for the essence of the social and his finding it in *usage*. It reads like another independent discovery of sociology's subject matter.
- 5 A number of interpreters (e.g., Bendix, Martindale) have pointed out how, through a great range of studies dealing with institutions as diverse as agriculture, industry, government, religion and music, Weber took note of the rationality by which the culture of the West was distinguished.
- 6 There are various applications and interpretations of ethnomethodology (Denzin, Garfinkel, Garfinkel and Sacks, Zimmerman and Pollner, Zimmerman and Wieder).
7. There are various applications and interpretations of what, for not altogether clear reasons, has come to be considered a sociological variety of "phenomenology" (Berger and Luckmann, Schutz, a, b, c, d).
- 8 It is in his Paul Carus Foundation lectures given in 1930, only a year before his death, and published in *The Philosophy of the Present* (a) that Mead finally made the principle of sociality explicit and central. But it is plainly anticipated in the earlier work in Mead's vivid sense of phased temporal development.
- 9 For an account of the difference between these two types of theory and the critical importance of observing it see Fallding (b, 24–42).
10. The question of appropriate *procedure and logic*, which is what the question of methodology is, is not to be confused with the question of technique, which makes the question of *methods*. Simply because he takes a general view of the logic of science, there is no reason to think Mead would deny a place for techniques devised to match the special properties of social behavior. There is another question again, equally distinct, that applies to empirical investigations taking inspiration from Mead: the question of whether they utilize observations that supply genuine instances of his variables—and this is the question of operationalization. McPhail and Rexroat (a) review a small collection of empirical studies that they consider to connect with Mead's theory in some way. However, it is doubtful whether any of these gives an operationalization of a strictly Meadian taking-the-attitude-of-the-other, the notion so cen-

tral to Mead's analysis. For what is required for that is not simply that persons be in relationship but in process of constructing a cooperative act, and that the partners to the act do not actually do what "other" does but have a readiness to do it. Cottrell's classic study perhaps comes closest to this, for it shows people to have the kind of capacity Mead's cooperators will require, in that even *observers* of others are shown to have a readiness to do as those others are doing. As for the mothers who open their mouths when feeding baby—standard data from O'Toole and Dubin that McPhail and Rexroat (a, 460–1) also refer to—it is dubious that this is operationalizing anything from Mead. Most mothers I have known to do this are hoping to induce the baby to imitate *them*, thereby learning that the thing to do with offered food is take it in. More often than not the mothers open their mouths precisely because the baby's is firmly closed. This is hardly taking the attitude of the baby! O'Toole and Dubin are convinced that what they observed was not imitation being invited, resting this certainty on the very tenuous consideration that mothers opened their mouths after the baby's opened. But feeding is a sequence of repeated acts! A mother's mouth open after one swallow by the babe is a mouth open in anticipation of the next one. One Meadian study McPhail and Rexroat might have considered is one of my own which showed a fuller family life to develop where members took the attitudes of the other members (Fallding, a).

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# A Structural Analysis of Class Voting\*

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## ABSTRACT

*Studies of the issue disagree about the reasons for—even the existence of—a low level of class voting in Canada. In order to study class voting, three items must be conceptualized and measured adequately: the class of the voters, the class of the parties, and the structural constraints on political partisanship. Central to our concerns is that the only true leftist party, the New Democrat party (NDP), does not field a candidate in every constituency and is not really a viable party everywhere it does contest the election. Thus, the working classes do not always have the realistic option of voting NDP and research which does not take this into account attenuates the relationship between class and voting. We hypothesize, therefore, that (a) the level of class voting is higher in constituencies in which the NDP is viable and (b) the level of nonvoting among the working classes is higher where the NDP is not viable. Reconceptualizing class in Marxian terms, our results indicate that class is related to voting and that there is a statistical interaction among class, voting, and the viability of the NDP. Furthermore, nonvoting among the working classes increases in nonviable constituencies and decreases in those constituencies where the NDP is viable, indicating that nonvoting may be a class response similar to voting NDP. Implications of this analysis for other Western democracies are noted.*

Social class, the principal structural cleavage in Western democracies, has a substantial political importance that typically is reflected in the party system (MacIver). Alford contended that an association between class and voting choice is "natural and expected." Rose and Urwin analyzed support for 25 leftist parties and found that 20 of them drew two-thirds or more of their support from the working classes. In fact, Lipset (a) maintained that

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the outstanding fact about support for political parties in almost every economically developed country is that the middle class votes primarily for parties of the right and the lower class primarily for parties of the left.

Despite apparently congenial conditions—a multi-party electoral system and close ties to Western European political traditions—it has been claimed that this generalization does not apply in Canada. Therefore, the Canadian case deserves further examination for judging the generality of this proposition. Various scholars have followed Alford's lead in arguing that Canada has the lowest level of class voting among Western industrial democracies, and have suggested two reasons for this: (1) Canadians do not perceive politics in class terms; and (2) the strength of religious, regional, and ethnic ties have constrained the development of class politics (Alford; Englemann and Schwartz; Meisel; Porter). Alford summarized his analyses by describing Canada as a case of "pure non-class politics."

There are those, however, who contend that there is class politics in Canada. For example, there is evidence of pockets of class voting in various provinces and constituencies, e.g., Waterloo South (Wilson), Biggar (Las-kin and Baird), and Middlesex East (Simmons). Consequently, Ogmundson (b,c) and Schwartz have both argued that class awareness exists, but that the translation of this awareness into a vote is hampered by the absence of a major working class party.

There obviously is considerable disagreement about weight to be accorded the lack of class awareness in the population (Alford; Porter) and the failure of parties to polarize on class issues (Regenstreif; Schwartz) in explaining the lack of class voting. Much of this dissensus may occur because of a lack of conceptual clarity and resultant inadequate measurement. For example, employment of controls for mass perceptions of the class base of the parties (Ogmundson, c) and for methodological flaws in estimating the indices of class voting (Myles) suggest that levels of class voting are higher than previously suspected. Clearly, to examine the relationship between class and electoral politics adequately, three phenomena must be conceptualized and measured properly: the class position of the individuals, the class positions of the parties, and any structural constraints on political partisanship (Przeworski and Sprague). It is the thesis of this paper that there are difficulties with the way in which all three have been conceptualized and measured in previous investigations in Canada. This research attempts to clarify the basis of these difficulties, to help resolve some of these issues, and to produce less ambiguous results that will have implications for research on the same issues in similar countries.

#### CLASS POSITIONS OF INDIVIDUALS

In studies of class and politics in Canada, most attention has been devoted to politics and class has been treated only residually. Three of the most

noted studies (Alford; Ogmundson, c; Wilson) used a manual–nonmanual occupational distinction to represent social class.<sup>1</sup> There are three problems with using this categorization of occupations as a measure of social class. First, even if one accepts the underlying rationale for this practice, recent theoretical analysis (Braverman) and empirical evidence (Hamilton, a; Vanneman) suggests that the manual–nonmanual distinction is no longer the most salient in modern Western societies. Second, the practice of treating occupations as classes fails to recognize that occupations, positions in the *technical* division of labor, are not equivalent to social classes, which designate *social relationships* between actors (Wright and Perrone). Third, it assigns an effect to occupation *per se*, rather than to those factors for which it may be an index. Weber, whose analyses are often invoked to defend this last practice, actually wrote that "'property' and 'lack of property' are, therefore, the basic categories of all class situations" (927). Furthermore, Anderson noted that "occupational enumeration, far from being the backbone of the reward structure, is largely shaped by the powers of organized property over salaries and wages and over the very composition and make-up of the occupational structure itself" (122).

In contrast to these analyses, we shall use a conception of social class which is in close accord with Marxian tradition. Recent studies, in both Canada (Stevenson) and the United States (Wright, b; Wright and Perrone) have demonstrated the possibility of using Marxian class categories in the study of stratification. For Marx, class is not a *thing*, but a *relation* between *positions* in the social organization of production. Marxist analyses usually employ three juridical criteria of social class: (1) ownership of the means of production; (2) employment of the labor power of others; and (3) sale of one's own labor power (Wright and Perrone), which produce three classes; capitalists, petty bourgeoisie, and workers. However, because all positions do not fit these criteria neatly, and because the criteria do not always coincide perfectly, some positions do not fit into this three-class scheme—Wright (b) has termed them contradictory locations in the class structure. The three most important of these contradictory locations are small capitalists, managers (including supervisors and foremen), and the new working class.<sup>2</sup> For further explication, see Wright (b) and Zipp (b).

#### CLASS POSITIONS OF THE POLITICAL PARTIES

There is considerable disagreement as to the class positions of the four major federal political parties. The predominant classification arranges them on a left–right ideological continuum, with the New Democratic party (NDP) as "Social-Democratic-Left," the Liberals as "Center-Left," the Conservatives as "Conservative-Right," and the Social Credit as "Reactionary-Right" (Alford). In many analyses the NDP and the Liberals are grouped as parties of the left and the Conservatives and Social Credit as

parties of the right (Alford). Some students of Canadian politics have criticized this classification by arguing that both the Liberals and Conservatives espouse conservative positions and that there are few, if any, differences between them (Englemann and Schwartz; McLeod; Mallory; Porter; Scarrow). Developing this further, Ogmundson (a) argued that there is both an economic and a social axis on which to consider the liberalism-conservatism of the parties. He classified the Liberals and the NDP as liberal and the Conservatives and the Social Credit as conservative on the social axis, and the NDP and the Socreds as liberal and the major parties as conservative on the economic axis. Zipp (a) corroborated this scheme by finding two significant axes along which to compare the parties, by far the most salient being Ogmundson's economic left-right (the major-minor party) dichotomy. Thus, it appears that the parties differ multidimensionally and that one-dimensional analyses may not be adequate.

#### STRUCTURAL CONSTRAINTS ON POLITICAL PARTISANSHIP

In this study we shall examine the direction of voting in the most recent Canadian Federal election (1974) for which a full set of data on a national sample of voting is available, bearing in mind the fact that voter choices are limited to the parties that field candidates in the constituency. The Liberals and the Conservatives "show the flag" in all federal constituencies, the NDP in virtually all (e.g., in 255 of 265 in 1965), and the Social Credit in very few outside of Quebec. But all of these candidacies, particularly those of the NDP, really are not viable: they are offered mainly in response to pressures for the NDP to be a national party. In fact, the NDP makes a conscious decision to concentrate its efforts in certain constituencies and to neglect others. Ogmundson (c) estimated that in the 1965 election only 80 of the 255 candidacies could be considered viable. Both Meisel and Regenstreif have shown that even if a candidate is fielded, voters hesitate to waste votes on a candidate who is unlikely to win. Thus, "since these minor parties are not a viable alternative in most parts of the country, it follows that this measure of class voting is not too accurate as a measure of the degree of class sentiment in the population" (Ogmundson, c, 102). These statements imply a statistical interaction among class, voting, and the presence of a viable third party. Whenever or wherever the voter is offered a viable third party candidate, the relationship between class and voting is likely to be stronger.

The structural circumstances that bear on the viability and changes in viability of the NDP in individual constituencies can be comprehended more fully by relating the matter to a discussion of Wright's (a) concerning structural causality. He elaborated a scheme of structural causality compatible with Marxist theory that revolves around six basic "modes of determination," that is, "a series of distinct relationships of determination

among structural categories of Marxist theory and between those categories and the appearance of empirical investigation" (a,15). Although the six are related and the understanding of each necessitates understanding the others, one—"structural limitation"—is particularly pertinent to this discussion. Structural limitation is a pattern by which some social structures create boundaries within which some other structures or processes can vary (Wright, a). Rather than being rigidly deterministic, it emphasizes the fact that any process or structure reflects the impact of others.

We would contend that in many ways the nature of the Canadian electoral system structurally limits the viability of the NDP. Traditionally, single-district, single-ballot, "first-past-the-post" systems weaken support for third parties and exaggerate support for the two major parties (Szyman-ski). For example, in the 1974 federal election, the Conservatives received approximately *two and one-third* times as many votes as the NDP (35.4 percent to 15.4 percent), but almost *six* times as many seats in Parliament (95 to 16). In contrast, the NDP received three times as many votes as the Social Credit (15.4 percent to 5.1 percent)—a "regional" third party— but only *one-and one-half* times as many seats (16 to 11). Thus, the federal electoral system discourages *national* but encourages *regional* third parties. The only way the NDP (or any other third party) can challenge the Liberals and Conservatives for control of the national government is by becoming a national party; however, electoral rewards (seats in Parliament) do not accrue proportionately to national third parties.

There are two ways to conceptualize the relationship between the viability of a party and the support it receives. The first is in cause and effect terms: viability determines support or support determines viability. The second conceives of viability and support as parts of one whole, not as two separate processes. Here we adopt the latter perspective and view the issue in dialectical terms; the temporal priority of support or viability is a moot question. The support which the NDP receives in any one election, in one sense, defines its viability; yet, in another sense, the support it receives is conditioned by its Parliamentary strength and by the electoral system. These processes are not only mutually reinforcing: they also manifest an underlying federal-provincial (or federal-regional) conflict. Just as the electoral system fails to reward national parties proportionately, the nature of the federal system has allowed the provinces to increase their strength at the expense of the federal government. This underlying conflict may present problems for the very existence of the Canadian state (Smith and Jackson).<sup>3</sup>

Another flaw in analyses of class and voting that arises from inattention to structural factors has been the neglect of nonvoting and its significance. Of the many explanations of nonvoting (Alford and Friedland; Lip-set, b; Parsons; Schattschneider), we are attracted to the argument that a major reason that people do not vote is that their interests are not repre-



sented among the several choices. Milbrath and Goel summarize several studies in which turnout rates increase when it is in more voters' interests to vote. Accordingly, if an NDP vote is a class-interest response for the working classes but this option is not realistic or even available, nonvoting also may be a class-interest or a "resentment against the system" response. Therefore, viewing *voting NDP and nonvoting as complementary phenomena*, nonvoting should increase and voting NDP should decrease in constituencies where the NDP is not viable, and the reverse would occur where the NDP is viable.

### Data, Measures, and Methods

With one exception, the data employed in this analysis come from the 1974 Canadian National Election Study. A national sample of 2,562 respondents, aged 18 and over, was interviewed in the months following the July 8 federal election. The sample was weighted by province and employing the proper weights reduces the effective sample size to 2,445. (For a thorough discussion of the construction and design of the sample, see Leduc et al.) In addition to this survey, voting returns for the 1972 federal election (Canada) are used. Votes received by each of the four major and all other federal parties were tabulated at the constituency level for all 94 ridings included in the 1974 study.

The variables used in the analysis are listed in Table 1. As with many secondary analyses, the available measures were less than ideal for the purpose. Nowhere was this more evident than with the availability of a desirable indicator of social class. However, enough information was available to construct a class scheme which approaches the one detailed earlier.

Problems in categorizing respondents' social class are responsible for the most substantial decrease in effective sample size. Only those who could be assigned a class location are included in these analyses. This eliminates people who never had been employed or who were unemployed or students at the time of the study and for whom information about a former or regular occupation could not be obtained. Retirees were included only if there was information on a main former occupation. Farmers were excluded because we are concentrating on the industrial sector of society.

Essentially four items were used to distinguish five classes: the traditional working class, the new working class, owners, managers and supervisors/foremen. The five classes are comprised as follows:<sup>4</sup>

*Owners:* Those who owned their own businesses.<sup>5</sup>

*Managers:* By occupational title.<sup>6</sup> All professional, semi-professional and

**Table 1.** INDEPENDENT VARIABLES USED IN ANALYSIS

Variable Name	Definition	Coding
Trad. working	Traditional working class	All else, traditional working class
New working	New working class	All else, new working class
S/F class	Supervisors/foremen class	All else, supervisors/foremen
Owners	Owners	All else, owners*
Viability	Viability of NDP candidate in particular constituency	Non-viable, viable
Maritime	Resident of New Brunswick, Nova Scotia, Newfoundland, or Prince Edward Island	All else, Maritime
Quebec	Resident of Quebec	All else, Quebec
Prairies	Resident of Alberta, Manitoba, or Saskatchewan	All else, Prairies
Brit. Columbia	Resident of British Columbia	All else, British Columbia**
Ethnicity	Respondents' ethnicity	All else, English mother tongue
Main Prot.	Mainstream Protestant religious affiliation	All else, Anglican, United, or Presbyterian
Catholic	Catholic religious affiliation	All else, Greek, Roman, or Russian Catholic
No religion	No religious affiliation	All else, no religious affiliation***
Sex	Respondents' sex	Female, male
Union	Respondents' union membership	Non-member, member

\*Managers are the omitted category.

\*\*Residents of Ontario are the omitted category.

\*\*\*Those with an affiliation other than Mainstream Protestant or Catholic ("Other") are the omitted category.

clerical/sales workers in the supervisory structure; and all real estate, financial, accounting, and advertising sales occupations.<sup>7</sup>

*Supervisors/foremen:* By occupational title.

*New working class:* All professional, semi-professional, technical, and scientific workers not in the supervisory structure.

*Traditional working class:* All skilled, semi-skilled, unskilled, and clerical/sales (except for financial, real estate, accounting, and advertising sales) workers not in the supervisory structure.<sup>8</sup>

Ogmundson (c) estimated that in 1965 only 80 of the 255 NDP candidates could be considered viable. He did not describe how he arrived at this estimate, but he probably used his familiarity with the election. Lacking such insights into the 1974 election, we employed a procedure that may be verified by independent investigators. However, the measurement of

viability is problematic at best. After considering several alternatives (Kornberg and Winsborough; Kornberg et al.; Lee and Kornberg) it appeared most plausible to use the proportion of the vote obtained in the previous election as an indicator of the viability of the party in the current election.<sup>9</sup> The cutting point between viability and nonviability only can be estimated, and in this study we decided that the NDP had to receive 25 percent or more of the vote in a particular constituency in 1972 in order to be considered viable in that constituency in the 1974 election. This figure seems reasonable, if a bit conservative, in a multi-party system.<sup>10</sup> Information obtained from the NDP provincial party secretaries established the construct validity of this measure.

The remaining independent variables, (i.e., sex, union membership, region, religion, and ethnicity) listed in Table 1, represent important cleavages in Canadian society that also are components of the relationship between class and voting in Canada. Labor unions participated in forming the NDP from the old Co-operative Commonwealth Federation in 1961 and research indicates that union membership is highly related to support for the NDP (Chi). It often is asserted that women are more conservative than men (see Hamilton [b] for a review), although others (e.g., Zeitlin) argue that as women share the same experiences as men, they also will be more likely to develop similar political orientations. Focusing on those in the labor force helps in judging whether sex has an effect on the relationship between class and politics. The impact of regional, religious, and ethnic forces on Canadian politics has stimulated much discussion. Historical, cultural, and economic similarities within these groups and differences among them continue to generate important social and political cleavages. In fact, with the exception of the NDP the federal parties may polarize more along these than class lines. It is essential to bear this in mind and to include these factors in our analyses to understand how they impinge on the relationship between class and politics. One point to note with respect to voting is that only three parties—the Liberals, the Conservatives and the NDP—are considered here. This was done for two reasons: (1) a sizeable number of Canadian voting studies (see Clarke et al. for a review) limit their analysis to these three as the only true national parties, the Social Credit running candidates in few constituencies outside of Quebec; and (2) there were too few Social Credit voters for some of the analytical techniques.

There are two major hypotheses of this study:

1. *Members of the traditional and the new working classes will be more likely to vote NDP, or not to vote, than members of the other classes; and*
2. *There will be a statistical interaction among class, viability of the NDP, and voting.*

The primary analytical technique for testing them is multiple discriminant analysis. Voting is the dependent variable and the independent variables are those listed in Table 1. The technique both tests for differences, if any, among the three parties on the independent variables and assesses their importance and characterizes them (Eisenbeis and Avery; Huberty). Discriminant functions are weighted composites of the independent variables, constructed to maximize the differences among groups defined by the dependent variable. Each independent variable has a weight on each function which indicates how it is related to that function. The weights are similar to partial regression coefficients, representing "the effect of a variable *net* of all other variables, and unlike factor loadings, they do not reflect the common aspects among the independent variables" (Zipp, a, 260). Weighted means—termed centroids—for each dependent group can be obtained by adding the products of each coefficient and the within-group mean for the appropriate variable. Centroids uncover the relationships among the dependent groups and between each group and the function. In general, they have a range of  $-3$  to  $+3$  and they express the relationship (1) among the dependent groups; and (2) of each dependent group to the discriminant function. If the first function does not account for all of the between-group variation, successive orthogonal functions are delineated. The number of functions derived is equal to whichever is the lesser, (1) the number of independent variables or (2) one less than the number of dependent groups.

Discriminant functions are unobservable constructs and are to be interpreted in terms of the independent variables which weight heavily on them. The optimal method for testing the significance of individual independent variables has been suggested by Eisenbeis and Avery. They recommend re-running the analysis, sequentially omitting a different independent variable each time, and then comparing the resultant chi-squares for the two equations. These differences in chi-square are distributed as chi-square with the appropriate difference degrees of freedom. If the absence of a variable from the equation results in a significant decrement in chi-square, then that variable is statistically significant. (Further explication and an example of discriminant analysis can be found in Zipp, a.)

## Results

### A. VOTING

The results of the discriminant analysis of class and voting for the three major federal parties are listed in Table 2. The results for an analysis including only class (column 1) indicate that social class is significantly related to voting, primarily through the effects of being in the traditional

**Table 2.** SOCIAL CLASS AND VOTING UNSTANDARDIZED DISCRIMINANT COEFFICIENTS AND GROUP CENTROIDS

	First Function			Second Function		
	(1A)	(2A)	(3A)	(1B)	(2B)	(3B)
Trad. working	-2.46*	.38	.35	.49	-.45	.09
New working	-2.89*	.45*	.60	-2.12	-1.04*	-.68
S/F class	-1.03	.06	.06	-1.83	-.62	-.62
Owners	-.74	.20	.20	.86	.06	.01
Viability		.53***	.54		-1.12*	.25
Maritime		-.19	-.18		.56	.44
Quebec		.81*	.81*		-.11	-.08
Prairies		-.67**	-.69**		.44	.44
Brit. Columbia		-.55	-.57		.35	.22
Ethnicity		-.33	-.33		-.49	-.42
Main. Prot.		-.31	-.30		.12	.07
Catholic		.88**	.87**		-.00	-.01
No religion		.15	.14		-1.19	-1.09
Sex		-.31	-.30*		-.56*	-.50
Union		-.03***	-.03***		-1.23***	-1.10***
Trad. working x viability			.13			-1.94**
New working x viability			-.70			-1.12
Constant	1.90	-.28	-.29	.08	1.76	1.29
Canonical r	.11	.40	.40	.06	.24	.26
Relative %	76.91	75.44	72.46	23.09	24.56	27.54
Prob.	.044	.000	.000	.30	.000	.000
<u>Groups/Centroids</u>						
Liberals	-.05	.33	.33	.05	.08	.09
Conservatives	.17	-.57	-.57	-.02	.14	.15
NDP	-.13	-.11	-.10	-.13	-.57	-.62

Relative % is obtained by dividing the eigenvalue associated with each function by the sum of all eigenvalues.

\*p less than .05

\*\*p less than .01

\*\*\*p less than .001

(-2.46) or the new working class. A single function separated the Conservatives (.17) from the Liberals (-.05) and the NDP (-.13). Members of the traditional and new working classes are more likely to vote NDP or Liberal and/or less likely to vote Conservative. Furthermore, considering the relative position of the group centroids, the NDP is furthest "left," the

Liberals are approximately at the "center," and the Conservatives are off to the "right."

The addition of the other main effects (column 2) produced two discriminant functions, the first dividing the Liberals (.33) from the NDP (−.11) and the Conservatives (−.57) and the second contrasting the NDP (−.57) with the Liberals (.08) and the Conservatives (.14). These variables reduce but do not negate the class effects. Although the effect of the traditional working class is no longer statistically significant, the effect of the new working class (.45 on the first function; −1.04 on the second) continues to be so. In addition to the new working class, the first function exhibits the effects of viability (.53), Quebec (.81), the Prairies (−.67), Catholic (−.88), sex (−.31), and union membership (−.03). Those indicate not only that Quebecers and Catholics are more likely to vote Liberal and/or less likely to vote Conservative or NDP but also and somewhat surprisingly that members of the new working class and those living in ridings where the NDP is viable are likely to do so. The opposite is true for men, union members, and Prairie voters: they are more likely to vote NDP or Conservative and/or less likely to vote Liberal.

A grouping of the NDP and the Conservatives makes some of the results difficult to interpret, although we need to bear in mind that the NDP is equidistant from the Conservatives and the Liberals. For example, Prairie residents are more likely to vote Conservative or NDP and/or less likely to vote Liberal. Because the Prairie effect is a deviation from the Ontario effect, comparisons of the mean levels of support for each party in these two regions may clarify the interpretation. The two regions are quite similar in the extent of support of the NDP: 17 percent of Ontario and 15 percent of the Prairie voters voted NDP in 1974. However, voters in the two regions differ sharply in their support for the major parties: 51 percent of the Ontario electorate voted Liberal and only 33 percent Conservative, but only 40 percent of the Prairie voters cast their ballots for the Liberals while 45 percent supported the Tories. This indicates that Prairie residents are less likely to vote Liberal, more likely to vote Conservative but no more likely to vote NDP than voters in Ontario. In contrast, those living in Quebec are more prone to vote Liberal and less likely to vote Conservative or NDP. Members of the new working class and/or voters in constituencies where the NDP is viable are much less likely to vote Conservative. Union members are more likely to vote NDP and men are more likely to vote either NDP or Conservative and less likely to vote Liberal.

One aspect of the results that must be emphasized is that class divided the parties as hypothesized; only the inclusion of the other social and demographic variables revealed this unexpected party grouping. Thus, social class separated the Conservatives, a right-wing party on both social and economic issues (Ogmundson, a) from the Liberals, who are to the left socially but to the right economically and the NDP, who are to the left on

both types of issues. The other social and demographic factors altered this by separating the Conservatives and the NDP from the Liberals. Although at first this may seem odd, it reflects other known features of Canadian politics that may make the outcome more realistic. Three important factors affect voting choice in Canada, religion, region, and ethnicity; and all three would partition the parties in this way. The most salient cleavages, Catholic-Protestant, Quebec-English Canada, and French-English, all divide the Liberals from the other two parties. The fact that this type of party split is reinforced several times may explain why the parties are grouped in this manner.

Four variables in concert account for an additional 25 percent of the variance in the matrix through the second function (Table 2); new working class ( $-1.04$ ), viability ( $-1.12$ ), sex ( $-.56$ ), and union membership ( $-1.23$ ). They separate the NDP ( $-.57$ ) from the liberals ( $.08$ ) and the Conservatives ( $.14$ ). Members of the new working class, those living in ridings where the NDP is viable, men, and union members all are more given to voting NDP and/or less given to voting for either of the major parties—results much more in keeping with expectations.

The results in Table 2, column 3, indicate that there is a significant statistical interaction among class, voting, and the viability of the NDP and thus confirms one of the major arguments of this research. Two functions are delineated: the first accounts for 72 percent of the variance in the matrix and contrasts the Liberals ( $.33$ ) with the Conservatives ( $-.57$ ) and the NDP ( $-.10$ ), while the second, responsible for an additional 28 percent of the discriminating ability of the independent variables, distinguishes the NDP ( $-.62$ ) from the Liberals ( $.09$ ) and the Conservatives ( $.50$ ), and, again on the first function, the NDP is almost equidistant from the Liberals and the Conservatives.

The first function is very similar to the first function for the additive model. Quebec, the Prairies, Catholic, sex, and union membership are related to voting in the same way in each, and the substance of these relationships will not be restated. Viability and membership in the new working class are no longer significantly related to voting, but their place is taken by the traditional working class  $\times$  viability interaction term ( $.13$ ). The positive sign for this term is unexpected; it indicates that, net of all else, being in this cell is positively related to support for the Liberals and/or negatively related to support for the NDP or the Conservatives. It was expected that those in this cell would be more likely to vote NDP, and although their bivariate correlations support this, the discriminant analysis does not. Two pertinent observations may be offered in this regard: first, since being in this cell is related very weakly to Liberal, and much more strongly (negatively) to Conservative support, the most plausible interpretation may be that this result reflects the fact that being in this cell is negatively related to voting Conservative. Second, a discriminant analysis

of voting only on class, viability, and their interactions produced one function which separated the NDP from the major parties and on which the traditional working class  $\times$  viability interaction was positively related to support for the NDP (results not reported). The inclusion of the other variables, which are related more strongly to voting choice, has relegated this relationship to a secondary position.

This is corroborated by the composition of the second function on which union members ( $-1.10$ ) and those members of the traditional working class who live in constituencies where the NDP is viable ( $-1.94$ ) are more likely to vote NDP and/or less likely to vote Liberal or Conservative. These latter results are much more in accord with expectations. Taken as a whole, these analyses do not imply that class is unimportant in politics; rather, they specify the nature of that relationship. Perhaps the reason for its secondary effect on voting is that the parties have polarized on religious, regional, and ethnic, but not class, lines. Certainly, this and the interaction among class, viability and voting indicates that class is important in electoral choice but that its importance has been minimized by the political system.

#### B. NONVOTING

If, as argued, nonvoting by members of the working class also indicates an awareness of class position, then its meaning would be similar to voting for the NDP. Indeed, it might be argued that they are complementary phenomena. It is enlightening to examine class and nonvoting in view of this possibility. First, members of the new working class have the highest levels of nonvoting (22.3 percent), and the traditional working class (19.0 percent) slightly less.<sup>11</sup> This is in accord with the possibility that nonvoting is a response similar to voting NDP. A further step in the analysis involves splitting the sample by viability of NDP candidacy-in the constituency; if nonvoting and voting NDP are complementary, nonvoting should increase and voting NDP should decrease in nonviable constituencies and the reverse would occur in ridings where the NDP is viable. Indeed, this proves to be the case. In nonviable constituencies, NDP voting falls from 11.4 percent to 7.7 percent for the traditional working class and from 15.7 percent to 12.8 percent for the new working class; nonvoting rises from 19.0 percent to 19.3 percent and from 22.3 percent to 23.7 percent for the traditional and the new working classes, respectively. In ridings where the NDP is viable, just the reverse occurs: NDP voting increases from 11.4 percent to 22.1 percent (traditional working class) and from 15.7 percent to 28.2 percent (new working class); nonvoting falls for each, 19.0 percent to 18.1 percent and 22.3 percent to 16.1 percent for the traditional and the new working classes, respectively.

A further indication that nonvoting may be a response functionally



equivalent to voting NDP is provided by the results of regressing voting for the NDP as opposed to nonvoting on social class. They show (not reported) that class is not significantly related to the dependent variable, indicating that class could not distinguish between NDP voters and nonvoters. *Thus, it appears that nonvoting may be one class-related response to a political order in which one's interests are not being represented.*

Finally, nonvoters were included as a dependent group (along with the three parties) and the discriminant analyses were re-estimated (Table 3). In the analysis of class only, one significant function emerged, and it grouped the NDP ( $-.10$ ) and nonvoters ( $-.21$ ) against the Liberals ( $.01$ ) and the Conservatives ( $.18$ ), largely on the basis of the traditional ( $-2.60$ ) and the new working classes ( $-3.31$ ). Members of these classes are more likely to be NDP voters or nonvoters and/or less likely to vote for either of the major parties. This lends further support to the plausibility of similar interpretations of nonvoting and voting NDP.

Three functions emerged for the discriminant analysis of the four groups on the full additive model. Since the first two account for almost 85 percent of the discriminating ability of the independent variables, these two dimensions represent the configuration among the groups accurately. The first function groups the Liberals ( $-.27$ ) and nonvoters ( $-.14$ ) against the NDP ( $.08$ ) and the Conservatives ( $.55$ ), while the second separates the Liberals ( $-.13$ ) and the Conservatives ( $-.13$ ) from nonvoters ( $.13$ ) and the NDP ( $.55$ ).

Class retains a significant effect on voting after controlling for these additional factors, with the traditional ( $-.44$ ) and the new working classes ( $-.55$ ) both deviating significantly from managers on the first function. In addition, this function reflects the effects of viability ( $-.58$ ), living in Quebec ( $-.76$ ) or the Prairies ( $.54$ ), being a Catholic ( $-1.07$ ) or a union member ( $-.03$ ). These effects are not easy to interpret, given the grouping of the dependent variables (Liberals and nonvoters vs. NDP and Conservatives), though following the logic of interpreting the class and voting results for the three parties only, the primary force of these variables seems to be separate the Liberals and the Conservatives (and their centroids are farthest apart), with the NDP and nonvoters toward the center and each other. Therefore, members of the traditional and the new working classes and of unions are less likely to vote Conservative and/or more likely not to vote, while Catholics and Quebeckers are more likely to vote Liberal and/or less likely to vote Conservative, with residents of the Prairies more apt to be Conservative supporters and/or less apt to vote Liberal.

Both the traditional ( $.71$ ) and the new working classes ( $.36$ ) continue to show significant effects on the second function, along with viability ( $.97$ ), living in Quebec ( $.37$ ) or the Prairies ( $.12$ ), and being a union member ( $1.20$ ). Those in the traditional and the new working classes, those living in viable constituencies, or union members are more likely to vote NDP or not

**Table 3.** SOCIAL CLASS AND VOTING. NONVOTING UNSTANDARDIZED DISCRIMINANT COEFFICIENTS AND GROUP CENTROIDS

	First Function		Second Function	
	(1A)	(2A)	(1B)	(2B)
Trad. working	-2.60*	-.44*	.69	.71*
New working	-3.31*	-.55**	-1.81	1.36**
S/F class	-1.42	-.13	-1.71	.78
Owners	-1.69	-.16	1.13	.36
Viability		-.58***		.97**
Maritime		.28		-.52
Quebec		-.76**		.37*
Prairies		.54***		.12***
Brit. Columbia		.57		-.03
Ethnicity		.38		.57
Main. Prot.		.14		-.12
Catholic		-1.01**		-.12
No religion		-.45		1.17
Sex		.28		.53
Union		-.03***		1.21***
Constant	2.17	.49	-.10	-2.15
Canonical r	.13	.34	.05	.23
Relative %	82.39	59.80	10.89	25.09
Prob.	.005	.000	.545	.000
<u>Groups/Centroids</u>				
Liberals	.01	-.27	.04	-.13
Conservatives	.18	.55	-.02	-.13
NDP	-.10	.08	-.12	.55
Non-voters	-.21	-.14	.02	.13

Relative % is obtained by dividing the eigenvalue associated with each function by the sum of all eigenvalues.

\*p less than .05

\*\*p less than .01

\*\*\*p less than .001

vote and/or less likely to vote for either of the major parties. Quebeckers and Prairie residents are more likely not to vote and/or less likely to vote Conservative (in the case of Quebeckers) or Liberal (Prairie residents). These patterns lend further support to an *interpretation of nonvoting as a class-related response*.

## Summary and Discussion

The purpose of this research was to assess the influence of social class in voting in Canada by using a Marxian class scheme and by controlling for the viability of the NDP in the various constituencies. Our central findings are:

1. When class alone is analyzed, members of the traditional and the new working class are more likely to vote for the NDP or the Liberals and/or less likely to vote for the Conservatives.
2. In the full additive model, only the new working class is significantly related to voting. Members of this class are more likely to vote for the NDP or the Liberals and/or less likely to vote Conservative.
3. There is an interaction between class, the presence of a viable NDP candidate and voting. Members of the traditional working class who live in ridings where the NDP is viable are less likely to vote Conservative and more likely to vote NDP.
4. In terms of class, nonvoters are more similar to NDP voters than to major party voters.

In sum, these results indicate that in Canada class is related to voting, net of several factors usually associated with voting. Although the class effects are not overwhelming, they are present and they persist in addition to the influence of region, religion, ethnicity, and so on. Furthermore, the significance of the interaction between class and viability illustrates the fact that the relationship between class and politics is affected by the presence or absence of an outlet for leftist voting. One thing is certain: Canada is not the bastion of pure non-class politics. The purported weakness of the influence of class on politics and the seemingly low levels of class voting found previously in Canada may be explained partially by the statistical interaction among class, voting, and the viability of the NDP. These had been attributed to a presumed disinterest in class issues and an absence of class consciousness among Canadians. Regional, religious, and ethnic loyalties are said to have prevented the development of class ties, rendering them insignificant as a source of political cleavage. Our results indicate that the reason does not lie totally with individual Canadian citizens, for class is related more strongly to voting where the citizen is presented with a viable outlet for the expression of class sentiments. Although the availability of the option will not mean higher levels of class voting automatically, it does emphasize the need to consider the options that are available when assessing the level of class politics (see Przeworski and Sprague).

The importance of establishing an available appropriate option in assessing the extent of class politics raises several other important issues.

First, we need to consider the role of the State and of political parties in class politics and in the class struggle. Although Marx never completed a full analysis of the State, neo-Marxists have provided a variety of analyses of its impact. Without entering into an extensive discussion of their differences (e.g., Miliband; O'Connor; Poulantzas), all agree that the State is an active participant in and sharper of the nature of class politics. A similar argument can be advanced for political parties; to a varying extent, they are independent agents which, within limits, condition the relationship between social class and electoral politics. The programs and policies that a leftist party espouses and the strategies that it employs all affect the possibilities for heightened class awareness among the electorate.<sup>12</sup> Investigators need to pay more attention to these concerns, especially to the part played by political parties, in order to understand the relationship between class and electoral politics.

A second, related, issue—Why are class-related options not always present?—requires consideration of structural conduciveness to the emergence and success of leftist parties. In the Canadian case, despite the facilitating setting of a parliamentary system (as opposed, for example, to a presidential one), there are significant impediments to the viability of leftist (or other third) parties at the federal level. We already have noted how certain features of the electoral system exaggerate support for the major parties at the expense of the NDP. In addition, several other aspects of Canadian politics have hindered the success of the NDP. First, like leftist parties in other countries, the NDP has been torn by an internal struggle over broadening the appeal of the party to achieve electoral success as against retaining ideological purity. It appears that, in many respects, the former view has prevailed, an outcome (Przeworski and Sprague) that is extremely important in accounting for its lesser viability as a left option. Using a somewhat narrow definition of the working class, Przeworski and Sprague argued that socialist parties cannot achieve electoral success by appealing only to the working class, but that attempts to expand the bases of their support diminish their attraction to the working class. How far toward the center a leftist party can move and still offer a viable leftist alternative is a question whose answer will vary from case to case. In Canada, two additional considerations are pertinent.

Second, throughout the (CCF) NDP's existence, the Liberals have co-opted their programs and officials. For example, federal Liberal governments have used the NDP-CCF provincial health system in Saskatchewan as the model for their national health system. They have taken credit for both raising the level of old age pensions and making them portable across provincial lines—two changes for which the NDP had long lobbied. Indeed, now deceased Liberal Prime Minister Louis St. Laurent is reputed to have said that members of the NDP were only Liberals in a hurry. This obviously has led to the weakening of the NDP's position and to the

perception of the NDP as a realistic alternative to the Liberals. Third, along similar lines, the recent resurgence of the Conservative party (in the 1979 federal election) has revitalized the two party system. With the Conservatives perceived as a real threat, many voters may turn to the Liberals instead of the NDP in order to prevent the Tories from forming the government. This, in part, illustrates how the viability of the NDP is determined by its electoral appeal and vice versa. All these factors, many of them shared by leftist parties elsewhere, condition the existence of class-related options in the electoral sphere.

Analogous considerations apply to both the way in which class will be manifested in behavior and attitudes and the classification of citizens into classes. Social class is a dynamic concept which reflects differences in historical conditions. At any one time the meaning with which class invests situations interacts with these cumulated historical experiences. For instance, the terms "new" and "traditional" as applied to the working class refer as much to aspects of the development of capitalist economies as to specific differences in the relations of production associated with those positions. The expansive nature of industrial and early monopoly capitalism provided for the subsistence needs of the traditional working class and contributed to a greater acceptance of established political organizations than seemed implied by a liberal reading of Marx and Engels (cf. Bottomore). In contrast, a key datum for new working class theory is that these positions have been expanding in a period when the contradictions in monopoly capitalism increasingly have been felt by these workers. The fact that such different experiences become entrenched is what we have in mind when we refer to the importance of historical context in interpreting the meaning of class. Historical factors like these along with the actions of groups (e.g., political parties, unions) to mobilize individuals may help to explain the manner in which class retains or loses significance in interpreting political alternatives.

In summary, to ascertain the extent and changes in the extent of class politics in various societies at different times will require much more attention than has been given heretofore to constructing appropriate indices of the composition of classes and the availability of meaningful class-related outlets. Both change. Moreover, such analyses must be multivariate and control for other important factors that impinge on political actions, particularly the influence of friends and family members. Finally, even though, in such countries as Canada and the United States, voting and similar political actions involve psychological factors such as party identification, analyses also must be fundamentally structural to permit the assessment of the possibilities for expressing a class-related political choice.

## Notes

1. Ogmundson (c) also used subjective class identification.
2. The term, new working class, has been used in various manners. Miller and Riessman, and Giddens used it to refer to the underclass, or the very poor, marginal working class. Goldthorpe and his colleagues (a,b) employed it to denote affluent blue collar workers. Oppenheimer used it to correspond to the skilled sector of white collar society. None of these uses, however, is consistent with this use of the term, which generally reflects those of Mallet, Gorz (a,b), James Wright, and several others.
3. Quebec is not the only province ever to entertain separatist desires, Newfoundland and the Western provinces, particularly Alberta, have done the same to a somewhat lesser degree.
4. A more elaborate description, including the exact questions used, is found in Zipp (b).
5. It would have been preferable to distinguish capitalists from petty bourgeoisie but the sample design and questions did not permit this. First, large capitalists are not likely to be included to any significant degree, if at all, in a national survey, so this class position is virtually eliminated at the start. Thus, the best that could be done was to distinguish small capitalists from petty bourgeoisie, a distinction, that while clear on the analytical level, becomes fuzzy when one starts to sort people into class positions. Therefore, it seemed reasonable to merge the petty bourgeoisie and small capitalists together into a class of "owners."
6. Detailed occupational descriptions were obtained from the Blishen codes. Respondents were classified as being in the supervisory structure if their job entailed issuing orders to others, as a question was included to measure this.
7. Although it would have been desirable to distinguish the different managerial levels as noted earlier, this was not possible with these data. As such, all levels within the managerial hierarchy—except for supervisors/foremen—were grouped into one class.
8. Although this class scheme may rely too heavily on occupation, it was constructed in this fashion out of necessity. Separate analyses bear on this point. First, our use of class was compared with a seven-fold occupational classification and found not to be equivalent to occupation. Second, class and a manual-nonmanual occupational dichotomy were used to explain income inequality and class was by far the superior indicator (Zipp, a).
9. This index of viability appears static, for it fails to suggest how a party may become viable in a particular constituency. We do not wish to imply that change is impossible, that a nonviable party must always remain so, for this certainly is not true. Generally, there are five conditions for change: (1) the party switches resources from one constituency to another, (2) the population make-up of the constituency changes, (3) there is a shift in issues, (4) there is a change in candidates, or (5) there is a change in constituency boundaries. Although our measure cannot deal with these, we still believe that it is a useful index for characterizing a large set of constituencies in a particular election. As with all measures, it only can approximate reality and the goal always is to reduce the width of approximation.
10. All the analyses were duplicated with viability measured as a ratio scale representing the proportion of the vote obtained by the NDP. In general, these results were very similar and only the results for the dichotomous coding of viability are reported here.
11. Owners (19.7 percent) have a slightly higher level of nonvoting than the traditional working class.
12. Miliband pointed out that the problems for leftist parties do not end once they attain office. Because they are more likely to come to power in hard times, they often have to make major concessions to begin to improve their country's situation. These often require appeasing major economic powerholders and, as a result, many of the programs which the party may have pledged are not implemented.

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# Fear of Crime as a Social Fact\*

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## ABSTRACT

*Fear of crime has emerged as a significant issue. Much research has investigated the extent and distribution of fear across such social statuses as age, sex, class, and race. Our research takes a different direction: it treats fear as a social fact which varies across sites and situations—fear between cities and the structural characteristics of cities which influence it, such as crime rates, the proportion of crime which is interracial, racial composition (percent non-white and segregation), and population size. Using data from the National Crime Survey (NCS) and various other sources, we regress fear of crime on these structural characteristics for 26 cities. For whites the analysis suggests that fear is affected by property crime rates and the proportion of crime which is interracial, and that racial composition indirectly affects fear by strongly influencing the proportion of crime which is interracial. For nonwhites the analysis suggests that fear is also affected by racial composition, but not by crime rates or the proportion of crime which is interracial. The paper explores the meaning of these findings for a structural theory of the fear of crime.*

Over the last fifteen years fear of crime has emerged as an important research topic. Surveys—Harris, Gallop, National Opinion Research Center (NORC), and the National Crime Survey (NCS)—report that a very high percentage of the U.S. population fear criminal victimization (Baumer; Boggs; Braungart et al.; Clemente and Kleiman; Garofalo, a, b; Goldsmith and Tomas; Hartnagel; Shotland) and that this percentage may have increased substantially since the mid 1960s (Erskine). Some studies suggest that fear of crime may not be proportionate to the objective chances of being victimized. For example, since the 1960s fear of crime seems to have increased considerably faster than the crime rate (Garofalo); people fear personal more than property crime, although property crime rates are much

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higher than personal crime rates; and the social categories which fear crime the most (females and the elderly) are those least likely to be victimized (Balkin; Clemente and Kleiman; Garofalo). Yet, as W. I. Thomas pointed out, situations people perceive as real are real in their consequences.

What are the consequences of the fear of crime? Research suggests that fear of crime can lead to deleterious psychological effects (such as feelings of anxiety, mistrust, alienation, and dissatisfaction with life) and to efforts to reduce fear (e.g., taking drugs), to avoid victimization (e.g., staying off the streets at night, avoiding strangers, and curtailing social activities), and to protect oneself (e.g., buying watch dogs, firearms, anti-burglary equipment, insurance, and learning self-defense). (See Balkin; Braungart et al.; Clemente and Kleiman; Conklin; Garofalo, a; Goldsmith and Tomas; Shotland.) Garofalo, for example, reports a gamma of .55 between limiting one's social life and fear of crime.

What do we know about the fear of crime? In 1967 the Presidential Commission on Law and Enforcement decried the lack of research. Writing seven years later Brooks stated that the situation had not changed; and today we still know very little. Most contemporary studies attempt to link fear of crime to individual social and demographic variables, such as sex, age, race, marital status, education, and income (Baumer). The best of these use multivariate statistical techniques and large samples, frequently analyzing NORC and NCS data (e.g., Braungart et al.; Clemente and Kleiman; Garofalo, a, b).

These studies are useful and should be continued. Our research, however, takes a different tack. It treats fear as a social fact which varies across sites and situations. The NCS shows that fear of crime varies substantially between cities. For example, in San Diego 27 percent of the population feel unsafe in their neighborhood at night, whereas in Newark 58 percent feel that way. Our research examines the structural characteristics of cities which influence this variation.

## Pertinent Explanatory Variables

### CRIME RATES

It seems reasonable to suppose that variation in fear of crime between cities should reflect, in part, variation in the crime rate between cities. In measuring the crime rate we used official rates, rather than victimization studies. Although the latter may more accurately reflect underlying crime rates than do the former, research (Conklin) suggests that people form their images of the crime problem from mass media reports, which are largely based on official statistics. Further, the correlation across cities between victimization rates and fear is substantially less than the correlation be-

tween official crime rates and fear (Garofalo and Laub). Hence, the crime rate, although partially a product of social construction, is a social fact and is probably a major cause of variation between cities in the magnitude of the publicly recognized crime problem.

#### INTERRACIAL CRIME

Crime between culturally dissimilar people may be more threatening and fear producing than crime between culturally similar people; for the former is associated with greater uncertainty, unpredictability, and danger, than the latter. In the United States, race differences in socialization are a major source of cultural dissimilarity, and nonwhites are associated with dangerous street crime in popular belief systems and stereotypes (Swigert and Farrell). Hence, particularly for whites, fear of crime may be linked to interracial crime; and variation between cities in fear of crime may reflect variation in interracial crime between cities.

#### RACIAL COMPOSITION (PERCENT NONWHITE AND SEGREGATION)

The racial composition of a city, by affecting the level of interracial crime and people's perception of their chances of being victimized, may affect the fear of crime. For whites, as the percentage of nonwhites increases, the level of interracial crime probably increases; additionally, some research (Lizotte and Bordua) suggests that the presence of nonwhites is used by whites to infer their chances of being victimized. Segregation, by reducing the daily visibility of nonwhites to whites as well as the interaction between whites and nonwhites, probably reduces interracial crime and whites' perception of their chances of being victimized; hence, it should reduce the fear of crime among whites. For nonwhites the implications of racial composition are less clear. Nonwhites may not feel threatened by interracial crime because a very small percentage of nonwhite victimizations is interracial. To the contrary, as suggested by Lizotte and Bordua, nonwhites' perception of their chances of being victimized may be influenced, not by the presence of whites, but by the presence of nonwhites, because they too may be influenced by cultural beliefs and stereotypes which link dangerous street crime to nonwhites. In fact, neighborhoods, as well as cities, may develop a reputation as crime ridden and unsafe to the extent to which they are predominantly nonwhite. Segregation by increasing intraracial interaction may then increase fear among nonwhites. Hence, we hypothesize that for whites, variation in the fear of crime between cities is positively affected by percent nonwhite and negatively affected by segregation and that for nonwhites, variation in the fear of crime is positively affected by both percent nonwhite and segregation.

## POPULATION SIZE

The urbanism thesis, developed by Wirth and critically evaluated by Fischer, suggests that large cities are associated with normative conflict, individualism, depersonalization, breakdowns in both primary relationships and informal social controls, and geographical mobility, which lead to harmful psychological states such as alienation, powerlessness, anxiety, and fear. Garofalo and Laub suggest that fear of crime may be part of this psychological syndrome which is linked to problems in contemporary urban living.

In sum, we argue that a somewhat different causal structure underlies variation in fear of crime between cities for whites and nonwhites. For whites we expect that variation in fear between cities will be affected by population size, crime rates, percent nonwhite, and segregation (negatively), and that percent nonwhite and segregation affect fear by influencing interracial crime and the perception of the chances of being victimized. For nonwhites we anticipate that variation among cities in fear will be affected by population size, crime rates, percent nonwhite, and segregation (positively), and that percent nonwhite and segregation will affect fear by influencing the perception of the chances of being victimized. The following equations will be estimated for white and nonwhites.

$$\text{Fear} = b_1CR + b_2\%NW = b_3S + b_4P \quad (1)$$

$$\text{Fear} = b_1CR + b_2\%NW + b_3S + b_4P + b_5IRC \quad (2)$$

where  $CR$  = crime rates,  $\%NW$  = percent nonwhite,  $S$  = segregation,  $P$  = population size, and  $IRC$  = interracial crime. Two equations are estimated (one including  $IRC$ ) in order to examine the extent to which interracial crime mediates the effects of percent nonwhite and segregation on the fear of crime.

## Methods

The equations are estimated using data from the NCS, sponsored and directed by the Bureau of the Census. The survey was conducted during the early and middle 1970s and includes about 10,000 respondents 16 years of age and older from each of 26 cities in the United States.<sup>1</sup> While the number of cities is not large, it is the largest sample of cities for which fear of crime data are available.

Fear of crime was measured by the following item: "How safe do you feel or would you feel being out alone in your neighborhood at night (during the day)?" Responses were coded as very safe, safe, unsafe, and very unsafe. For purposes of this analysis, cities are ordered by the percen-

tage feeling either unsafe or very unsafe, and following the literature (see Baumer), we examined fear at night rather than during the day. Although the term, crime, is not included in the question, the item was preceded by questions on crime trends, which were introduced with the phrase, "Now I'd like to get your opinion about crime in general." It is unlikely, therefore, that respondents were thinking of dangers other than crime in answering the questions.

Data on the causal variables were collected from various sources. Recorded crime rates were obtained from the *Uniform Crime Reports*. So as to emphasize serious crimes, only the seven index crimes were used. Population size and percent nonwhite were taken from the U.S. Census. And city segregation for 1970 was taken from Sorenson et al. The index measures the dissimilarity between distributions of nonwhite and white households among city blocks, that is, the extent to which the racial composition of city blocks reflects the racial composition of the city as a whole.

Two indexes of interracial crime were computed from NCS data on robbery: the proportion of white victimizations which is interracial and the proportion of nonwhite victimizations which is interracial.<sup>2</sup> For the other three property crimes (larceny, burglary, and auto theft) the race of the offender is generally unknown to the victim. For the three personal crimes (homicide, assault, and rape) the rate of interracial crime is quite low, and the victim and offender often know each other as acquaintances or friends (Ferracuti and Newman). These are not the type of victimizations which engender fear to walk the streets. Additionally, homicide is not included in the NCS. Robbery, however, is the epitome of dangerous street crime in which the offender is a stranger whose racial identity is generally known to the victim.

## Results

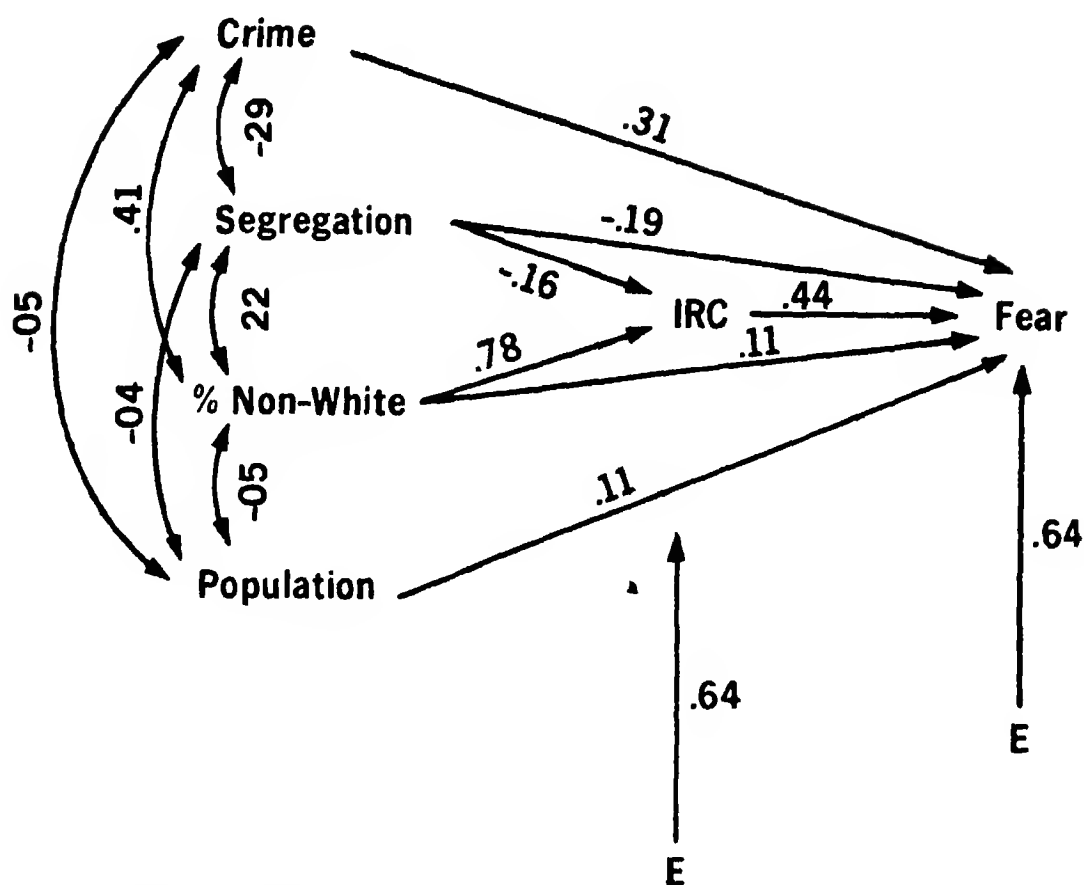
The data are analyzed separately for whites and nonwhites.<sup>3</sup> Ordinary least-squares regression is used to estimate the equations. Standardized coefficients (betas) are used to compare the relative effects of the causal variables for each subsample and unstandardized coefficients (bs) are used to compare the effects of the variables between the subsamples.

The findings for whites are presented in the first panel of Table 1. The first column shows the correlations between fear and the causal variables; and the second and third columns show the bs and betas, respectively, for the causal variables, excluding interracial victimization. (Multicollinearity is not a problem in estimating equation one; the strongest correlation between the causal variables is .41 between the crime rate and percent nonwhite. See Figure 1.) The estimates for equation (1) (columns 2 and 3) suggest that percent nonwhite, segregation, population, and recorded

Table 1.

	Fear of Crime									
	Whites					Nonwhites				
	Not Including IRC		Including IRC			Not Including IRC		Including IRC		
	r	b	beta	b	beta	r	b	beta	b	beta
% Nonwhite	.52	.18	.47	.04	.11	.50	.21	.43	.23	.46
Segregation	-.26	-.17	-.27	-.12	-.19	.40	.28	.32	.30	.35
Population	.17	.0008	.20	.0004	.11	.24	.001	.29	.001	.30
Crime rate	.56	.10	.29	.11	.31	.09	.01	.02	.01	.02
IRC*	.65			22.61	.44	-.45			6.26	.05
R <sup>2</sup>		(52)			(59)		(42)			(42)

\*For whites IRC refers to the proportion of white victimizations which is interracial, and for nonwhites IRC refers to the proportion of nonwhite victimization which is interracial.



E = residual effects

Figure 1. PATH DIAGRAM OF FEAR-WHITES

crime rates affect fear in the predicted direction, explaining 52 percent of the variance in fear between cities.<sup>4</sup>

To distinguish the effects of reported property and personal crime, the seven index crimes were sorted into personal crimes (homicide, assault, and rape) and property crimes (robbery, burglary, larceny over \$50, and auto theft) and the equations were reestimated.<sup>5</sup> Only property crime rates affect fear. With both indexes in the equation, property crime shows a beta of .52, personal crime shows a beta of  $-.29$ , and the betas of the other causal variables remain approximately the same. Inspection of the correlations and stepwise regression analyses of fear on the four property crimes and on all seven crimes suggests that the property crime effect reflects mainly the effect of robbery.

As shown in columns four and five interracial crime (the proportion of white victimizations which is interracial) also has a substantial effect on fear. Including it in the equation increases the explained variance of fear to 59 percent and substantially reduces the direct effects of percent nonwhite and segregation on fear, suggesting that these effects are mediated by interracial crime.



In sum, the path model in Figure 1 shows that variation in whites' fear of crime between cities is mainly a function of crime rates (particularly robbery), and the proportion of white victimizations (robbery) which is interracial; and shows that the latter in turn is affected by the racial composition of a city (percent nonwhite and segregation). We interpret these relationships as suggesting that cultural dissimilarity between whites and nonwhites makes interracial crime appear particularly uncertain, violent, and dangerous to whites.

The findings for nonwhites are presented in the second panel of Table 1. The betas for equation (1) (Table 1, column 8) show that fear of crime is affected by percent nonwhite, segregation, and population size. (Note that the effect of segregation is positive for whites and negative for nonwhites.) Crime rates, however, show no effect. Categorizing crime rates under *personal* and *property* does not change these estimates; the betas for both are under .05.<sup>6</sup> Adding interracial crime (the proportion of nonwhite victimizations which is interracial) to the equation (Table 1, columns 9 and 10) does not increase the  $R^2$ ; it shows no effect on fear (beta = .05) and does not mediate the effect of racial composition on fear.

## Discussion

Fear of crime is an important social problem. Extant research suggests that it has increased since the mid 1960s, that it affects people's psychological dispositions and social behavior, and that it is linked to individual socio-demographic characteristics. This study treats fear as a social fact and investigates the structural characteristics of cities which influence variation in fear between cities.

Perhaps the most interesting findings of this research are that the fear of whites and nonwhites is influenced by *different* structural characteristics of cities, and that the racial composition of cities influences the fear of both whites and nonwhites. For whites fear of crime is directly affected by property crime rates and the proportion of white victimizations which is interracial; percent nonwhite and segregation are important structural characteristics because they influence the level of interracial crime. The model explains 59 percent of the variance in fear among cities. For nonwhites, fear of crime is unaffected by crime rates and the proportion of nonwhite victimizations which is interracial; but it is directly affected by population size, segregation and percent nonwhite. The total model explains 42 percent of the variance in fear among cities, considerably less than that explained for whites.

This difference between the explained variance of whites' and nonwhites' fear is accounted for by differences in the effects of the crime rate and the level of interracial crime. First, the observation that the *crime rate*

(as an aggregate index or disaggregated into property and personal crimes) does not affect variation among cities in nonwhites' fear of crime is somewhat of an anomaly. Why is fear of crime among nonwhites unaffected by reported crime rates? Based on some reported research, we initially argued that people's perception of their chances of being victimized is probably more influenced by crime in the media, which reflects reported crime, than by crime in their personal lives. For various reasons this may not be true for nonwhites. The relative influence of news reports and actual victimization on fear of crime may differ according to the amount of crime in the immediate neighborhood. Media reports may have less effect on people residing in high crime neighborhoods (generally nonwhites) because they receive more direct information about crime from their immediate environment, than do people in low crime neighborhoods. Additionally, because of lower social integration and literacy, nonwhites, as compared to whites, may be less influenced and sensitive to news reports, which reflect reported crimes, and may be more influenced by crime in their immediate neighborhood as reflected in their personal lives and the lives of their friends. Second, the proportion of nonwhite victimizations which is interracial may not be a meaningful variable for nonwhites, because the proportion is very low (a city mean of .07) and shows very little variation among cities. Rather the data suggest that the fear of crime among nonwhites is influenced by the presence of other nonwhites.

What underlies the effect of the percent nonwhite on the fear of nonwhites? Drawing on research by Lizotte and Bordua, we have suggested that the cultural beliefs and stereotypes which link nonwhites with street crime may influence nonwhites as well as whites. Thus, nonwhites' perception of their chances of being victimized may be influenced by the racial composition of cities and neighborhoods, which may develop a reputation as crime ridden and unsafe to the extent to which they are predominantly nonwhite. Yet, it may not be the actual presence of nonwhites which is feared, but the presence of large numbers of unruly and uncivil teenagers which frequently characterize nonwhite neighborhoods because of population density, family size, and family disruption. In their study of 4 urban neighborhoods, Lewis and Maxfield report that people are as concerned about social disorder, measured by the presence of unruly teenagers, drugs, vandalism, and unoccupied houses, as they are about index street crimes. Clearly, much more research is necessary on the link between the racial composition of social units and the fear of crime.

Our research takes an initial step. It examines fear as a social fact, rather than as an individual fact. The analysis suggests that the variation in fear, among cities, is substantial, that the variation is strongly influenced by structural characteristics of cities, and that the causal structure underlying the variation for whites and nonwhites is different.

## Notes

1. Fear data for 13 of the 26 cities are available in Garofalo, and fear data for the other 13 cities were kindly supplied by James Garofalo.
2. The index excludes commercial robberies and the few individual robberies where the victim could not identify the race of the offender.
3. In the NCS the category nonwhites is predominantly black, with a small number of Asians, American Indians, etc. Hispanics are classified as whites.
4. To examine the data for non-linear trends, we reestimated the above equation, including second degree polynomial terms for each variable. A stepwise analysis shows that %NW<sup>2</sup> adds significantly to the explained variance, increasing it from 52 percent to 73 percent. We plotted the curve, beginning at the constant, described by the regression coefficients for %NW and %NW<sup>2</sup> after setting the other variables in the equation equal to their mean values. The curve shows a ceiling effect. As percent nonwhite increases from 0 to 35 percent, the percentage of whites who express fear increases from 27 percent to 45 percent, further increases in percent nonwhite show little additional effect on fear among whites.
5. Robbery can also involve violence to the victim and in most cases does involve the threat of violence. It is classified here as a property crime, because it is directed toward acquiring property, not toward personal violence, and because the relationship between the offender and the victim is more like that for property than personal crimes.
6. To examine the data for non-linear trends, we reestimated the above equation, including second degree polynomial terms for each variable. A stepwise analysis shows that %NW<sup>2</sup> adds significantly to the explained variance, increasing it from 42 percent to 61 percent. We plotted the curve, beginning at the constant, described by the regression coefficients for %NW and %NW<sup>2</sup> after setting the other variables in the equation equal to their mean values. As for whites, the curve shows a ceiling effect. As the percent nonwhite increases from 0 to 35 percent the percentage of nonwhites who express fear increases from 30 percent to 53 percent. Further increases in percent nonwhite show little additional effect on fear among nonwhites.

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# The Effect of Arrests on Crime: A Multivariate Panel Analysis\*

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## ABSTRACT

*We estimate multiwave panel models for the effect of clearance rates and a vector of socioeconomic control variables on index crimes, using a sample of 98 U.S. cities for the years 1964–70. No consistent evidence of a substantial effect is found*

The effect of police practices on rates of law violation is a matter that has a practical bearing on crime prevention strategies. It can also enhance our understanding of the contributions made by formal methods of social control to conformity. Despite the importance of these concerns, surprisingly little is known about the effects of criminal justice institutions on crime rates. Indeed, only in recent years have researchers begun to study the degree to which crime can be reduced by marginal changes in police employment, expenditure, patrolling strategies, or efficiency in solving crimes.<sup>1</sup>

Statistical investigations of the relationship between aggregated rates of crimes known to the police and various indicators of police activity have employed various analytic strategies. Some researchers have analyzed cross-sectional data (Brown; Geerken and Gove; Sjoquist; Wilson and Bolland, a, b); others have analyzed time series (Cloninger and Sartorius; Phillips and Votey) or panel data (Greenberg et al., a; Logan). The present analysis has been designed to meet methodological criticism of this body of research.

Critics of research dealing with the effect of law enforcement on crime rates have pointed to two important statistical sources of bias: speci-

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fication bias arising from the omission of relevant variables, and simultaneous equation bias (Fisher and Nagin; Greenberg, a, c; Nagin).

#### *Specification Bias*

Since crime rates are expected to be influenced by social, economic, and demographic variables, the effect of sanction levels on crime rates can be assessed only if these other variables are taken into account. Some researchers have ignored this issue altogether, reporting analyses based only on the relationships between crime rates and sanction levels (e.g. Brown; Geerken and Gove; Greenberg et al., a, b). Other investigators have attempted to control for such effects by introducing in their equations a set of variables believed to influence crime rates. However, should variables that influence both the crime rate and the sanction level be omitted from the analysis, parameter estimates for the effect of sanctions on crime will be biased, perhaps badly.

#### *Simultaneous Equation Bias*

Ordinary least-squares (OLS) methods for estimating regression equations assume that independent variables are unaffected by the dependent variable. In the present context, this means assuming that the crime rate has no influence on the predictor variables included in a regression equation for the crime rate. There are reasons for thinking that at times this assumption may be false. It has been suggested, for example, that high levels of crime could strain the limited resources of law enforcement agencies, reducing effective sanction levels. It may also be the case that when crime rates rise, the criminal justice system responds with harsher or more certain punishment. If these, or other processes in which crime rates affect law enforcement exist, failure to take them into account could lead to seriously biased estimates for the effect of law enforcement on crime.

Simultaneous equation methods permit the effects of crime on sanctions and sanctions on crime to be determined even when feedback effects of this sort are present. However, these methods can be applied to cross-sectional or time series data only if stringent assumptions are made about the effects of exogenous variables on the jointly dependent endogenous variables. To estimate the effect of a criminal justice sanction on crime one must be able to specify *a priori* the precise effect that one or more exogenous variables has on crime. Since criminological theory typically does not make predictions about the magnitude of effects that are expected to be present, this is usually done by specifying that these exogenous variables have no effect at all on crime.

It is rare in criminological research that assumptions of this kind can be made with much confidence. Too little is known about the effects on crime of a good many variables for us to be able to assert with conviction that they do not influence the crime rate. Yet if the researcher makes an

incorrect assumption about such effects, parameter estimates can be seriously biased. Statistical theory provides no method for discovering this bias from the parameter estimates themselves.

In their survey of the crime deterrence literature, Fisher and Nagin concluded that the assumptions made for the purposes of identifying simultaneous equation systems were often highly implausible on substantive grounds. Their conclusion that it will be difficult to arrive at an alternative set of more plausible assumptions underscores the desirability of using statistical techniques that do not entail such sharp limitations.

Researchers have begun to realize that panel data (collected for multiple units of analysis at a number of fixed times) can help to overcome the limitations of cross-sectional analyses. Thus Wilson and Boland (b) observe that information about changes in arrests and crime rates over a 5- or 10-year period would provide a surer basis for inferences about their reciprocal effects than the cross-sectional data they analyze; and Tittle, noting that the conclusions reached in his perceptual study of deviance deterrence are premised on assumptions about the causal order of variables that cannot be tested with his cross-sectional data, suggests that panel data would be helpful in this regard.

We have pointed out elsewhere (Greenberg and Kessler; Kessler and Greenberg) that panel data do not automatically resolve all questions of causal inference. One must still make some a priori statistical assumptions in order to estimate the effects of variables on one another. Under specific circumstances, though, multiwave panel data make it possible to identify simultaneous equation systems with weaker assumptions than those required when working with cross-sectional or time-series data. In addition, panel data permit partial tests of these assumptions, and allow information about the possible contribution of omitted exogenous variables to be extracted from the data.

These features make panel analysis a methodology of choice for studying the relationship between crime rates and sanctions. However, the panel analyses of crime rates published to date (Greenberg et al., a; Logan; Pontell) fail to control for the effect of exogenous variables on crime and sanctions. As we noted above, this failure can result in spurious correlations being treated as evidence of causal effects, and hence lead to biased estimates of the effect of arrests on crime. The present study deals with this possibility by extending an earlier panel analysis of crime rates and clearance rates (Greenberg et al., a) through the introduction of a set of socioeconomic control variables. The earlier study found no evidence that higher clearance rates led to lower crime rates. In the present study we are able to determine whether this conclusion holds up when appropriate control variables are taken into account.

## Data and Procedures

### DATA

Our analysis is based on information about crimes, clearances (crimes solved by the police, usually but not always through arrest), and a number of control variables, for a stratified random sample of 98 U.S. cities with populations of more than 25,000, for the years 1964 through 1970.<sup>2</sup> We explore the relationship between per capita rates for the seven index crimes and total index crimes (as measured by offenses recorded by the police) and the clearance rates for these offenses, defined as the ratio of offenses cleared to the number of offenses known to the police for that offense in that year.<sup>3</sup> Other influences on these variables are taken into account by introducing control variables believed on theoretical grounds to be relevant to crime causation and to police effectiveness in solving crimes.

The following variables are used in our analysis<sup>4</sup>:

1. *Population* (1967). A larger population implies a greater degree of anonymity, and hence weaker informal social controls. This in turn facilitates the emergence of a criminal subculture. In addition, police enforcement is less likely to be effective when victims do not know the identities of their victimizers.
2. *Population density* (1970). This variable is chosen for the same reason as *population*.
3. *Percent of the population below the age of 18* (1970). Arrest rates for teenagers are disproportionately high, probably because psychological and social stress associated with adolescence lead to higher rates of involvement in crime (Greenberg, b). Comparative lack of expertise in crime would be expected to increase the clearance rate of those in this age bracket.
4. *Percent of the labor force employed in manufacturing* (1970). We consider this variable to take account of the possible influence of labor force composition on crime. One might speculate that variability in the stress associated with different kinds of jobs is reflected in crime rates, and that blue collar parents socialize their children differently than white collar parents, in ways that have consequences for involvement in delinquency. In addition, Humphries and Wallace note that the ecological layout of manufacturing cities differs from that of cities with other kinds of economic activity in ways that can be expected to influence patterns of crime and law enforcement.
5. *Percent of the labor force that is unemployed* (1970). For offenses involving illegal acquisition, this variable measures one incentive to violate the law. Stress associated with unemployment may also be causally related to violent crimes. Individuals who are unemployed have, on the average, re-



duced prospects for future earnings, and thus risk less from an arrest than those who are presently employed. Reduced risk might be expected to result in higher crime rates.

6. *Median income (1960)*. This is an index of potential victim stock for property crimes. To the extent that cultural evaluation of theft and violence are linked with socioeconomic status, median income will also control for cultural contributions to variation in crime rates. In addition, median income represents community resources available for law enforcement, and thus may indirectly influence the clearance rate.

7. *Percent of population with Spanish surnames (1970)*. Minority group membership implies lower income and reduced prospects for future lawful achievement. Members of groups that have been victimized by discrimination are expected to accord less legitimacy to legal norms and law enforcement and, lacking internalized respect, less likely to conform to the law and its agents. In addition, the police may be less hesitant about arresting suspects who are members of minority groups.

8. *Percent of families headed by a female (1970)*. Several delinquency theorists (Cohen; Miller) have argued that delinquency can originate in the reaction of adolescent males to a female-headed household. A household headed by a single parent may also possess fewer resources for supervising and controlling children.

9. *Skewness of income (1970)*, as measured by the ratio of the standard deviation of income to median income.<sup>5</sup> Anomie theory (Merton) suggests that skewness of income holds out to those in low income brackets the possibility of receiving incomes that are higher than can be attained through legitimate means. This may lead to crime as an illegitimate means of achieving material goals in the absence of internalized inhibitions against stealing. Cloward and Ohlin contend that prospective thieves who fail at illegal enterprises involving theft or organized crime may turn to violence in frustration. This variable is thus potentially relevant to the genesis of violent crimes as well as theft. Inclusion of this variable is also dictated by conflict-theoretical arguments that wealthy elites strengthen police forces to cope with the threats to the social order created by economic inequality (Jacobs). The particular indicator of inequality we use has the desirable properties of being scale-free, and of being smaller for a given spread of income when overall incomes are higher.

10. *Percent of the population that is black (1970)*. The rationale for the inclusion of this variable is the same as that for variable 7. We treat the two variables separately rather than lumping blacks and Hispanics into a single minority category because cultural differences and differences in family structure between the two groups may be relevant to crime causation.

11. *Regional dummy variable for northern cities.*<sup>6</sup> Region may be a proxy for cultural differences relevant to crime causation (e.g., subculture of violence) and law enforcement effectiveness, as well as for aspects of social structure that distinguish different regions of the U.S., but that are not fully taken into account by the other variables.

12. *Regional dummy variable for southern cities.*<sup>7</sup> This variable is included for the same reason as variable 11. Cities not classified as either northern or southern are western.

Comparison with the control variables employed in studies conducted by economists shows that our list taps dimensions of social life that they have not included.<sup>8</sup>

## PROCEDURES

Because there is reason to think that the lagged and instantaneous effect of crime on clearances might be of opposite sign, we estimated models that included both lagged and instantaneous effects.<sup>9</sup> As theory does not tell us precisely what time-lag to expect, we estimated models with lags of one, two, and three years. We found that the autocorrelations among crime rates a year apart were extremely high—so high, in fact, that the correlation matrix could not be inverted. This means that the effect of other variables on these rates is too low over the space of a year for this effect to be distinguished statistically, given our sample size. For this reason, we examined models involving lags of two and three years.

All the models considered were variations on the model shown in Figure 1, a three-wave model involving per capita crime rates ( $C$ ), clearance rates ( $A$ ), and a set of exogenous control variables ( $Z$ ) assumed to influence  $C$  and  $A$ . To avoid complicating the figure, the exogenous variables are omitted, and correlations among errors are not shown; however, the models estimated did take into consideration the possible existence of cross-sectional and serial correlations among error terms and higher-order autoregressive terms (e.g., the effect of  $A_t$  on  $A_3$ , where the subscripts label the waves of observation). Models with two-year lags were estimated with data for the years 1964, 1966, 1968 and 1970; models with three-year lags, with data for 1964, 1967 and 1970.

The computer program LISREL IV (Jöreskog and Sörbom) was used to estimate all the models discussed below. We began our analysis by estimating four-wave models with a two-year time span between each wave, for each index offense separately, and for total index offenses. In these models we assumed that high-order autoregressive terms were absent, and that all correlations among errors were zero. When correlations among variables were too high to permit inversion of the correlation matrix, we considered three-wave, three-year lag models instead.

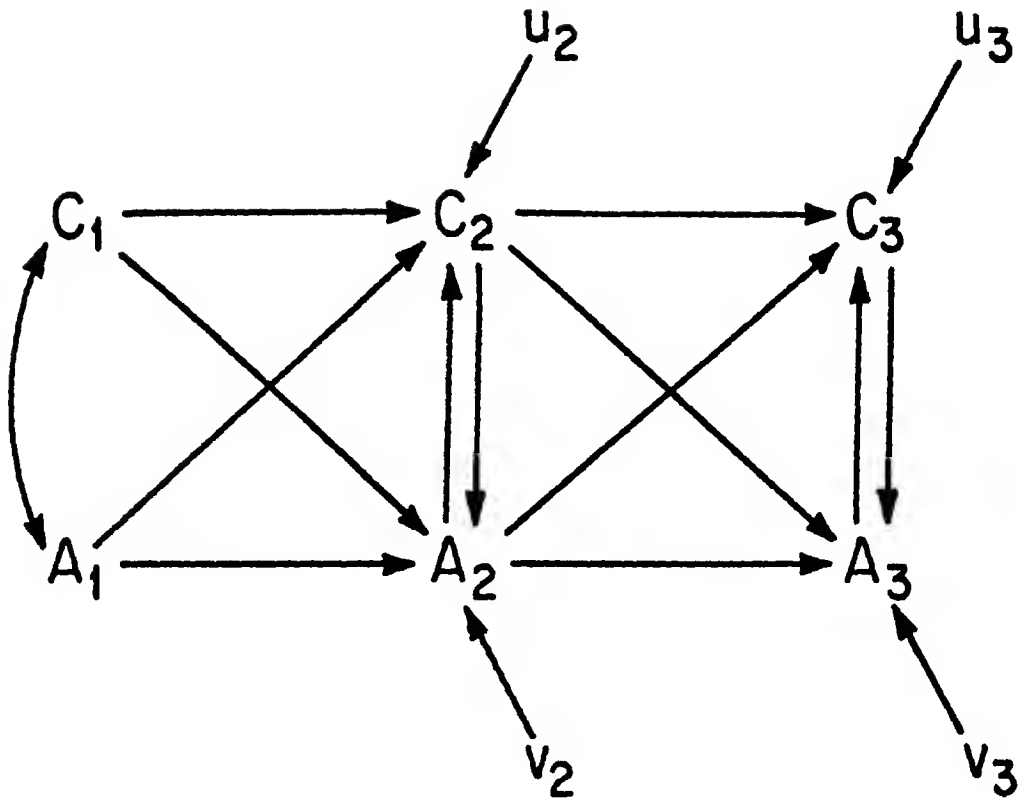


Figure 1. THREE-WAVE PANEL MODEL FOR CRIME RATES, ARREST RATES, AND EXOGENOUS CONTROL VARIABLES

In estimating the four-wave models we assumed that instantaneous standardized regression coefficients (e.g., for the influence of  $A_t$  on  $C_t$ ) remain constant over time. Cross-lagged effects linking time 1 with time 2 were unconstrained, but those linking time 2 with time 3 were assumed to equal those linking time 3 with time 4. A comparison of the estimates linking times 1 and 2 with those linking 2 and 3 (or 3 and 4) then permits a partial test of the constancy assumption. No constraints were imposed on the first-order autoregressive terms, but cross-lagged effects lagged by two or more waves were assumed to be absent.

#### *Equilibrium Conditions*

Provided the crime rates–clearance rates system is not in equilibrium, these models are overidentified, with every parameter either just-identified or overidentified, even though no assumptions have been made about the magnitude of the effect of any exogenous variable on crime or on clearances. In equilibrium, on the other hand, the normal equations used to estimate model parameters are redundant, so that insufficient information is available to identify the model. Intuitively, this happens because in equilibrium multiple waves of data do not provide the additional information

needed to identify equations; in a sense, one is seeing the same thing in each wave, so that the over-time information of panel data does not add to the information contained cross-sectionally.<sup>10</sup>

The assumption that the relationship between crime rates and clearance rates was not in equilibrium between 1964 and 1970 is reasonable on substantive grounds. Crime patterns began to change markedly in the early sixties. For instance, homicide rates, which had been declining for three decades, began to rise in 1963, and continued to increase through the early seventies. Victims were more likely than before to be unknown to perpetrators, slain in connection with street robberies; and were proportionally less often than before spouses or friends of the perpetrator. Clearance rates began to decline in the same period.

Some commentators have attributed changes in crime patterns in the early sixties to the disillusionment with the failure of the civil rights movement to achieve more rapid gains in the status of blacks, and declining clearance rates to restrictions imposed on the police by the U.S. Supreme Court in these years. For purposes of the present analysis, it is immaterial what the reasons were for these changes. What is important is that an external shock or constraint of some sort generated disequilibrium among our variables, at least for a time. Internal evidence from our estimates suggests that the assumption of disequilibrium is not unreasonable.

#### *Model Revision*

The models on the basis of which we draw our inferences were arrived at through a series of analyses. We first estimated the initial model described above for each offense. Where the initial model fit the data poorly, the model was revised by adding serial correlation of errors, higher-order autoregressive effects, or causal effects linking crime rates and clearance rates separated by more than one wave. The technical output provided by the LISREL program provided guidance as to which additional effects would prove helpful in improving the fit. Once an acceptable fit was obtained, effects that were small and consistent with zero at the 0.05 significance level were fixed at zero. These revised models were then reestimated. This process was repeated until a good fit was achieved *parsimoniously*. Comparison of the chi-square statistics for good-fitting models in which the effect of clearances on crime were estimated, with identical models in which these effects were fixed at zero, enabled us to determine whether a significant crime-prevention effect was present.

There is a danger that in fitting and trimming a model in this way one will overfit the data. To protect against fitting what are essentially sampling fluctuations, a useful rule-of-thumb is to incorporate only effects that are fairly consistent over time. Some of the models arrived at by our procedure violate this principle. In some of the models, for example, we found that an effect linking time 1 and time 2 variables proved to be

significant, while the corresponding effects linking time 2 with time 3 and time 3 with time 4 were small and insignificant. We decided to consider such models despite the danger of over-fitting, since the alternative was to accept models that fit the data poorly. In that case, interpretation of parameter estimates would be uncertain, and we would be faced with substantial ambiguity in model specification as well. It remains true, though, that where a given effect is found for one pair of waves but not for other pairs in the panel, our confidence that it represents a genuine effect is greatly reduced.

## Findings

The best-fitting models for each offense are summarized in Table 1, which contains parameter estimates for the models with 2-year lags, and Table 2, which contains parameter estimates for the models with 3-year lags.<sup>11</sup> Where more than one model provided an acceptable fit, all are shown in the table. We discuss for each offense the effect of arrests on crime, the effect of crime on arrests, and the effect of exogenous control variables on arrests and crime. Stability coefficients are given in a table in the appendix, as they are of less interest.

### THE EFFECT OF ARRESTS ON CRIME

If arrests at time  $t$  reduce crime rates at time  $t'$  (where  $t'$  may or may not be the same as  $t$ ), the estimated standardized coefficient  $A_tC_{t'}$  should be negative and statistically significant. This proves to be true in only one of the two models that provide an acceptable fit to the murder data (Table 1). In model (a), one lagged effect is negative and statistically significant ( $A_1C_2 = -.356$ ), but the other lagged effects (for  $A_2C_3$  and  $A_3C_4$ ) and the instantaneous effects are all consistent with zero. In model (b), all parameters for the effect of clearances on crime are fixed at zero. Model (a) fits the data significantly better than (b), but both models provide quite good fits. Evidence for a possible crime-prevention effect here is clearly very limited. The lack of consistency in the estimates for model (a), and the good fit obtained for the null model (b) lead us to conclude that we have no persuasive evidence in our data for the existence of a crime-prevention effect for murder.

For burglary, we find good fits with two different models. In model (a), the lagged effect  $A_1C_2$  is estimated while all other lagged effects are fixed at zero; and the instantaneous effects  $A_tC_t$  are constrained to be equal. The lagged effect is extremely small, providing a check on the assumption that the parameters  $A_2C_3$  and  $A_3C_4$  are zero. The instantaneous effect is negative and statistically significant, but quite small in magnitude.

**Table 1.** ACCEPTABLE MODELS FOR CRIME RATES (C) AND CLEARANCE RATES (A) WITH TWO-YEAR LAGS<sup>a</sup>

Parameter	Offense					
	Murder (Model I)	Murder (Model II)	Rape	Burglary (Model I)	Burglary (Model II)	Grand Larceny
<u>A → C</u>						
A <sub>1</sub> C <sub>2</sub>	-.356*			.017		
A <sub>2</sub> C <sub>3</sub>						
A <sub>3</sub> C <sub>4</sub>						
A <sub>2</sub> C <sub>2</sub>				-.085*		
A <sub>3</sub> C <sub>3</sub>				-.085*		
A <sub>4</sub> C <sub>4</sub>				-.085*		
<u>C → A</u>						
C <sub>1</sub> A <sub>2</sub>	-.365*			-.096		
C <sub>2</sub> A <sub>3</sub>						
C <sub>3</sub> A <sub>4</sub>						
C <sub>1</sub> A <sub>4</sub>	-.081					
C <sub>2</sub> A <sub>4</sub>			-.329*			
C <sub>2</sub> A <sub>2</sub>			-.291*	-.034		
C <sub>3</sub> A <sub>3</sub>				-.034		
C <sub>4</sub> A <sub>4</sub>				-.034		
<u>C ↔ A</u>						
C <sub>1</sub> A <sub>1</sub>	-.173	-.173	-.105	-.255*	-.255*	-.451*
C <sub>2</sub> A <sub>2</sub>	.065	.074			-.074*	
C <sub>3</sub> A <sub>3</sub>	-.193*	-.193*			-.052*	
C <sub>4</sub> A <sub>4</sub>	-.019	-.023			-.038	
<u>Fit Statistics</u>						
χ <sup>2</sup>	12.47	23.14	17.46	29.06	28.13	21.81
d.f.	14	17	16	16	17	17
probability level	.50	.10	.30	.02	.02	.10
largest S-Σ <sup>b</sup>				.068	.091	

<sup>a</sup>X<sub>i</sub>Y<sub>j</sub> represents the standardized regression coefficient for the effect of X<sub>i</sub> on Y<sub>j</sub>.

<sup>b</sup>S-Σ is the difference between the observed and predicted covariance matrix. Cell entry is the largest element in the difference matrix. Entries are given only for models in which the probability level is greater than 0.05.

\*Statistically significant at the 0.05 level.

Table 2. ACCEPTABLE MODELS FOR CRIME RATES (C) AND CLEARANCE RATES (A) WITH THREE-YEAR LAGS<sup>a</sup>

Parameter	Offense			Total Index
	Aggravated Assault	Robbery	Auto Theft	
<u>A → C</u>				
A <sub>1</sub> C <sub>2</sub>	.077			
A <sub>2</sub> C <sub>3</sub>	-.143*			
<u>C → A</u>				
C <sub>1</sub> A <sub>3</sub>			.239 <sup>‡</sup>	
<u>C ↔ A</u>				
C <sub>1</sub> A <sub>1</sub>	-.177*	-.215 <sup>‡</sup>	.456*	-.346*
C <sub>2</sub> A <sub>2</sub>	.053			-.012
C <sub>3</sub> A <sub>3</sub>	.055			-.100*
<u>Fit Statistics</u>				
χ <sup>2</sup>	11.09	13.82	39.61	7.37
d.f.	5	9		8
probability level	.02	.30	<.001	.30
largest S-Σ <sup>b</sup>	.127		.070	

<sup>a</sup>X<sub>i</sub>Y<sub>j</sub> represents the standardized regression coefficient for the effect of X<sub>i</sub> on Y<sub>j</sub>.

<sup>b</sup>S-Σ is the difference between observed and predicted covariance matrix. Cell entry is the largest element in the difference matrix. Entries are given only for models in which the probability level is greater than 0.05.

<sup>‡</sup>Statistically significant at the 0.05 level.

In model (b), all effects in which A influences C are fixed at zero, but cross-sectional correlations among errors are estimated (in model (a) they were fixed at zero). The fit here is also good, but not significantly better than in model (a). Thus the evidence for a crime prevention effect here is ambiguous, and the effect is small in any event.

For aggravated assault (Table 2), our best-fitting model differs significantly from the observed correlation matrix; but since the fit is substantively quite good, we proceed to interpret parameter estimates. We find a statistically significant estimate A<sub>2</sub>C<sub>3</sub> = -.143, consistent with a crime-prevention effect. However, the estimate for A<sub>1</sub>C<sub>2</sub> is smaller, positive and not statistically significant (.077). The lack of consistency here makes us re-

luctant to interpret the estimate for  $A_2C_3$  as evidence for a crime-prevention effect.

In all other offense categories, good fits to the data were obtained for models in which crime prevention effects were small and not statistically significant. When these effects were then fixed at zero and the models reestimated, good fits were still obtained; subtraction of the chi-square statistics for the two models showed the crime prevention effects to be statistically insignificant.

#### THE EFFECT OF CRIME ON CLEARANCE RATES

Estimates of the parameter  $C_1A_1$ , provide evidence regarding the effect of crime rates on clearance rates. For several of the models in Tables 1 and 2, we find isolated estimates for  $C_1A_1$ , parameters that are negative and statistically significant; for example, in model (a) for murder (Table 1), the parameter  $C_1A_2$  is  $-.365$ , statistically significant. But the other seven parameters are either not significant ( $C_1A_4 = -.081$ ) or consistent with zero. Thus there is no consistent evidence for a saturation effect, or for a process in which rising crime rates enhance the efficiency of law enforcement efforts. In other models such as murder (b), burglary (b), and grand larceny (Table 1), and aggravated assault, robbery, and total index offenses (Table 2), all parameters are consistent with zero.

#### THE EFFECT OF EXOGENOUS VARIABLES

The effect of exogenous variables can be studied in our models in two ways: through parameter estimates for the influence of control variables on our endogenous variables, and through the correlations of errors in models where these were estimated. Since the focus of this paper is on the impact of law enforcement on crime, we report our findings for the effect of the control variables on crime rates and clearance rates somewhat cursorily.<sup>12</sup>

With only a few exceptions, the effects of the control variables on crime rates and clearance rates were neither large nor consistent from one year to the next. A few of the variables did show consistent effects that were more than miniscule in magnitude (statistically significant at the 0.05 level or having an absolute value of at least .10); we confine our discussion to these contributions.

The effect of city population on murder, rape, and robbery was positive, but no such effect was found for the other offenses. The effect of population density on robbery was also positive, while its effect on clearances for murder, assault, robbery, larceny, and total index offenses was negative. Contrary to expectation, the proportion of the population below the age of 18 had no effect on crime rates. It tended to reduce the clearance rate for assault, but to increase it for auto theft.



Proportion of the labor force employed in manufacturing was found to have no consistent, substantial effect on crime rates. It did increase the clearance rate for assaults, but reduced it for auto thefts. Percent of the labor force that is unemployed had a positive effect on murder, but not on any of the other offense rates. Median income had a negative effect on rape, a positive effect on burglary, but no consistent effect on the other crime rates. The effect of this variable on clearances for rape and auto theft was negative.

Percent of the population with Spanish surnames was positively related to rape and assault; it tended to reduce clearance rates for rape, but to increase them for assaults. Percent of the population that is black was positively associated with murder, assault, robbery and auto theft, as well as with clearances for assault and robbery.

Percent of families headed by a female had no consistent effects on crime rates, but had a positive effect on clearances for robbery and larceny. Skewness of income had a negative effect on rape and no consistent effect on other crime rates. It did tend to reduce clearances for larceny and auto theft. Cities in the North had higher robbery rates and clearance rates for robbery, but lower clearances for auto theft; cities in the South had lower rates of rape and clearances for rape.

Given that the correlations of control variables with crime rates and clearances are neither negligible nor inconsistent, this failure to find stronger and more consistent effects may seem surprising. The reason for this finding, however, can readily be seen by considering the unstandardized structural equation for the crime rate:

$$C_t = a + b_1 C_{t-1} + b_2 A_t + b_3 A_{t-1} + b_4 Z_t + u_t. \quad (1)$$

For simplicity, the equation includes only a single control variable  $Z$ , but the argument will not be affected if additional control variables are present. If we subtract the quantity  $C_{t-1}$  from left-hand and right-hand members of the equation, we have

$$\Delta C_t = C_t - C_{t-1} = a + (b_1 - 1)C_{t-1} + b_2 A_t + b_3 A_{t-1} + b_4 Z_t + u_t. \quad (2)$$

We see that the coefficient  $(b_1 - 1)$  measures the effect of the level of  $C$  on change in  $C$ , while the remaining coefficients are direct measures of the effect of  $A_t$ ,  $A_{t-1}$ , and  $Z_t$  on change in  $C$ . When we estimate equation (1), then, we are not estimating the effect of  $Z$  on  $C$ , but the effect of  $Z$  on *change* in  $C$ . Once this is understood, the paucity of statistically significant coefficients for the effect of our control variables on crime rates is less mysterious. We might expect, for example, that a certain level of unemployment would generate a corresponding level of crime in a city. But we would not expect a fixed level of unemployment to lead to steadily *increasing* levels of crime. The same is true for our other control variables.

Even where the control variables did contribute significantly to the

regressions, their contributions were almost always quite modest. Hardly any of the standardized effects were as large as .30 in magnitude. Comparing regressions with all control variables deleted from the regressions (reported in Greenberg et al., a) with those reported here, we find increments in the variance explained of roughly 5 percent, not a large amount. Moreover, our conclusions about the effect of arrests on crime are not substantially changed by the inclusion of these variables. In the earlier study, good fits were obtained for all offenses with models in which the effect of clearances on crime rates was fixed at zero. Here, such effects are either consistent with zero or quite small in magnitude.

The insensitivity of these conclusions to the inclusion or exclusion of the twelve control variables used here gives us confidence that our findings are unlikely to be badly biased by the omission of additional causes of crime.<sup>13</sup> Our confidence that this is so is strengthened by our estimates of the correlations among contemporaneous error terms of crime rates and clearance rates. Good fits for rape, burglary (a), grand larceny, robbery, and auto theft were obtained for models in which these correlations of errors were fixed at zero; in models for murder (a), murder (b), burglary (b), aggravated assault and total index offenses these correlations were estimated; but as seen from the entries in Tables 1 and 2, the estimates proved to be invariably small. In no case did they exceed .20 in magnitude, and in most instances they were considerably smaller. The amount of bias in estimates for the parameters  $AC$ , that could be present as a result of the omission of variables responsible for these correlations of errors could not be large.

## Discussion

Our analysis finds no consistent evidence for the proposition that higher arrest clearance rates result in substantially lower index crime rates. Where parameter estimates for the effect of arrests on crime are consistent (as in the model for aggravated assault), they are quite small. When effects are larger in magnitude (as in one of the two models for murder), they are not consistent over time. For most of our models we did not even find inconsistent evidence for a crime-prevention effect of arrests.<sup>14</sup>

Our failure to find evidence for a crime-prevention effect contrasts with the econometric studies based on cross-sectional or time-series data, which have found evidence consistent with a crime-prevention effect. We attribute these discrepant findings to the dubious assumptions made in the econometric analyses. Cloninger and Sartorius assume that crime rates are entirely uninfluenced by socio-economic variables, and thus that law enforcement variables *alone* influence crime rates. As noted earlier, Phillips

and Votey control for labor force participation alone, neglecting all other social influences on crime. Sjoquist considers a wider range of control variables, but neglects the possible reciprocal effect of crime on arrests.

In the most careful cross-sectional analysis to date, one which finds that arrests reduce crime for robbery but not for burglary, and only inconsistently for auto theft, Wilson and Boland (b) assume that police patrol strategy is affected by a city's political climate but not by its crime rate; and that patrol strategy affects the crime rate only by changing the probability of an arrest, not directly. They frankly concede that their analysis depends on the validity of these assumptions, and while they find them plausible, they cannot demonstrate that they are true. We think Wilson and Boland's assumptions are at least debatable. Crime has become a hot political issue over the last fifteen years. In this atmosphere, a city's political climate and police patrol strategies may well have been influenced by its crime pattern. Police can deter crime by appearing on the street, even if they make no arrests; indeed, it is a common experience that highway drivers will slow down when they see a squad car, even if they do not see any cars being ticketed.

Our approach, based on panel data, has obviated the need for assumptions as stringent as those made in earlier work, solely for reasons of statistical convenience. This is not to say that the present approach is entirely free from assumptions. The method we outline here assumes that parameters are stationary, and our parameter estimates are not entirely consistent with it. Fortunately, this assumption is not needed in the null models. We have assumed also that lagged effects are felt within the space of a few years; were the correct time-lag closer to a century, we would be unlikely to see any signs of a causal effect. Here our everyday knowledge about the duration of social processes comes to our aid to exclude such possibilities.

Since the assumptions we make are less stringent and more plausible than those made in earlier studies, we are inclined to attribute the discrepant findings to model misspecification in the earlier work. We were able to verify in our own data that an inappropriate cross-sectional analysis could lead to biased parameter estimates consistent with a crime-prevention effect. We did this by estimating multiple regression equations for the crime rates in 1970 using 1970 clearance rates and the 12 control variables as predictors. Here lagged endogenous variables are not included among the predictors, and no attempt is made to model the short-run effect of crime on clearances.<sup>15</sup> In these models, the estimated standardized regression coefficients for the effect of clearances on crime rates were: murder,  $-.004$ ; rape,  $-.128$ ; assault,  $-.177^*$ ; robbery,  $-.232^*$ ; burglary,  $-.213^*$ ; auto,  $-.142$ ; grand larceny,  $-.319^*$ ; and total index offenses,  $-.232^*$  (starred parameters are significant at the 0.05 level). All eight estimates

are negative, and five of them are statistically significant. This contrasts with our failure to find consistent negative estimates for these parameters in the panel analysis.

Although we find no evidence that marginal changes in clearance rates have an appreciable impact on crime rates, we are far from claiming that police practices have no effect on crime. Part of the appeal of the deterrence doctrine in its current revival at the hands of economists is that we all know from introspection and from everyday observation, that people can be made to conform by threatening them with undesirable consequences. It may be that we fail to see evidence of this in the present data because the clearance rate is too poor an indicator of the probability of an arrest for us to detect a prevention effect. Another possibility is that marginal changes in risk are not communicated very well to prospective offenders. Since systematic information about risks is not at all easy for prospective offenders to obtain, this may well be the case. Where information about changes in penalties is not communicated, we can hardly expect those changes to result in changed deterrence.

Once we acknowledge that information about penalties and how they change can be highly inaccurate, our failure to find substantial and consistent evidence that clearances reduce crime becomes understandable. It can still be the case, however, that crime is deterred by mistaken beliefs about penalties. There is an old joke about the small hamlet that purchased a sign reading "Radar used to apprehend speeders"—but couldn't afford the radar equipment. Our findings cannot say whether that investment was a sound one. A sign in a store warning that "shoplifters will be prosecuted" may reduce theft even if the store owner never apprehends or prosecutes shoplifters.

What we can say is that given the sort of publicity about law enforcement practices that now prevails, marginal increases in clearance rates—those that are on the order of the differences observed among the clearance rates of the cities in this sample—are unlikely to lead to measurable reduction in index crimes. More substantial changes in clearance rates, or in the sanctions imposed at later stages of the criminal justice system (or propaganda campaigns about the risk of crime, whether empirically founded or not) might well have a greater effect.

## Notes

1. Research dealing with the effect of law enforcement on crime rates is often advertised as "deterrence research." In fact, most of this work does not study the deterrent effect of sanctions alone, but the net effect of law enforcement on crime, regardless of the process by which enforcement creates this effect. The possible contributions of rehabilitation and incapacitation (restraint), reinforcement of the collective conscience, and so forth, are not eliminated in analyses of the aggregate relationships between crime rates and sanction levels.
2. For details of the sampling procedure, see Greenberg et al. (a). On the basis of a separate analysis of data for all 50 states as well as the data for the sample of cities analyzed here we

argue elsewhere (Greenberg et al., b) that data for cities are less likely to be subject to aggregation bias.

3 Roland Chilton has recently argued that ambiguities in the definition of a clearance make it less satisfactory than the ratio of arrests to crimes (although an arrest is the most common way the police clear a crime, there are other ways as well). However, the latter ratio is not entirely satisfactory either. Since a single crime can be cleared by the arrest of more than one individual, the ratio of arrests to crimes is not identical to the theoretically relevant probability that a crime will be followed by an arrest. We consider it an open question at this time which indicator is the most appropriate in studying the impact of arrests on crime.

4 Although we had initially planned to include indices for poverty and for high income, these variables proved to be highly correlated with our other control variables and were consequently excluded from the analysis. Data for some of the control variables included were available for both 1960 and 1970. However, correlations among observations at the two times proved to be quite high (ranging from 0.70 to over 0.90). To avoid problems of multicollinearity, data for only one of the two time points were used. We employed 1970 data for all variables except median income, 1960 values were used for this variable because they were not as highly correlated as the 1970 values with other variables. Data for all the control variables were drawn from the Bureau of the Census.

5 These quantities are computed on the basis of the proportions of families earning less than \$3,000, between \$3,000 and \$4,999, between \$5,000 and \$6,999, between \$7,000 and \$9,999, between \$10,000 and \$14,999, between \$15,000 and \$24,999, and over \$25,000. For computing purposes, the upper limit of the highest-income category was taken to be \$35,000.

6 Cities located in the following states were classified as northern: Connecticut, Illinois, Indiana, Iowa, Massachusetts, Michigan, Minnesota, Missouri, New Hampshire, New Jersey, New York, Ohio, Pennsylvania, Vermont and Wisconsin.

7 Cities were classified as southern if located in Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Texas, Virginia. Cities were classified as western (neither northern nor southern) if located in California, Nebraska, Nevada, New Mexico, North Dakota, Oklahoma, Utah and Washington.

8 For example, Phillips and Votey introduce only a single control variable, an indicator of labor force participation; Sjoquist fails to include indicators of such theoretically relevant variables as age distribution of the population, the degree of affluence in a community, inequality of income, occupational distribution, broken homes, and region of the United States.

9 We have shown elsewhere (Greenberg and Kessler) that parameter estimates may have the wrong sign in models that have incorrectly specified lags. In the present context, there is reason for thinking that lagged and instantaneous effects of crime on sanctions may be of opposite sign, since saturation effects, if they exist, should be short-run (instantaneous), while pressure to increase the efficiency of policing in response to rising crime rates is expected to be effective only gradually, over a period of time. If these two opposite-sign effects are lumped together in a model that included only an instantaneous effect or only a lagged effect, the two processes would tend to cancel one another.

10 As the system of equations approaches equilibrium, the standard errors of parameter estimates increase rapidly, so that even moderately large parameters fail to achieve statistical significance in large samples. We did not encounter this in our estimates. At the point where equilibrium is reached, our models become underidentified. Intuitively, this happens because in equilibrium, multiple waves of data do not provide the additional information needed to identify equations. For a more mathematical treatment of approach to equilibrium in multi-wave panel models, see Kessler and Greenberg.

11 Models involving lags of three years were estimated only when the correlation matrix with variables lagged at two-year intervals could not be inverted.

12 We do not provide estimates for the effects of control variables on crime rates or clearance rates here (there are several dozen such estimates for each offense), but restrict our discussion to the most noteworthy features of the estimates. The first-named author will provide tables of these estimates upon request.

13 Given this argument one may wonder whether our use of panel models renders our analysis equally insensitive to the effect of *arrests* on crime. The answer is no. If the true structural equation governing crime rates is  $C_t = a + b_1A_t + b_2Z_t + e_t$ , where  $e$  is an error term, and

one subtracts from this the same equation lagged by one time unit and rearranges terms, one obtains the equation

$$C_t = C_{t-1} + b_1(A_t - A_{t-1}) + b_2(Z_t - Z_{t-1}) + (e_t - e_{t-1}).$$

This equation tells us that the coefficients of  $A_t$  and  $Z_t$  represent the respective effects of change in  $A$  and  $Z$  on change in  $C$ . If  $Z_t$  and  $Z_{t-1}$  are correlated and only  $Z_t$  is included in the analysis, its contribution will be suppressed by the factor

$$(1 - r_{Z_t, Z_{t-1}}).$$

Where the correlation between  $Z_t$  and  $Z_{t-1}$  is high, as it is in our data, this suppression is high. Since  $A_t$  and  $A_{t-1}$  were not highly correlated in our data, both were included in our initial models, and one or the other deleted only when its contribution was negligible. Here the low correlation between lagged and instantaneous values reduces the suppression effect.

14. We also examined models involving offense rates for individual offenses and arrest clearance rates for total offenses. The rationale for considering these models was the possibility that an arrest for one offense might deter someone contemplating the commission of another offense. Moreover, if criminals switch crime categories (and there is reason to believe that some do), the arrest and incarceration of someone charged with a given offense may eliminate offenses in other crime categories through the restraining (incapacitative) function of imprisonment. None of these models yielded evidence suggesting the existence of a crime-prevention effect for arrests.

15. Our panel analyses suggest that no serious error is likely to result from the neglect of reciprocal causation. Any discrepancy between the cross-sectional analysis and the panel analysis, therefore, must arise from the inclusion of the lagged endogenous variable in the latter analysis.

To see the circumstances under which the two approaches will yield the same conclusions about the effect of predictor variables on the criterion, consider the panel equation  $y_2 - y_1 = a_1 y_1 + a_2 x + a_3 z$ . If the variables have reached equilibrium, so that  $y_1 = y_2$ , we can set the left hand member equal to zero, and solve the resulting equation for  $y_1$ , obtaining  $y_1 = -(a_2/a_1)x - (a_3/a_1)z$ . The coefficients for  $x$  and  $z$  are in the correct proportion, although their magnitudes will in general be biased. Thus it is only in equilibrium that the static and dynamic approaches yield comparable results.

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## Appendix

The stability coefficients for the two-year lag models summarized in Table 1 are given in Table A.1; those for the three-year lag models summarized in Table 2, in Table A.2.

**Table A.1.** STABILITY COEFFICIENTS FOR TWO-YEAR LAG MODELS FOR CRIME RATES (C) AND CLEARANCE RATES (A)<sup>a</sup>

Parameter	Offense					
	Murder (Model I)	Murder (Model II)	Rape	Burglary (Model I)	Burglary (Model II)	Grand Larceny
C <sub>1</sub> C <sub>2</sub>	.334	.382	.267	.840	.856	.927
C <sub>2</sub> C <sub>3</sub>	.368	.368	.213	.854	.893	.796
C <sub>3</sub> C <sub>4</sub>	.273	.274	.621	.621	.650	.904
C <sub>1</sub> C <sub>3</sub>			.236			
A <sub>1</sub> A <sub>2</sub>	.356	-.288	.173	.568	.606	.568
A <sub>2</sub> A <sub>3</sub>	.237	.237	.069	.594	.608	.334
A <sub>3</sub> A <sub>4</sub>	.183	.189	.218	.670	.673	.687
A <sub>1</sub> A <sub>3</sub>		.427		.301	.303	.386
A <sub>1</sub> A <sub>4</sub>			-.206			
A <sub>2</sub> A <sub>4</sub>			.166			

<sup>a</sup> $x_i y_j$  represents the standardized regression coefficient for the effect of  $x_i$  on  $y_j$

**Table A.2.** STABILITY COEFFICIENTS FOR THREE-YEAR LAG MODELS FOR CRIME RATES (C) AND CLEARANCE RATES (A)<sup>a</sup>

Parameter	Offense			Total Index
	Aggravated Assault	Robbery	Auto Theft	
C <sub>1</sub> C <sub>2</sub>	.760	.771	.912	.847
C <sub>2</sub> C <sub>3</sub>	.660	.689	.986	.825
A <sub>1</sub> A <sub>2</sub>	.467	.256	.638	.606
A <sub>2</sub> A <sub>3</sub>	.557	.196	.713	.513
A <sub>1</sub> A <sub>3</sub>	.269	.140	1.015	

<sup>a</sup> $x_i y_j$  represents the standardized regression coefficient for the effect of  $x_i$  on  $y_j$



# Problems in Ratio Correlation: The Case of Deterrence Research\*

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## ABSTRACT

*Potential problems in ratio correlation cannot be resolved outside a particular substantive context. Within the context of deterrence research, several approaches are examined: the "conceptual-meaning" resolution, the Pearsonian approximation formula and null comparison, simulation techniques, decomposition into component covariances, part correlation, and the use of residual scores. A simulation experiment shows that when the terms used in the measures of certainty of imprisonment and crime rate are randomly scrambled, the resulting ratios correlate in a manner comparable to what occurs with the data in their original form. These scrambled-data correlations, however, are due purely to artifactual effects of the common term. The most useful test for the existence of this common-term artifact appears to be the technique of part correlation. With empirical imprisonment data, the part correlations are lower than the zero-order correlations, supporting the possibility that the original correlations may have been at least partially artifactual.*

Much sociological research consists of relating complex indexes to each other. These indexes are formed by combining separate but related indicators in some fashion, by adding, subtracting, multiplying, and dividing them in various ways. Such indexes are indispensable to many kinds of social scientific research, and it is therefore important to consider carefully the ways in which the construction of our indexes can affect the validity and interpretation of our research. This paper will consider in detail one such type of index construction: the formation of ratios, with specific attention to problems that may arise when attempting to correlate two ratios in which the numerator of one is the same as the denominator of the other. In one rapidly expanding body of research, the deterrent or preventive effects of legal sanctions on crime, the strongest findings of most of the major

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studies involve correlations between two ratios having exactly this kind of common-term problem. This research will be used for illustration throughout the discussion.

Although recognition of the problems involved in correlating ratios having common terms dates back to the turn of the century (Pearson), it has only very recently been given concentrated attention by sociologists. Excellent general discussions of problems in the correlation of ratios or difference scores having common terms have been provided, with a range of sociological examples (Fuguitt and Lieberman; Long; Schuessler a,b). The discussion has recently reached practically a stage of debate (Freeman and Kronenfeld; Kasarda and Nolan; MacMillan and Daft). No single set of rules or formulas can be derived from existing statistical discussions that will be adequate for all data based on ratios with common terms. For this reason, the discussion of problems in ratio correlation will be applied to one set of data in one substantive context (deterrence).

### **The Problem of Spurious Ratio Correlations in Deterrence Research**

In critiques of deterrence research thus far (Greenberg; Nagin), relatively little attention has been paid to one very basic difficulty common to much of this research. The early core of deterrence research consisted of a group of studies that discovered a remarkably consistent negative relationship between crime rate and some measure of certainty of criminal sanction (punishment) for crime. These studies all used cross-sectional data aggregated by state, county, or city units. The sanctions they dealt with were *execution* (Ehrlich, c), *imprisonment* (Antunes and Hunt; Bailey et al.; Bean and Cushing; Chiricos and Waldo; Ehrlich, a, b; Erickson and Gibbs; Gibbs; Gray and Martin; Kobrin et al.; Logan, b; Tittle; Vandaele), and *arrest* (Block; Chapman; Kobrin et al.; Logan, c; Phillips and Votey; Tittle and Rowe). Thus, units with higher rates of punishment (arrest, imprisonment, or execution) tended to show lower rates of crime, and vice versa.

The problem common to these studies consists of an alleged definitional dependency, or contamination, between the measures of the independent and dependent variables. The independent variable, certainty of sanction, consists of the ratio of sanctions to crimes, denoted here as  $A/C$ . ("A" is used because in most studies the sanction is admission to prison or arrest clearances.) The dependent variable is per capita crime rate, denoted as  $C/P$ . The common term,  $C$ , has been either identical or very highly correlated between the two ratios, in the data analyzed by the studies described above.

The question at issue here is whether an increase in the common term,  $C$ , might tend both to deflate the value of  $A/C$  and inflate the value of  $C/P$ . If so, this would produce a negative correlation, as an artifactual

effect, in the same direction as the relation predicted by the deterrence hypothesis. Thus, the first problem in interpreting the negative correlations found in deterrence research is to try to determine whether they are real or artifactual. (Other problems, such as the direction of causal effect, also exist in the deterrence research but are not discussed here.)

One data set from the literature described above (Logan, b) will be used for illustration. These particular data were chosen for their availability and because they are similar in character—and in some cases overlapping or identical—to data analyzed by the several imprisonment studies cited above. However, the analysis here will carry implications for all deterrence studies that measure certainty of sanction by means of a ratio of sanctions administered to crimes known, whether the sanction be arrest, conviction, imprisonment, or execution.

### Responses to the Problem of Spuriousness in Ratio Correlation

The problem of correlating ratios having common terms has been addressed specifically in the deterrence literature (Chiricos and Waldo; Logan, a, b, c; Tittle) as well as in more general methodological literature (Bollen and Ward; Freeman and Kronenfeld; Fuguitt and Lieberman; Kasarada and Nolan; Long; MacMillan and Daft; Pearson; Schuessler, a, b). Several resolutions that have been suggested will be critically examined as to difficulties in their application and interpretation:

1. The *conceptual-meaning* resolution.
2. The Pearsonian null comparison.
3. Simulation techniques.
4. Decomposition into component covariances.
5. Part correlation and the use of residual scores.

#### 1. THE CONCEPTUAL-MEANING RESOLUTION

One early and continuing disclaimer has been that if our primary conceptual and theoretical interest is in the ratios per se, rather than in the components that make up the ratios, then the issue of spuriousness in the correlation of two ratios does not logically arise, even if they do have a common term (Fuguitt and Lieberman; Kasarada and Nolan; Kuh and Meyer; McNemar; Rangarajan and Chatterjee; Schuessler, a; Yule). Spuriousness, according to this argument, becomes a problem only when the common term is used to standardize, or otherwise adjust, both of the two major variables of interest (e.g., city taxes and city expenditures, both being divided by population size). If our hypothesis is stated in terms of the correlation between two ratios, we cannot legitimately test it with correla-

tions between the unadjusted component terms, nor vice versa, since correlations between ratios will generally differ from the correlations between the component terms. With regard to the deterrence data, it has been argued (Logan, a, b) that in the ratio  $A/C$ , neither component is conceptually sufficient by itself; the deterrence hypothesis refers to their relation to each other and not to the absolute values of either component. An analogy could be made to the well-known positive association between speed (miles per hour) and gas consumption (gallons per mile), where we are also not interested in the relation among the components but in the relation between the ratios. In this example, it is clear that the ratio variables are not mere epiphenomena, created by the artifact of dividing one measurement by another, but are conceptually meaningful variables in themselves. Further, we expect these ratios to have a causal correlation opposite in sign to what would be expected as a result of indexical artifact.

Several recent papers have made strong cases for the argument that there is nothing inherently spurious or biased in the empirical correlation between ratios containing common terms and that where ratios properly express the concepts or variables of interest they ought not be abandoned in favor of some alternative measures (Kasarda and Nolan; Long; Mac-Millan and Daft a, b). Long for example, demonstrates that a correlation between two ratios can take any sign, depending on the slopes and intercepts of the relations among the components. As will be shown in a later section on decomposition, this is merely an analytic truth. Correlations between ratios are affected by the nature of the components (their variances, covariances, slopes and intercepts) *and vice versa*. This, however, does not tell us whether in any given instance we are to take the sign of the ratio correlation as given, which then constrains the components, or to take as given the constraints on the components, which then affect the sign of the ratio correlation. Thus Long et al. have demonstrated that there is no *necessary* bias in the empirical correlation of ratio variables, but just because bias does not *automatically* occur does not mean that it *cannot* occur.

In sum, the problem with the conceptual or theoretical-meaning resolution, along with the demonstration that common-term ratios are not inherently biased, is not that they are wrong, but that they are insufficient. They do not provide us with any means of distinguishing causal correlations from artifactual ones.

## 2. THE PEARSONIAN NULL COMPARISON

In perhaps the earliest treatment of this problem, Karl Pearson developed a formula giving an approximation of the correlation between two ratios in terms of the correlations and rel-variances (or coefficients of variation) of the component terms. The formula for the general case is:

$$r_{(Y/Z)(X/W)} = \frac{r_{YX}V_YV_X - r_{YH}V_YV_H - r_{XZ}V_XV_Z + r_{ZH}V_ZV_H}{\sqrt{V_Y^2 + V_Z^2 - 2r_{YZ}V_YV_Z} \sqrt{V_X^2 + V_H^2 - 2r_{XH}V_XV_H}} \quad (1)$$

where  $V_X$  = the rel-variance (or coefficient of variation) of  $X$  (i.e., the standard deviation divided by the mean). Formulas for the correlation of ratios having common terms can be easily derived from this basic formula. For the case of  $A/C$  and  $C/P$  we have, with appropriate changes in notation:

$$r_{(A/C)(C/P)} = \frac{r_{AC}V_AV_C - r_{AP}V_AV_P - V_C^2 + r_{CP}V_CV_P}{\sqrt{V_A^2 + V_C^2 - 2r_{AC}V_AV_C} \sqrt{V_C^2 + V_P^2 - 2r_{CP}V_CV_P}} \quad (2)$$

The most commonly suggested approach (Chayes; Fuguitt and Lieberman; Kuh and Meyer; Pearson; Rangarajan and Chatterjee; Yule; Yule and Kendall) to the problem of spuriousness in ratio correlation involves a manipulation of formula (2) to produce a null value representing the correlation between  $A/C$  and  $C/P$  that would be expected to occur under the condition that the components ( $A$ ,  $C$ , and  $P$ ) are uncorrelated. If  $r_{AC} = r_{CP} = r_{AP} = 0$ , formula (2) becomes:

$$\frac{-V_C^2}{\sqrt{V_A^2 + V_C^2} \sqrt{V_C^2 + V_P^2}} \quad (3)$$

a value that will obviously be negative. Pearson referred to this value as the "spurious correlation," while Chayes refers to it as the "null correlation." Comparing the values computed by (2) and (3) provides a method of determining whether the empirical correlation approximated by (2) is larger than the correlation that would be obtained from these data if the ratio components were uncorrelated, as estimated by (3). This null comparison is supposed to be analogous to a test of the ordinary null hypothesis that  $r = 0$ . Thus, an empirical correlation larger than the value from (3) would be needed to reject the null hypothesis of a non-artifactual correlation between the ratios.

As an illustration, if we applied these formulas to our deterrence data (Logan, b), for the case of total felonies, the approximation formula (2) gives  $r = -.79$  while the null formula (3) gives  $r = -.68$ . The (approximate) ratio correlation is larger than the null value.

To use this comparison to test whether the ratio correlation is real or artifactual, however, would be misleading, for two reasons.

First, the ratio correlation approximated by formula (2) is seriously in error when applied to the data in the example above. Using decimal values for  $A/C$  and  $C/P$ , the standard Pearsonian correlation coefficient (exact) is  $-.44$ , compared to the approximation formula value of  $-.79$ . Because Pearson's formula was derived from a binomial expansion in which

terms higher than the second order were dropped, it is an approximation that becomes subject to serious error when any of the values of  $V$  rise above 0.15 (Chayes, 15). In the deterrence data on imprisonment used here for illustration,  $V$  ranges from 0.94 to 1.86. For the "total felonies" data used just above,  $V_A$ ,  $V_C$ , and  $V_P$  are 0.94, 1.45 and 1.06, respectively.

The second, and more important, problem with the Pearsonian procedure is that, strictly speaking, the null comparison does not directly test the hypothesis of noncorrelation between two ratios; rather, it tests the hypothesis of noncorrelation among the *components*. Finding a correlation larger than the null value, and thus rejecting the null hypothesis, should therefore only mean rejection of the assumption that the components are uncorrelated in the population. We cannot reject the possibility that the ratios may still be correlated only as a function of the common term, because we do not know how strong that artifactual correlation would be under the different condition of correlated components. Moreover, there is a paradox in this procedure: in order to apply formulas (2) and (3) we must already have data on the components; hence, the test is unnecessary, because we will already know whether the components are correlated.

In sum, it is not clear to me that Pearson's null comparison can properly be used as a test of the null hypothesis that a given empirical ratio correlation is not greater than some calculated value that would be expected to occur with those data if the correlation were due entirely to the operation of a common term in the two ratios.

### 3. SIMULATION TECHNIQUES

Since Pearson's formula provides a poor approximation, Chayes suggests substituting a simulation technique as an alternative means of determining a null value for the correlation of two ratios formed from three uncorrelated components. For a large number of simulated cases, Chayes generates quasi-random values for each of three components in such a way as to reproduce the means and standard deviations that these components have in some empirical sample data. These three components are then combined for each case into two ratios having a common term, and then those constructed ratio values are correlated. Chayes proposes that this correlation provides a proper substitute for the null correlation figured by formula (3). When the coefficients of variation for all components are small, the null correlations computed by formula (3) and by Chayes' simulation technique should be about the same. Otherwise, Chayes' technique is to be preferred. However, since Chayes' simulation technique is directly analogous to the Pearsonian null comparison, the criticisms already made of that comparison also will apply to the simulation technique when it is being used as a substitute for Pearson's null comparison.<sup>1</sup>

One very recent discussion of ratio correlation, using simulated

data, includes an application to the deterrence problem as one of several illustrations. MacMillan and Daft produce simulated data for crimes ( $C$ ), imprisonments ( $I$ ), and population ( $P$ ) to test for bias in the correlation of  $I/C$  and  $C/P$ . They criticize earlier simulations (Chiricos and Waldo) for generating  $C$ ,  $I$ , and  $P$  to be independent of each other, and argue that "the appropriate simulation experiment to test for bias is to have  $C$ ,  $P$ , and  $I$  increase at the same rate across jurisdictions." This is the point at which their logic departs from that of all previous simulation research on ratios (Chayes; Chiricos and Waldo; Freeman and Kronenfeld; Logan, a; Tittle), in which the whole purpose is to allow the components to vary independently of one another.

For 100 cases, MacMillan and Daft set  $C = a + .10P + u$  and  $I = a + .31C + u$ , where  $a$  = a constant intercept and  $u$  = a small random error term.  $P$  is set randomly between 10,000 and 100,000. Thus,  $I$  is a strongly predetermined linear function of  $C$ , which in turn is a linear function of  $P$ . By varying the intercepts, they predetermine the relation of the ratios to their components, and thereby to each other. Whenever the intercept for the production of  $C$  from  $P$  is zero or when the intercept for the production of  $I$  from  $C$  is zero there will be no correlation between  $I/C$  and  $C/P$ . In the former case, for example,  $C$  will always be about 10 percent of  $P$  (adjusted by error term), regardless of the size of  $P$ . Since  $C/P$  will be constrained to remain virtually constant at about .10, it will not correlate with  $I/C$  (or anything else). MacMillan and Daft seem to believe that only if the impossible occurred and  $I/C$  correlated with  $C/P$  under these circumstances would we have any evidence of a definitional dependency, or common term, effect.

MacMillan and Daft have overconstrained their data. They have predetermined, in the above case of a zero intercept, that  $I/C$  will not vary much from .10, regardless of the size of  $P$  or  $C$ . It may be sensible to place limits on the generation of the separate components, but MacMillan and Daft have placed severe limits on the relations among the components. This goes against one of the main purposes of producing ratios from simulated data, which is to measure the effect of a common term when the components are known to be independent.

In summary, MacMillan and Daft constrain not only the components, but their ratios. They then discover with scatterplots and correlations that the ratios behave in the ways in which they have been constrained to behave. This is used to prove that they could not have behaved otherwise. Hence (they say), the use of ratios is valid and unbiased with respect to the effect of a common term.

There is another way of interpreting a simulation technique, in terms of measurement errors, that seems at least intuitively compelling. In producing quasi-random component values, it would seem desirable not only to reproduce the means and standard deviations of the empirical data, as

Chayes does, but the ranges as well, particularly with data that cannot take on negative values or values above or below a certain range because of some practical or theoretical limits.<sup>2</sup> With all these restrictions on the random generation, it is as if we took the actual empirical data and scrambled the values among the cases, thereby creating massive measurement error.<sup>3</sup> For example, our new "case" representing Alabama might be randomly assigned California's number of prison admissions ( $A$ ), Ohio's number of crimes ( $C$ ), and Nevada's population size ( $P$ ). These components would be random with respect to each other, and the resulting ratios ( $A/C$ ;  $C/P$ ) would be meaningless.

Thus, if it could be shown that an empirical correlation found between conceptually meaningful ratios formed from real-data components is about the same as that which is found between conceptually meaningless ratios formed by first scrambling the component data, this would certainly raise troublesome questions about the nature of the empirical correlation, even if it could not be said to definitely prove that the empirical correlation is spurious or artifactual.

#### *A Scrambled-data Simulation Technique*

Using computer programs for random number generation and a Fortran sorting procedure, the real values for admissions to prison ( $A$ ), crimes ( $C$ ), and population size ( $P$ ) for 48 states were scrambled in a random fashion and then recombined into ratios  $A/C$  and  $C/P$ . These scrambled-data ratios were then correlated and their scatterplots examined. As was the case with the original (unscrambled) data,  $A/C$  and  $C/P$  correlated negatively, at moderate strength, for all felony categories. However, examination of the scatterplots showed that for every felony category a few extreme outliers gave the data a strong curve, asymptotic on both axes. This curve was present in the original data, but became more pronounced with the scrambling and recombining procedure. To adjust for the skewed distributions of  $A/C$  and  $C/P$ , and the curvilinearity of their correlation, log transformations were made on each ratio, both in the original data and in the scrambled data.<sup>4</sup>

The program written to scramble the values of  $A$ ,  $C$ , and  $P$  randomly, recombine them into  $A/C$  and  $C/P$ , and correlate these scrambled ratios, was looped 1,000 times. This produced a sampling distribution of the correlations that would obtain between  $A/C$  and  $C/P$  in a hypothetical population where  $A$ ,  $C$ , and  $P$  were distributed as they are in the real world but without any correlation to each other. Since in the scrambled data  $A$ ,  $C$ , and  $P$  were not related<sup>5</sup> and the resultant ratios  $A/C$  and  $C/P$  had no substantive meaning or causal relation to each other, the sampling distribution of scrambled-data correlations represents correlations due entirely to the artifactual effect of having a term common to both ratios. This sampling distribution can thus be used to test whether the empirical correlations



found in the original data are significantly different from the average correlations that occur artifactually in the corresponding sets of scrambled data. Table 1 presents the results of this test.

Only for sex offenses is the original (real-data) correlation significantly greater (in the predicted negative direction) than what occurs under the conditions of the scrambled-data simulation. For most felonies, the scrambled-data correlations are as strong or stronger than those in the original data. This does not prove that the real-data correlations are artifactual, but it does at least shake our faith in the results of published deterrence research to think that they could be replicated using data so randomly scrambled as to be meaningless. This is more remarkable than a roomful of monkeys pounding eternally on typewriters and eventually reproducing a work of Shakespeare. This is like 1,000 monkeys shredding and re-composing a data set and each one tending, on the average, to reproduce the findings of the original researcher. Does this make researchers out of monkeys? Or monkeys out of researchers?

In any case, this simulation experiment is sufficient to raise serious questions about the reality of at least some of the original ratio correlations that have been published in deterrence research. But although simulation tests may demonstrate a very real possibility of spuriousness in the correlation of deterrence ratios, they do not prove its existence. Simulation tests follow the logic of a syllogism in which the major premise is this: If *A*, *C*, and *P* are constrained in certain realistic ways but are random with respect to each other, then *A/C* and *C/P* will correlate to a certain degree as a function of their common term, plus the constraints on *A*, *C*, and *P*. Thus if

**Table 1.** ORIGINAL VS SCRAMBLED DATA CORRELATIONS BETWEEN CERTAINTY OF IMPRISONMENT (A/C) AND CRIME RATE (C/P) (LOG TRANSFORMED)

Offense	Scrambled-Data Correlations		Original-Data Correlation	p-value*
	Mean	Standard Deviation		
Total felonies	-.55	.08	-.47	.37
Homicide	-.57	.08	-.19	<.001
Sex offenses	-.60	.07	-.76	.02
Robbery	-.62	.07	-.67	.44
Assault	-.69	.05	-.71	.76
Burglary	-.54	.08	-.45	.32
Larceny	-.45	.10	-.26	.04
Auto theft	-.38	.11	-.31	.52

\*Probability of a value (at least) as extreme as the original-data correlation, under the sampling distribution of 1,000 scrambled-data correlations.

a significantly different correlation occurs in our empirical data, we can conclude that something else, beyond the common term and the constraints, is affecting the relationship between the ratios. But, as with any syllogism, affirming the consequent does not lead to any valid conclusion. If the empirical correlation is close to what occurs artifactually in simulated data, that suggests the possibility but not the valid conclusion that the empirical results are artifactual.

#### 4. DECOMPOSITION INTO COMPONENT COVARIANCES

Schuessler calls attention to the point that the relation between two ratios can be expressed as a function of the relation among the components with exact rather than approximate results if the variables are first converted by log transformations. This is because  $\log(A/C) = \log A - \log C$ , and, unlike the correlation of ratios, the correlation of difference terms may be expressed with exact, rather than approximate, results as a function of the moments of the component terms (Schuessler, b, 380). Schuessler notes (a, 217) that with log transformations, the covariance of  $A/C$  and  $C/P$  can be expressed as

$$\sigma_{12} = (\sigma_{ac} + \sigma_{cp}) - (\sigma_c^2 + \sigma_{ap})$$

where  $\sigma_{12}$  = the covariance of  $\log(A/C)$  and  $\log(C/P)$ ,

$$a = \log A - \overline{\log A},$$

$$c = \log C - \overline{\log C},$$

$$p = \log P - \overline{\log P},$$

$\sigma_{ac}$ ,  $\sigma_{cp}$ ,  $\sigma_{ap}$  = the respective covariances of  $a$ ,  $c$ , and  $p$ , and

$\sigma_c^2$  = the variance of  $c$ .

Thus, the covariance of log transformed ratios can be expressed as a direct function of sums and differences of component covariances and the common term variance. When this is done, "it is possible to gauge the statistical weights of the constituent covariances in the covariance of ratios (as logs)" (a, 216).

Like Pearson's formula, Schuessler's technique expresses the correlation of two ratios in terms of the moments of the constituent components, but Schuessler's decomposition procedure has two distinct advantages. First, with the use of log transformations the results are exact, and thus not subject to the occasionally serious distortions of Pearson's approximation. Second, the decomposition procedure does not require the making of unrealistic assumptions in order to determine the contribution of the common term to the ratio correlation.

For the purpose of illustration, Schuessler (a, 217-9) took data on imprisonment and offense rates (for all *Index* felonies combined) for 1966

and analyzed them by this decomposition technique. Plugging his empirically obtained values into formula (4) yielded:

$$-.03 = (.24 + .22) - (.28 + .21).$$

A comparable outcome is obtained with our deterrence data. For total felonies:

$$-.10 = (1.13 + 1.14) - (1.37 + 1.00).$$

Schuessler remarks of his finding that it "reflects the weight of the common term variance (.28) relative to the approximately equal covariances. An implication is that a positive relation between the admission rate and the crime rate is unlikely in the absence of a weak relation between the numbers of admissions and the population. Such a relation could occur, but it is not very probable" (a, 219).

It is important to note that Schuessler's conclusion that the negative covariance of the ratios reflects the relative weight of the common term variance and the component covariances could just as easily be stated the other way around. It depends on what is taken as given. (Should we say that  $2 + 2 = 4$ , or that  $4 - 2 = 2$ ?) We could say that *given a negative relation between  $A/C$  and  $C/P$* , we are not likely to find a large variance in  $c$ , or a strong covariance between  $a$  and  $p$ , compared to the other component covariances (when data are transformed to logs). The reversibility or symmetry of the decomposition procedure is noted by Schuessler in his introduction (a, 203), where he indicates that it may be "instructive to analyze ratio variables in terms of their components, *and vice versa*" (emphasis added), and again in his concluding discussion, where he emphasizes that "the decomposition of moments of ratios into moments of components does not carry the implication that components are the causes of ratios, or vice versa" (226).

However, this point deserves even more emphasis than Schuessler gives it, because anything less than a rigorous reading of the literature on ratio correlations leaves the strong impression that expressing a correlation between ratios in terms of the moments of the components is the same as showing that the former is *due to* the latter, which is not necessarily the case. There is no inherent reason why the (co)variances of components should be said to account for ratio correlations, rather than vice versa.<sup>6</sup>

Analytically, of course, apart from any substantive interpretation, the correlation and the component (co)variances constrain each other. Which way it will be stated in any given substantive example depends on the theoretical context and meaning of the component and ratio variables.

The decomposition exercise is thus analytic, rather than explanatory. Technically, it is tautological, although it may occasionally be a revealing and useful tautology. For this reason, Schuessler cautions that decomposi-

tion should not be done for its own sake, but only to test a hypothesis or to clarify an otherwise puzzling relationship.

With regard to the question of a possible artifactuality in the correlation of the deterrence variables  $A/C$  and  $C/P$ , it is not enough to be able to express the covariance of these ratios (after log transformations) in terms of moments of components, or vice versa. We still want to know what the correlation of  $A/C$  and  $C/P$  would be if it were not for the statistical effect of the common term—i.e., if the effect of that common term could somehow be removed or adjusted for. This adjustment can be accomplished through partialling, or residualizing, procedures.

## 5. PART CORRELATION AND USE OF RESIDUAL SCORES

One solution to the common-term problem that has been suggested in the deterrence literature (cf. Logan, b) is the use of part correlation. In part correlation, one variable is related to a second variable from which the effect of a third variable has been removed. Thus, the part correlation  $r_{1(2.3)}$  correlates one variable with the residuals of a second, as opposed to the partial correlation  $r_{12.3}$ , which correlates the residuals of each of the first two variables after they have each been regressed on a third. The general formula for part correlation is:

$$r_{1(2.3)} = \frac{r_{12} - r_{13}r_{23}}{\sqrt{1 - r_{23}^2}}, \quad (5)$$

which expresses the correlation between 1 and the residuals of 2 regressed on 3 (Dubois, 60–62; McNemar, 186). Its significance can be tested by the formula:

$$F = \frac{r_{1(2.3)}^2}{(1 - R_{1.23}^2) / (N - 3)}, \quad (6)$$

where  $R_{1.23}^2$  is the multiple correlation of 1 with 2 and 3 (McNemar, 322). The choice between part and partial correlation may be arbitrary, or it may in some cases be guided by conceptual or theoretical considerations in the specification of the model.<sup>7</sup>

In terms of the present problem, the part correlation  $r_{C/P(A/C.1/C)}$  expresses the correlation of crime rate with certainty of punishment after the effects of the common term,  $C$ , have been removed from the certainty measure.  $1/C$  is used here instead of  $C$  in order to meet the linearity assumption.  $A/C$  is a curvilinear function of  $C$ , but a linear function of  $1/C$  (Fleiss and Tanur, 44).<sup>8</sup>

Fuguitt and Lieberman (140) express reservations about the part correlation technique and conclude that "until more work is done on the

rationale for part or partial correlations to remove the effect of a common term we prefer the straightforward Pearson procedure." Citing Fleiss and Tanur, they point out that it can be shown for the special case of data generated such that  $A$ ,  $C$ , and  $P$  are unrelated, the value of  $r_{A/C \cdot C/P \cdot 1/C}$  and of  $r_{A/C \cdot C/P \cdot 1/C}$  will be 0. Hence, for those data the part correlation  $r_{C/P(A/C \cdot 1/C)}$  will also = 0, thus providing a rationale for the use of the part correlation technique "but under the assumption that  $[A, C, \text{ and } P]$  are independent. Hence it is not clear that Logan has circumvented the objection that an independence assumption is unreasonable" (emphasis added).

However, this confuses the conditions under which a technique is proven to work, with the conditions under which the technique may be applied. Fleiss and Tanur created random data in order to demonstrate that a partial correlation would be zero when applied to data where it was known that it should be zero. Unlike the Pearson procedure, however, the application of the part correlation to real data does not *require* that we assume, even provisionally for purposes of test, that the ratio components are uncorrelated. Its advantage is that it can be applied to data where the components are known to be correlated.

The rationale for the use of part correlation in the analysis of ratio variables is the same as the rationale for any use of part or partial correlation: (1) Let  $A$ ,  $C$ , and  $P$  vary as they do in reality; (2) hypothesize that some part or all of the correlation between  $A/C$  and  $C/P$  is the spurious result of the simultaneous relation of  $C$  to both  $A/C$  and  $C/P$ ; (3) adjust the values of  $A/C$  by regressing on  $1/C$  and taking the residual scores as our new values; (4) compute a part correlation as expressing the covariation between the two ratios that cannot be attributed to the operation of the variable being statistically controlled. No difference is made by the fact that in ordinary partialling analyses the controlled third variable is thought to be *causally* antecedent to or intervening between the other two variables, whereas in the present problem, the common term is thought to be definitionally or analytically related to the ratio variables of interest, as well as perhaps causally prior to one of them ( $A/C$ ).

Thus, it would seem that part correlation ought to provide a sensible answer to the question of how  $A/C$  and  $C/P$  would correlate to the absence of any effect of the common term. In Table 2, columns 1 and 2 compare the zero-order and part correlations as calculated from the original imprisonment data. When adjustments are made for the effects of the common term, the correlations reduce considerably but do not vanish completely.<sup>9</sup>

Columns 3 and 4 of Table 2 illustrate the application of part correlation to the scrambled data, where the values of  $A/C$ ,  $C/P$ , and  $1/C$  are known to be meaningless and unrelated except for the effect of the common term. As expected, the part correlations for those data reduce to an average value of zero, though they may depart from zero by chance in any one trial.

**Table 2.** ZERO-ORDER VS PART CORRELATIONS BETWEEN CERTAINTY OF IMPRISONMENT (A/C) AND CRIME RATE (C/P), USING ORIGINAL AND SCRAMBLED DATA VALUES (LOG TRANSFORMED)

Offense	Original Data		1000 Scrambled-Data Part Correlations	
	$r_{\frac{A}{C} \frac{C}{P}}$	$r_{\frac{C}{P} (A/C, 1/C)}$	Mean	Standard Deviation
Total felonies	-.47	-.30	.00	.09
Homicide	-.19	-.11	.00	.09
Sex offenses	-.76	-.22	.00	.10
Robbery	-.67	-.20	.00	.08
Assault	-.71	-.13	.00	.08
Burglary	-.45	-.12	.00	.10
Larceny	-.26	-.14	.00	.10
Auto theft	-.31	-.22	.00	.10

In comparing column 2 with columns 3 and 4, it can be seen that several of the original-data part correlations are low enough that their reliability or reality might be questioned. The values for homicide, burglary, assault, and larceny are that low, while the values for total felonies, sex offenses, auto theft, and robbery remain high enough to have confidence in. The standard used here for "confidence" is two standard deviations (column 4) around the mean of zero.

In sum, then, the application of part correlation techniques to these data suggests that for all eight offense categories, the common term may be acting as an artificial inflator of the correlations between sanction rates and crime rates. For about half the felony categories, there may even be no non-chance correlation once the common term is removed.<sup>10</sup>

Part correlation provides a method of testing for the existence and degree of artifactual<sup>11</sup> common-term effects in a simple two-variable analysis. However, it is still possible that the operation of other variables may be obscuring the relation between the two major variables of interest. Thus, a non-zero part correlation might be shown to be spurious by controlling for other variables. Alternatively, a zero part correlation might be shown to be the net result of counteracting effects of some third variable or set of variables that mask a causal relation between the two major variables. The desirability of entering additional variables into the analysis indicates the need for a simple method of adjusting the data to remove common term effects prior to the application of conventional techniques for multivariate analysis. Residualization<sup>12</sup> seems to provide this solution.

It has been argued (DuBois; Fuguitt and Lieberman) that a ratio may be seen as a kind of residualized variable, under certain conditions. Stated

conversely, a residualized score may sometimes provide a substitute for a ratio measure. Fuguitt and Lieberman (138) go so far as to suggest that residual scores will generally be preferable to ratios except under those certain conditions where the two are equivalent. Thus, it might be thought that we could just respecify the measure of certainty of punishment, not in terms of a ratio ( $A/C$ ), but in terms of a residual ( $A.C$ ).

Whether a ratio or a residual is preferable as a measure of a concept, however, depends in good part on the nature of the concept. It is never a purely methodological problem. In the case of certainty of punishment, a residual score would be less faithful to the meaning of the concept being measured. The residuals of  $A$  regressed on  $C$  are the values of  $A$  relative to what one would predict from the value of  $C$ . The ratio  $A/C$ , in contrast, refers to the value of  $A$  relative to the total potential for  $A$ . It constitutes a proportion that can range from zero to one. This makes the ratio measure much closer to the concept of *certainty* or *probability* of punishment, and thus preferable to the simple residual of  $A$  on  $C$ .<sup>13</sup>

It is possible, however, to combine the conceptual advantage of a ratio measure with the statistical benefits of residualization. The solution is simply to remove the effects of the common term from the ratio itself, rather than from the numerator only. Thus, we could use as our independent variable, "residual arrest rate," defined as the residuals of  $A/C$  regressed on  $1/C$ . This residual variable, freed of possible measurement contamination with the dependent variable, could then be treated just like any other variable in a multivariate analysis of the determinants of crime rate.<sup>14</sup>

## Summary and Conclusions

A recent and rapidly expanding line of deterrence research, which has received much attention with potentially significant policy implications, has involved the cross-sectional correlation of crime rates with measures of certainty of punishment. Where the index used to measure certainty of sanction has been the ratio of prison admissions or arrest clearances to crimes known to police ( $A/C$ ), there arises the possibility of definitional contamination with the dependent variable, crime rate ( $C/P$ ), due to the presence of a common term in the two measures. Arguments to the effect that  $A/C$  and  $C/P$  are meaningful as ratios, and that there is no inherent bias in their correlation are valid but insufficient. They cannot rule out the possibility of an artifactual correlation resulting from measurement error in conjunction with the common term in the two ratios.

In a simulation experiment, the results obtained with some previously analyzed deterrence data (Logan, b) were replicated after randomly scrambling the components  $A$ ,  $C$ , and  $P$  in such a way that the resulting ratios were meaningless and unrelated, but for the effect of the common

term. These results do not prove that the original empirical data are meaningless, nor that previously published empirical correlations are artifactual. However, they do at least raise questions about the reality of the negative correlations between *A/C* and *C/P* found in much deterrence research. Therefore, several other techniques for dealing with problems in ratio correlation were examined, with an eye toward their applicability to deterrence data.

Analytic techniques have been used to express the correlation between ratios either approximately, in terms of the moments of the components (Pearson), or exactly, with log transformations, in terms of the (co)variances of the components (Schuessler, a,b). These techniques, however, must not be interpreted as showing that the characteristics of the components *produce* the correlation between the ratios, any more than vice versa. Thus, they do not provide a means of demonstrating that a correlation between two ratios is a spurious result of the operation of a common component. This is not to deny the general utility of these analytic techniques,<sup>15</sup> but only to assert that they do not provide a satisfactory test for common-term spuriousness.

As a test for this source of spuriousness, the technique of part correlation is suggested. Part correlation allows one to remove the effects of a common term from one of the two ratios, thereby correlating the residual variation in that ratio with the full variation in the other ratio. An extension of part correlation to the multivariate case would be to residualize one ratio on the common term, creating a new variable (the residual score). This score would then be used in all further analyses as a measure of the first variable that should have no definitional contamination with the second variable. Additional variables could then be entered into the analysis using standard multivariate techniques.

The capability of part correlation to discount the artifactual effect of a common term was verified by computing part correlations on the randomly scrambled data for *A/C* and *C/P*, since in those data it was known that the two ratios correlated solely by virtue of the common term. Across 1,000 trials, the average part correlation between these ratios was 0.00, as expected. When part correlations were run on the original, unscrambled data, the correlations were greatly reduced from their zero-order levels. For about half the felony categories, the part correlations were within a range where they could have occurred by chance.

Two general conclusions—one substantive and one methodological—may be offered. The substantive conclusion is that confident assertions that deterrence works (e.g., Tullock) are premature. The questions raised in this analysis ought to at least encourage some more systematic examination of the issue of possible artifactuality in the findings of much recent and ongoing deterrence research. The general methodological conclusion is that the most useful test for the existence of a common-term artifact in a



bivariate ratio correlation is the technique of part correlation. Where additional variables are included, the best approach would seem to be to residualize one of the two ratio variables by regressing it on the common term, then to proceed from there.

## Notes

1. As an exercise, Chayes' simulation technique was compared to the empirical deterrence correlation for total felonies. For 1,000 simulated cases, values of  $A$ ,  $C$ , and  $P$  were randomly generated in such a way as to reproduce the means and standard deviations of these components in the empirical data. The ratios formed by these quasi-random components correlated at .003, compared to the empirical correlation of  $-.44$ . By the same logic that rejected the Pearsonian comparison, however, this ought not to be regarded as a test of the artifactuality of the empirical correlation.
2. Previous attempts in the deterrence literature to use simulation techniques to investigate the possibility of a common-term artifact have either failed to set any limits at all on the simulated data (Tittle), or constrained the simulated components only by the ranges of the real data (Chiricos and Waldo; Logan, a)
3. That measurement error is a key source of bias in ratio correlation is more rigorously documented by Long. Both underestimation and random error in the measurement of  $C$  would create bias in the correlation of  $A/C$  and  $C/P$ , as would overestimation in the measurement of  $A$ . That official statistics on crime ( $C$ ) underestimate is well established. That admissions to prison ( $A$ ) could be overestimated has been completely overlooked in deterrence research. National Prisoner Statistics on admissions to prison include not only new commitments (which are relevant to certainty of punishment) but also transfers and technical parole revocations (which are not). The type of measurement error produced here, however, is not the normal type of random error, since it applies not to the measurement of the component terms, but to the accuracy of their recombination into ratios. Errors that may exist in the original component measurements still exist, of course.
4. In both cases, scatterplots showed that the transformations made the relations linear.
5. The truth of this was verified empirically with the scrambled data.
6. Thus, even Schuessler is simplifying (by leaving out "and vice versa") when he concludes that "decompositions are a reminder that a restriction on the correlation of ratios inheres in one or more restrictions on the component variables from which the ratios were formed" (b, 394).
7. Where the two ratios involve common deflations or standardizations of different components, then partial correlation seems appropriate, and simpler. Where the conceptual focus is on  $A$  relative to  $B$ , rather than on  $B$  relative to  $A$ , then  $A$  ought not to be partialled out. In practice, this may generally mean that one should partial common terms out of denominators and not numerators, but perhaps not necessarily. Rather than attempting to establish some abstract methodological rule, it would be better to ask in each case whether it makes conceptual sense to partial the effect of the common term out of the ratio. If it does so for both ratios, use partial correlation, if it makes more sense for one ratio than for the other, use part correlation.
8. The effect of  $C$  (or rather,  $1/C$ , because of the curvilinearity problem) is removed from  $A/C$  rather than from  $C/P$  because this makes more sense substantively. We are interested in the effects of that part of the variation in certainty of sanction beyond that which is a function of number of crimes.
9. The part correlations may even be understated, since the log transformation procedure does not seem to have the same effect of improving the fit of the part correlations as it had with the zero-order correlations. In fact, log transformations on  $A/C$ ,  $C/P$ , and  $1/C$  reduce the values of the part correlations below what is obtained with untransformed data. Using the untransformed data, the values in column 2 of Table 2 would be, in order:  $-.34$ ,  $-.12$ ,  $-.39$ ,  $-.45$ ,  $-.30$ ,  $-.42$ ,  $-.33$ ,  $-.22$ . Those values are considerably stronger for most offenses, but still below the corresponding zero-order correlations for all offenses except assault. Using untransformed data, the zero-order correlations, in order, are:  $-.44$ ,  $-.20$ ,  $-.55$ ,  $-.56$ ,  $-.24$ ,  $-.45$ ,  $-.38$ ,  $-.29$ .

10. This is in contrast to earlier conclusions based on part correlations (Logan, b), where the effects of log transformations on part correlations were not considered, nor were comparisons made with the application of part correlation to scrambled data.

11. One reviewer suggested that these effects be referred to as structural rather than artifactual. This was based on the reasoning that  $C$  would never be purely random with respect to  $A$  and  $P$  when using real data. Rather,  $C$  for any state would probably be smaller than  $P$  for the smallest state but bigger than  $A$  for most states. This structural constraint would inflate the negative correlation between the two ratios. This is plausible, but it does not seem important to me at this point to develop the distinction between artifactual and structural effects. The technique of part correlation would serve as a check on their existence under either name.

12. Residualization refers to the procedure of regressing one variable on another and creating a new variable (the residual) equal in value to the difference between predicted and actual values of the first variable. Thus, "residual  $Y$ " is the deviation in  $Y$  above or below what would be predicted from the associated value of  $X$ . Residual  $Y = Y - \hat{Y}$ , where  $\hat{Y} = a + bX$  (the regression of  $Y$  on  $X$ ).

13. While it is argued above that there is a basic *conceptual* difference between a ratio and a residual, there is also an empirical difference. The two are equivalent only under limiting conditions (Bollen and Ward, Fuguitt and Lieberman, Kuh and Meyer). The following pairs of coefficients contrast the correlation of crime rate ( $C/P$ ) with certainty of punishment, measured first as a ratio ( $A/C$ ) then as a residual ( $A - C$ ):  $-.44$  vs  $-.09$ ,  $-.20$  vs  $.06$ ;  $-.55$  vs  $-.21$ ;  $-.56$  vs  $-.13$ ;  $-.24$  vs  $.14$ ,  $-.45$  vs  $-.07$ ,  $-.38$  vs  $-.13$ ;  $-.29$  vs  $-.15$ .

14. For the bivariate case, calculating the part correlations by formula (5) produces exactly the same results as first calculating the residual values for ( $A/C - 1/C$ ) and then correlating these residual scores with  $C/P$ . Therefore, the use of "residual certainty" ( $A/C - 1/C$ ) as the measure of the independent variable ought not to cause any problems in any multivariate analyses.

15. Schuessler's discussions, in particular, demonstrate that it can often be quite illuminating to decompose a ratio correlation into the covariances among the components and the variance of the common term. This would be particularly helpful where a ratio correlation that did not make any substantive sense could be expressed in terms of covariances among the components that did make sense.

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# Compliance Under Threat of Severe Punishment\*

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## ABSTRACT

*This study examines the role-related conditions which generate a target's compliance with a source when threatened with genuine, severe punishment. Given the ethical restrictions of laboratory experimentation, cases of robbery are used as a data base. Compliance is affected by the source's capacity to punish, intent as to the use of force, and the target's capacity to oppose and ability to comply with the source's command. If the source is perceived as capable of inflicting threatened punishment and as making punishment contingent on opposition, and if the target perceives self as incapable of effective opposition and able to comply, the target will comply. Some implications of this research are discussed.*

Coercion requires two roles, a source and a target. In coercion, the source exacts compliance from the target, despite a conflict of interests, by means of actual or threatened punishment. A central problem addressed by social scientists is to determine the conditions under which the target will comply with the source given a threat of punishment. Two bodies of research consider this problem. The first, conducted primarily by experimental social psychologists, studies the social influence process in highly structured situations, such as games (cf. Tedeschi et al., c, d). The second body of research, conducted primarily by sociologists, examines the process of deterring individuals from crime (cf. Gibbs, b; Zimring and Hawkins).

These bodies of research identify a number of conditions generating compliance. Some involve background features of the participants, such as their social status (Foley and Tedeschi). Others involve features of the participants' roles in the transaction. As to role-related conditions, four hypotheses can be derived from the social influence and deterrence literature. (1) *The greater the severity of the threatened punishment, the greater the likelihood the target will comply* (Ehrlich; Gibbs, a; Horai and Tedeschi; Miche-

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ner and Cohen; Smith and Leginski; Tedeschi et al., a). (2) *The greater the source's capacity to inflict threatened punishment, the greater the likelihood the target will comply* (Levinger; Michener et al., a, b; Tedeschi). Source capacity means the degree to which the source *can* inflict threatened punishment owing to the possession of needed resources. (3) *The more believable the source's threat of punishment, the more likely the target will comply* (Chiricos and Waldo; Gibbs, a; Horai and Tedeschi; Michener et al., b; Tittle). Threat believability is the probability that the source *will* punish opposition.<sup>1</sup> (4) *The lesser the target's capacity to oppose the source, the greater the likelihood the target will comply* (Michener et al., b; Tedeschi et al., b). Target capacity refers to the degree to which the target can defeat the source by virtue of possessing the necessary resources.

While the social influence and deterrence research identify some major conditions producing compliance, they have not adequately determined the role-related conditions generating compliance in situations where threatened punishment is genuine and severe. Because the social influence research generally employs laboratory experimentation, there are limits to the kinds of punishment researchers can use. The target can be threatened with severe yet imaginary punishment, e.g., taking away large sums of play money, or real but mild punishment, e.g., withholding small earnings from task performances. For ethical reasons, the target cannot be threatened with genuine, severe punishment. Although the deterrence research examines situations where the target is threatened with genuine, severe punishment, such as execution, it suffers from two limitations. It fails to establish that the target complies with or breaks a law because of the threat of punishment, and it fails to examine the decision-making process involved in the target's response to the threat. Both limitations stem from the fact that the deterrence research usually analyzes aggregate data, studying the relationship between properties of the source, such as the severity of threatened punishment, and crime rates. At this level of analysis, it is difficult to know whether the target's action is due to the threat of punishment. Several sociologists have questioned whether compliance with a law is due to threatened punishment or to socialization or some other nonlegal condition (Erikson et al.; Meier and Johnson). Furthermore, aggregate analysis generally excludes the target's decision-making, and this makes it difficult to determine what the target considers in deciding between compliance and opposition (Henshel and Carey).

This investigation attempts to determine the role-related conditions which generate compliance in situations where the target is threatened with genuine, severe punishment. Given the ethical restrictions of laboratory experimentation, cases of coercion must be drawn from a natural setting. Given a need for data reflecting role-related conditions, the participants' backgrounds must be controlled. Robbery is a type of transaction meeting these needs. In robbery, the source typically threatens the target

with genuine, severe physical punishment. Also, the impact of the participants' background characteristics can be reduced to a degree. Because robbery usually is a fleeting transaction between strangers, the participants have little background information about one another; they must act on the basis of information flowing from their interaction. In this paper, I will first examine the role-related conditions which targets claimed were pivotal to their decisions to comply or oppose. Then I will use these findings to assess the hypothesized relations between compliance and source capacity, threat believability, and target capacity.

### **Sample and Method**

Robbery is a transaction in which a source (offender) unlawfully takes goods from a target (victim), against his or her will, by means of force.<sup>2</sup> By definition, robbery involves a conflict of interests between the source and target; it is an exploitative transaction in which the source attempts to gain at the target's expense.

The data were drawn from cases of robbery and attempted robbery over a one-year period in one Texas city. Sampling was of a multi-stage design. Between February 1976 and March 1977, 732 cases of robbery and attempted robbery were reported to the police. Cases occurring in alternate months of that period were selected for analysis, producing a roster of 354 cases. Some of these cases were then excluded: 54 because the police determined that the incident was not a robbery or attempted robbery; 39 because there was too little information about the incident; and 60 because the source began the robbery by using incapacitating force, a move designed to fell the target. Because my present concern is with the target's response to a threat of severe punishment, I have confined myself to the remaining 201 cases where the source opened with a command for compliance backed by an explicit or implicit threat of death or serious injury.

Two types of information were used to understand the target's response. First, official documents, including police reports and target statements, were used to reconstruct the target's role in the crime. These documents provided information on the participants, who said and did what to whom, the chronology of action, and the target's perceptions and decisions. Statements by witnesses and, in some cases, the source, helped to validate the target's account. Second, for some cases, the target's official account was augmented with intensive interviews. Thirty-five targets were interviewed about 39 cases. Interviewing not only provided a means of checking the target's official statements, but it also offered a way of probing the target about issues not covered in official statements.

## Role-Related Conditions Generating Compliance

In the cases studied, the source began the robbery with a command for compliance backed by a threat of death or serious injury for failure to comply. Targets responded to this move in one of two ways. In 122 cases, the target complied. In 79 cases, the target opposed, adopting a line of noncompliance (e.g., refusing to obey) or resistance (e.g., trying to defeat the source). The problem is to determine the conditions under which the target complied.

Targets' explanations of their compliance or opposition revealed that their responses depended on the values of several variables.<sup>3</sup> First, targets considered the source's capacity to inflict death or serious injury.<sup>4</sup> They referred to two features of the source which can be taken as elements of capacity: the strength of the source's coercive resources and the source's position to convert those resources into actual punishment. The greater the strength of the source's resources and the closer and more watchful the source, the greater the source's capacity. And the greater the source's capacity, the more likely the target was to comply. When the target thought the source had lethal resources,<sup>5</sup> such as a firearm or knife, and was in a position to use them, compliance was likely:

### *Target #4*

I wasn't going to try anything because he had a piece (firearm). When he's got a piece, you give him the money. That's all there is to it. If you try anything, he might shoot you.

### *Target #30*

I would've killed him if I could've gotten to my pistol. But . . . I couldn't do a thing, you know. He was standing right there watching me. And his pistol was loaded. I could see the steel bullets in the cylinder. And he was shaking so bad he might've shot me if I tried anything. If I had a clear chance, you know, I would've nailed him. But hell, I'm not going to risk my life for a few measley bucks.

An examination of the cases shows that the target complied in 75 percent of the 161 cases where he or she thought the source had lethal resources and was in close physical proximity. However, when the target thought the source was incapable of inflicting threatened punishment, because the source did not have lethal resources or was not in a position to use them, opposition was likely:

### *Target, Case 429*

He stuck his hand in his shirt and told me, "I'm sorry . . . but this is a holdup" I asked him, "Where's the gun?" I wasn't going to give him any money if he didn't have a gun. Sometimes robbers try to bluff you. They just say they have a gun, but they really don't. If he showed me a gun, I would have given him the money. He never showed me a gun, so I never gave him the money.



The target opposed in 95 percent of the 40 cases where he or she thought the source did not have a firearm or knife or was not in a position to use a weapon.

Second, targets considered the source's intent regarding the use of force. Targets believed the source either intended to inflict punishment for opposition or intended to inflict punishment regardless of the target's response. The assessment of intent depended on the target's interpretation of the source's gestures, and how those gestures were interpreted depended, in part, on the nature of the threat. Tedeschi identifies two types of threats. A contingent threat communicates the conditions under which the target will or will not be punished (e.g., "If you don't give me the money, I'll kill you" or "Give me your money and nothing will happen to you"). A noncontingent threat communicates the source's intended line of action without saying what the target can or cannot do to avoid punishment (e.g., "I'm going to kill you" or "I'm going to take your money"). When the source issues a contingent threat, the target is likely to assess the source's intent as inflicting punishment only for opposition. Given a capable source, when the target thinks force is contingent on opposition, compliance is likely. An analysis of the cases shows that the target complied in 68 percent of the 179 cases in which he or she thought the source intended to inflict punishment only for opposition. However, when the source issues a noncontingent threat, the target is likely to assess the source's intent as inflicting punishment indiscriminately. When the target thinks the source intends to punish indiscriminately, opposition is likely, regardless of the source's capacity. Here, opposition appears to be the best alternative, for it offers a chance to escape death or serious injury:

#### *Case 401*

The offender entered the grocery store and walked to the candy display located next to the cashier's counter, behind which the clerk was standing. Appearing to browse through the candy, the offender pulled a knife from his pocket and moved to the side of the cashier's counter. In a loud, excited voice, the offender stated, "I want all your money." The clerk stood motionless, trying to determine the offender's plans. The offender responded to the clerk's inaction by moving behind the counter, raising his knife above his head, and shouting, "I'm going to kill you and take the money." The clerk believed the offender intended to kill him and take the money from the cash register. Fearing impending death, the clerk grabbed the offender's arm and tried to knock the knife from his hand.

The target opposed in all of the 22 cases in which he or she believed the source intended to use force indiscriminately.

While the source's capacity and intent are critical to the target's response, they are not sufficient for explaining his or her action. There were 24 cases where the target opposed despite the belief that the source was capable and intended to inflict punishment for opposition. These cases

reveal two additional variables that affected decision-making, the target's capacity to resist and ability to comply. When targets talked about their decisions to comply or oppose, some spoke of their capacity to defeat the source. They made reference to two features of themselves which can be taken as elements of self-capacity: whether the target has resources to oppose effectively and whether the target is in a position to use them. The stronger the target's resources and the better the target's position to use them before the source could react, the greater the target's capacity. And the greater the target's capacity, the more likely the target will oppose. When the target believes he or she has lethal resources or strong allies and is in a position to mobilize them quickly, opposition is likely, regardless of the source's capacity:

*Case 460*

The offender walked into the office of the warehouse and confronted the manager. The manager asked the offender what he wanted. The offender then took a large hunting knife from his coat pocket and ordered the manager to give him the office's money. The manager knew that he could defeat the offender if he could move to the back room. The manager stated, "The money's in the back room. I'll go get it." With that, he turned and started for the back room, the offender following about 8 feet behind. As soon as he entered the back room, the manager grabbed a rifle resting against the wall. He spun around, aimed the rifle at the offender, and shouted, "Hold it."

Data on target capacity are incomplete, probably because some targets never considered it. But an examination of the cases for which information is available shows that the target opposed in 90 percent of the 20 cases where he or she considered self as having lethal resources or potent allies and being in a position to mobilize them.<sup>6</sup> However, when the target considers self incapable of opposition, for he or she lacks sufficient resources or is not in a position to use them, compliance is likely. The target complied in 81 percent of the 107 cases where he or she considered self incapable of effective opposition.

Finally, some targets considered their ability to comply with the source's demand. Compliance requires specific knowledge and skill, as well as control over the wanted goods. For example, if the source orders the target to surrender the money in the safe, the target must have knowledge of the safe combination and the skill to open it, and the safe must contain money. When the target is unable to obey the source's order because he or she does not have the wanted goods or lacks the knowledge and skill to surrender them, opposition is imperative, regardless of source or target capacity. An analysis of the cases shows that the target opposed in all of the 17 cases where he or she could not comply with the source. While inability makes opposition necessary, the way in which the target opposes depends on the capacity of the source and target. When the source appears

incapable of inflicting threatened punishment or the target considers self capable of defeating the source, opposition takes the form of resistance. However, when the source appears capable and the target considers self incapable, opposition takes a form of noncompliance. The target indicates that obstacles beyond control prevent compliance and therefore, because opposition is involuntary, punishment is unwarranted:

*Case 21*

The offender approached the target on the street. Without warning, the offender pointed a pistol at the target and said, "Don't move or I'll shoot." The shocked target responded, "Okay, I'm not moving." The offender then ordered, "Give me your wallet." The target stated, "I don't have a wallet." The offender stared at him. Fearing the offender would interpret this as intentional opposition and possibly shoot him, the target reached into his back pocket, removed some folded papers, held them out, and pleaded, "See, this is all I've got. Please don't hurt me."

This analysis shows that the target's response to a threat of severe punishment is affected by the source's capacity and intent and the target's capacity and ability. If the source appears capable of inflicting punishment and appears to make punishment contingent on opposition, and if the target considers self incapable of effective opposition and able to comply, the target will comply. But if the source appears incapable of inflicting punishment or intent on inflicting punishment indiscriminately, or if the target considers self capable of effective opposition or unable to comply, the target will oppose.

### **The Problematic Nature of Capacity, Intent, and Ability**

Although source capacity and intent and target capacity and ability affect the target's response, the values of these variables generally are problematic for both the researcher and the target. It is difficult for the researcher to measure these variables independent of the target's interpretations, because targets differ in their interpretive methods. For instance, when the source claims to have a firearm but fails to show it, some targets interpret the statement as genuine, an indication of high source capacity, whereas others take it as a bluff, a sign of low source capacity. While many targets take a noncontingent threat as indicating an intent to punish indiscriminately, others interpret it (particularly one where the source states "I want your money" or "Give me your money") as indicating an intent to punish only for opposition. When bystanders are present, some targets see them as potential allies and therefore define themselves as capable of defeating the source, but others do not consider comparable bystanders as allies and do not define themselves as capable. Thus, researchers can speak about the pivotal conditions generating compliance and the basic elements con-

stituting them, but they cannot identify fixed or invariant criteria of those elements.

Importantly, the values of these variables are problematic for the target. This may be due to the source's ambiguous or faulty communication, a common feature of communication in natural settings, or to the unexpected nature of the communication, a common characteristic of deviant exploitation (LeJeune and Alex). This also may be due to the interdependence of some variables. For instance, the target's capacity depends, in part, on the source's capacity. Whether the target is in a position to mobilize resources depends on the source's position; the source who is inattentive gives the target an opening in which to oppose.

For the most part, the pivotal conditions are emergent properties of the transaction, determined through focused interaction between the source and target. In some cases, focused interaction was minimal; the source demanded money, and the target briefly scanned the source, noted that he or she had a firearm or knife and only wanted the money, and immediately surrendered it. In most cases, one or more conditions were determined through a series of moves. Consider source capacity, for instance. In 52 cases, the source did not display a weapon or was not in a position to use it when he or she first threatened the target. Thinking the source lacked the capacity to kill or injure, the target balked or opposed. In turn, the source warned that opposition would meet with death or injury and, in the course of issuing the warning, brandished a weapon or moved into striking range. Recognizing the source had the capacity to inflict threatened punishment, the target complied:

#### *Case 139*

Just above a whisper, the offender told the clerk, "You see this bag here (referring to a sack he placed on the counter top)? Well, pick it up and give me only the cash." The clerk stared at him. The offender stated, "I have a gun. If you make a sound, I'll shoot you." The clerk looked at the offender but did not see a firearm. Believing the offender was trying to bluff her, the clerk responded, "The cash register is locked and I don't know how to open it." The offender replied harshly, "You know how to open it. I saw you open it." The clerk stood silent, thinking about how to manage the offender. The offender pulled a pistol from his coat pocket, pointed it at her, and announced, "Stop fucking around or I'll shoot you." The clerk saw the pistol. Realizing the offender could kill her, the clerk opened the cash register and handed him \$280.

The target's ability to comply also may be determined through interaction. In 7 cases where the target informed the source that he or she could not comply, the source accepted the claim as genuine and adjusted the demand so as to tap the target's existing ability. For example, the target said that he or she could not surrender the money in the safe due to ignorance of the safe combination, and the source adjusted the demand, requesting money in the cash register. In these cases, then, the target's ability to comply

depended on the source's adjustment of the demand to meet the current exigencies.

The pivotal conditions are flexible, shifting over the course of joint action. In 17 cases, for instance, the target's capacity shifted from low to high. In some of these cases, the shift occurred during the transfer of goods. Here, the armed source initially maintained careful watch over the target. As a consequence, the target could not mobilize resources to defeat the source before the source could react. But when the source shifted attention to the goods, the target surreptitiously secured a weapon and attacked:

*Case 440*

The offender turned his pistol on clerks #2 and #3 and stated, "Be cool. Don't try anything stupid." The two clerks stood silent. The offender then turned back to clerk #1, who was removing bills from the register and placing them on the counter top as ordered. Noting the offender was not watching him, clerk #2 grabbed a beer mug from the table. As the offender was about to scoop up the cash, clerk #2 moved behind the offender and hit him on the head with the mug. The offender fell, stunned

In another 8 cases, the assessed intent of the source changed. In these cases, the source opened with a command for compliance backed by a contingent threat. The target opposed, informing the source that he or she did not have the ability to comply. Rather than forfeit the robbery or adjust the demand, the source appeared to interpret the noncompliance as voluntary opposition and responded with a warning or prodding force (e.g., a slap or punch). This move was taken as a sign that death or serious injury would result regardless of the target's response. Given this shift in perceived intent, the target decided that resistance offered the only chance to preserve well-being:

*Target, Case 31*

I didn't know what to think when he hit me. I thought he was going to try to knock me out so he could look around for the money. Before he had a chance to hit me again, I hit him. . . . I was fighting out of self-defense.

Thus, coercion is a problematic transaction. Although the target attends to the source's capacity and intent as well as to self-capacity and ability, the values of these variables are neither clearcut nor rigid; they emerge and change over the course of interaction.

## Discussion

Focusing on robbery, this study sought to determine the role-related conditions which generate compliance under threat of severe punishment. The research findings can be used to assess the hypotheses drawn from the

social influence and deterrence literature relating compliance to source capacity, threat believability, and target capacity.

When the source orders compliance under threat of severe punishment, the target is put in a fateful position; compliance and opposition can lead to very different results. In deciding between these alternatives, the target weighs the relative merits of each and selects the one judged most useful in preserving well-being (cf. Tedeschi et al., c; d). This study found that four variables affect the target's response to a threat of genuine, severe punishment. First, the assessed capacity of the source to inflict threatened punishment shapes the target's action. The greater the assessed capacity of the source, the greater the likelihood the target will comply. When the target thought the source had lethal resources and was in a position to use them, the target was likely to comply. But when the target thought the source did not have lethal resources or could not use them, the target was likely to oppose. Second, the assessed capacity of the target to defeat the source affects the target's action. The lesser the assessed capacity of the target, the greater the likelihood the target will comply. When the target thought he or she lacked sufficient resources or was not in a position to use them before the source could react, the target was likely to comply. But when the target thought he or she had sufficient resources and could use them, the target was likely to oppose. Thus, the hypothesized importance of high source capacity and low target capacity is supported.

While this study provides support for the hypotheses on source and target capacity, it suggests that the assessment of capacity involves more than a consideration of resources. In the social influence literature, capacity generally is defined in terms of the possession of sufficient resources. This study found that targets consider an additional element in assessing capacity—a participant's position to use resources. In experimental games, position usually is controlled. In natural settings, however, the participants' positions may be problematic and consequential. In an international conflict, for instance, one nation's position to mobilize its forces against another may be as important to the target's decision-making as the size and quality of the nation's forces (Schelling). Position also helps to explain situations where the target responds in a manner inconsistent with the response expected given the resources of the source and target. Poor source position may explain cases where the target opposes even though the source appears to have sufficient resources (Wrong). For example, individuals who commit crimes carry out their actions when they think the police are not in a position to monitor them. Conversely, poor target position may account for cases where the target complies even though he or she has sufficient resources to defeat the source. Government agents have the resources to defeat terrorists who take hostages and demand goods under threat of killing the hostages, but if the agents are not in a position to

use their resources without jeopardizing the hostages' safety, they will refrain from opposition and try to negotiate a settlement (Schreiber).

Third, the assessed intent of the source shapes the target's action. When the target thinks the source intends to punish only opposition, the target is likely to comply. However, when the target thinks the source intends to punish indiscriminately, the target is likely to oppose. When the target thought compliance as well as opposition would bring death or serious injury, he or she always resisted; opposition offered a chance, however remote it may have seemed, to preserve well-being.

The social influence and deterrence research have not ignored the target's assessment of intent. For the most part, however, they have focused on only one type of intent. Intent typically has been defined in terms of threat believability—the probability that the source will inflict some or all of the threatened punishment for opposition. This study found that the target considers a more general type of intent—whether the source will inflict punishment for compliance as well as for opposition. In experimental games, general intent often is controlled; targets are informed that opposition makes them eligible for punishment. The deterrence research ignores general intent, but for good reason. Legal coercion—where the state commands citizens to comply with laws, despite a conflict of interests, under threat of punishment—is relatively rational. The process is bound by procedural rules which permit officials to punish individuals only if it is determined beyond a reasonable doubt that they violated the law. As a consequence, in most situations where citizens contemplate committing a crime, they need not consider whether the state will punish them indiscriminately. In other natural settings, however, targets participate with sources who are irrational or of unknown or dubious integrity. In these situations, the source's general intent may be important to the target's decision-making. Research on nuclear deterrence indicates that one nation's general intent regarding the use of nuclear weapons is critical to another nation's strategy of action (Schelling; Singer; Snyder). The intent to punish indiscriminately may help to explain situations where the target opposes despite the belief that the source is capable and the target is incapable. Thus, a child may flee from an abusive parent who commands compliance under threat of spanking, for the child thinks the parent will punish regardless of his or her response. In a battle, soldiers may fight rather than surrender, even against great odds, if they think the enemy tortures and executes prisoners.

Finally, the target's ability to comply affects his or her response. Compliance is possible when the target has the ability to meet the source's demand. If the target is unable to comply, he or she must oppose. When the target lacked the wanted goods or the knowledge and skill to surrender them, he or she always opposed. Opposition can appear as resistance, an

attempt to defeat the source. Or it can take a form of noncompliance. Here, the target indicates that he or she cannot comply due to obstacles beyond control and may suggest that the source adjust the demand so as to make compliance possible; this sets the stage for negotiation.

The social influence and deterrence research often assume the target has the ability to comply. In experimental games, targets generally are provided with the knowledge, skill, and resources needed for compliance. In legal coercion, targets are assumed to have the ability to comply with laws; citizens who are unable to comply, because of mental deficiency, duress, or some other condition, are exempted from sanctioning. But in other settings, the target's ability may be problematic and consequential. While a child may want to comply with a parental order, if he or she has not developed the knowledge or skill necessary for compliance, opposition is imperative. In extortion, such as kidnapping or blackmail, the target may not have the amount of money requested by the source. This leaves the target little choice save opposition. But fearing for the safety of the hostage or reputation, the target will not resist; the target will negotiate with the source in hope of reaching an agreement on an amount which he or she can provide (Best).

Threat believability is not as critical to decision-making as source capacity and general intent and target capacity and ability. According to the social influence and deterrence research, the more believable the threat, the more likely the target will comply. The robbery data do not support this relationship. Focusing on cases where the target complied, there was great variation in the assessed probability that the source would punish opposition. In 40 percent of the 122 cases, the target believed the source definitely would punish opposition (a condition of high threat believability). Here, the target personified the source as the kind of person who would inflict punishment for opposition (e.g., "robbers kill people who won't follow their orders") or thought the particular source would punish opposition (e.g., "he was so nervous I thought he would panic and kill me"). However, in 60 percent of these cases, the target believed there was only a chance the source would punish opposition (a condition of low threat believability). The fact that the source had lethal resources suggested a possibility of death or serious injury, and this was enough to warrant compliance:

Question: Do you think he would have hurt you if you resisted him?

*Target #18*

-- I don't know. . . . When somebody's holding a pistol on you, you don't play around.

*Target #31*

Whether he's kidding or not, if he has a gun, I'll give him everything. I mean I don't care if he's serious or not.



Moreover, some targets believed there was but a slight chance the source would punish opposition. The target personified the source as a thief who employs a threat of force as a tool for obtaining compliance and, if opposed, would probably forfeit the venture. Despite this assessment, the target complied in order to avoid the risk, as remote as it seemed:

*Target #9*

I knew the kids were amateurish. They were young, good looking, clean boys. I just didn't think they would hurt anybody no matter what happened. But when you're looking down the barrel of a Saturday Night Special, then I don't care who's holding it. You do what you're told. It's just not worth the risk to try anything.

This is not to say that threat believability is ineffectual. It is plausible that if believability were zero, the target would oppose, regardless of the stakes. But when threatened punishment is extremely severe, high and low threat believability have virtually equal impact. This is consistent with the subjective expected value theory developed by Tedeschi and his colleagues (c, d). This social influence theory asserts a multiplicative relationship between the probabilities and values of punishment: the target is likely to comply if the probability of punishment is high and the magnitude of punishment is low or if the probability of punishment is low and the magnitude of punishment is high. This study offers a measure of support for the latter contention. Given a strong source who makes punishment contingent on opposition and a weak target who can comply, when the stakes are extremely high, the target will comply even when threat believability is low.

## Notes

1. "Believability" is used instead of "credibility." According to Tedeschi and Lindskold (322-4), credibility is an objective estimate of the probability the source will punish opposition, while believability is a subjective assessment. Threat credibility is based on the history of interaction between the source and target, it is the proportion of times the source punishes opposition over all occasions where the target opposes. Threat believability is the target's estimate of the probability of being punished for opposition. The target may employ credibility to determine believability, but other cues, such as the source's comportment, also may be used. In robbery there usually is no history of interaction between the source and target. Thus, believability is more appropriate.

The deterrence literature speaks of the certainty of punishment rather than threat believability. These are comparable, for both involve the probability of being punished for opposition.

2. The criminal law distinguishes between aggravated robbery, where the source uses a weapon, and robbery, where he or she is unarmed. In this study, robbery refers to both forms.

3. Targets' accounts of their decision-making may be self-serving, casting themselves in a favorable light. Yet, witnesses or sources often corroborated the existence of certain features on which targets claimed their decisions rested.

4. The source's capacity, like the other conditions examined below, is based on the target's perception. Whether the source really has the capacity to punish is not as important as whether the target perceives the source as capable (Bacharach and Lawler).

5. Firearms and knives are considered lethal resources, for targets believed such weapons were sufficient to cause death or serious injury.
6. There is little reason to believe there were substantially more targets who considered themselves capable of effective opposition. Targets do not routinely carry lethal resources or, if they do, often are not in a position to mobilize them, for sources generally take the offensive at the start of the transaction.

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# Trends in Reported Happiness Within Demographically Defined Subgroups, 1957-78\*

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## ABSTRACT

*Surveys conducted between 1957 and 1978 indicate a decline in reported levels of happiness from the 1950s to the early 1970s, followed by a gradual rebound. The pattern is different for certain subgroups of the population. Those in the bottom quartile in family income showed little decline in happiness during the initial period and a steeper rise during the 1970s. A similar pattern was observed for those age 65 and older. Further analysis of the differences among age groups suggests that cohort effects dominate true age effects, and that the period effect observed prior to the 1970s was greater among young people than among older people. These patterns are interpreted as evidence of a possible shift away from materialistic values during the 1960s. The magnitude of the trend, however, is very small.*

Interest in the subjective quality of life has emerged in recent years in reaction to exclusive dependence on objective and especially economic indicators of well-being (cf. Campbell and Converse). If we wish to compare the quality of life of different groups, or trends over time, it is not enough, so it is argued, to compare income in dollars or health in sick days or safety in terms of victimization rates. We must also find out how individuals perceive their environment and how they feel about it if the comparisons are to be truly meaningful, since people differ in what is important to them and what they feel constitutes a better or worse condition. A counter-argument is that individuals may be so adaptable to a wide range

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of conditions that comparisons of subjective responses are meaningless or even pernicious, obscuring real differences in the conditions under which people live.

The usefulness of social indicators lies largely in how they behave over time; indeed, by some definitions (e.g., Sheldon and Freeman) measures not available as time-series cannot qualify as social indicators. It is here that subjective social indicators are severely deficient compared to objective indicators, many of which have been collected on a regular basis for many years. One of the very few measures with any claim to representing the subjective quality of life that has a history of more than a decade or so is a simple, much-maligned question about happiness. In various formats, national cross-sections of the U.S. population since 1946 have been asked questions about how happy they are. A thorough documentation of the history of this type of question is provided in a recent article by Smith, who reports evidence of a gradual increase in happiness from the mid-1940s up to about 1957, followed by a decline that lasted until about 1970 or so and a possible rise since then.

This paper extends the analysis reported by Smith by examining trends within subgroups of the population. It also is an extension, on the temporal dimension, of an analysis reported by Campbell et al. (26-31) who found that an overall decline in happiness between 1957 and 1972 was not uniformly observed in all subgroups. In fact, it appeared that the decline was limited principally to younger people with moderate to high incomes. Persons age 60 and older, and those with incomes in the lower third of the distribution, showed little or no change in happiness over time. Owing to differential trends across income, a decline was observed in the discrepancy, in average level of reported happiness, between those with low incomes and those with higher incomes. The authors interpreted this decline in the importance of income as suggesting that non-material, non-economic needs have become increasingly important to those with adequate and secure incomes. In Inglehart's term, this may be a manifestation of the emergence of "Post-Materialists", a segment of the population which has become less concerned about material well-being and more concerned with the satisfaction of higher order needs, in the Maslovian sense.

Considerably more data are now available than was true for the analysis just described, as witnessed by Smith's compilation of some 45 national samples in which some version of the happiness question has been asked. It is the purpose of this paper to test the validity of the differential trend reported by Campbell et al. for age and income groups and to consider some interpretations of these trends. The meaning of the differences between age groups is considered by examining trends for different birth cohorts. The differential trends across income levels are further examined and compared to trends across educational levels.

## Methods

This paper describes an analysis of data collected between 1957 and 1978 as part of 15 national samples, all but one by personal interviews. Seven of these studies were conducted by the Survey Research Center at the University of Michigan, while the remaining eight were conducted by the National Opinion Research Center at the University of Chicago. This set omits the series of surveys conducted by the American Institute of Public Opinion (Gallup Poll), which used a different version of the happiness question than was used by SRC and NORC and which yielded a different distribution of answers, as shown by Smith (Figure 1). (There are also what appear to be very minor and inconsequential changes in wording for the questions asked by SRC and NORC, as documented by Smith Table 3 and Appendix.) These studies are listed in Table 1, along with the number of respondents who answered the happiness question and the percentage distribution of answers.

The data analysis technique merits some comment. Because the happiness question is measured on a 3-point scale (and indeed, the distribution makes it almost a dichotomous variable), there is a strong possibility of heteroscedasticity, or unequal error variances across subsets. In this case, ordinary least-squares would provide estimators of trends that, while unbiased, would not be minimum variance unbiased estimators. The weighted least-squares procedure provides more precise estimators of population parameters when the variance of the dependent variable is not constant.

Another possibility would have been to treat the variables as strictly categorical and to use procedures such as those developed by Goodman. In general, however, categorized data analysis techniques do not cope well with polytomous dependent variables, especially when the categories are ordinal or intervally scaled. Neither do most such techniques allow the analyst to treat easily the independent variables as intervally scaled, a serious shortcoming in the present analysis where our interest is in trends over time.

For these reasons, data were analyzed using the weighted least-squares procedure developed by Grizzle et al. and as implemented by the program GENCAT (Lanids et al.). This technique is appropriate for categorical data, but it involves the specification of a design matrix just as in ordinary least-squares, so that ordinal or interval properties of independent variables are readily incorporated. Moreover, the dependent variable can be transformed in any desired fashion, such as by taking the mean value for respondents in each cell defined by cross-classifications on the independent variables, rather than restricting it to a set of nominal categories.

Exploratory analysis (preliminary to the work reported in this paper) varied the specification of the dependent variable. Some analyses simply

Table 1. DISTRIBUTIONS OF ANSWERS TO HAPPINESS QUESTION IN FIFTEEN STUDIES, 1957-78

Study Title	Year	Organi- zation	Not too Happy (0)	Pretty Happy (1)	Very Happy (2)	Total	Average	Sample Size
Americans view their mental health I	1957	SRC	11.23	54.1	34.7	100%	1.235	2,451
Anti-Semitism	1964	NORC	10.7	52.1	37.1	100	1.264	1,966
Quality of American life I	1971	SRC	9.9	61.2	28.9	100	1.190	2,147
Social indicators	1972	SRC	9.6	65.9	24.5	100	1.149	2,343
General Social Survey	1972	NORC	16.5	53.2	30.3	100	1.138	1,606
General Social Survey	1973	NORC	13.1	51.1	35.9	100	1.228	1,500
General Social Survey	1974	NORC	13.1	49.0	37.9	100	1.248	1,480
General Social Survey	1975	NORC	13.1	54.1	32.9	100	1.198	1,485
General Social Survey	1976	NORC	12.5	53.4	34.1	100	1.215	1,499
Americans view their mental health II	1976	SRC	10.9	58.2	30.9	100	1.200	2,225
Omnibus Study: Personal	1976	SRC	10.3	60.8	28.8	100	1.185	1,402
Omnibus Study: Telephone	1976	SRC	12.1	56.3	31.6	100	1.195	1,734
General Social Survey	1977	NORC	11.9	53.2	34.8	100	1.229	1,527
General Social Survey	1978	NORC	9.6	56.1	34.3	100	1.248	1,517
Quality of American life II	1978	SRC	8.6	62.5	28.9	100	1.202	3,740

predicted to the proportion of respondents who answered in either the highest of the three scale categories ("very happy"), or the lowest category ("not too happy"); others predicted to the log odds transformations of these proportions. However, it appeared that information was contained in both of the extreme categories, so some was lost by using proportions in just one of them. In particular, differences in trends were noted for the two proportions. The proportion of respondents who said they were "very happy" declined from about 35 percent in a 1957 SRC study, down to less than 30 percent in the early 1970s, where it stayed about level or perhaps rose slightly. A parallel but elevated pattern is observed in NORC data. On the other hand, the proportion of respondents who said they were "not too happy" showed little or no trend in SRC data, staying at about 10 percent; but in NORC data this proportion increased from about 11 percent in 1964 to over 15 percent in 1972, followed by a decline back down to about 10 percent in 1978. It was therefore considered more appropriate to attempt to preserve all of the information on the scale by treating the three categories as if they were equally spaced on an interval scale; this is probably not a completely accurate scaling, but it is also probably better than collapsing two of the three categories. (With only three answer categories for a single question, we do not have much information about relative happiness to begin with; the least we can do is to try to preserve what is available.)

To find the best description of the trends over time for subgroups of the population, the initial step was to find the distribution of respondents on the happiness question of each such subgroup in each of the 15 studies. For example, four subgroups were defined by age of the respondents, and the number of respondents in each of these four groups who said they were "very happy," "pretty happy," and "not too happy" was obtained for each of the 15 studies: a total of 180 frequencies. This vector of frequencies was then analysed using the GENCAT program.

The dependent variable was defined as the mean happiness score for each subgroup in each study. The mean was generated by assigning scores of 2 to the respondents who said they were "very happy," scores of 1 to those who were "pretty happy," and scores of 0 to those who were "not too happy." Variation in these mean scores were to be explained by the following set of variables: the year in which the study was conducted; the subgroup defined by a particular demographic characteristic of the respondent; the organization which conducted the study (i.e., NORC or SRC); and interactions among these variables. The effects of year were generally assumed to be linear or, at most, curvilinear with a single inflection point; therefore, only linear and quadratic terms were included for year and for interaction terms involving year.

The initial model, with all interaction terms included, was then sequentially simplified by eliminating terms which did not have statistically



significant effects. In GENCAT, the effects of individual terms or sets of terms can be assessed by specification of contrast matrices, and non-significant terms can be eliminated in the next step of the analysis by removing the corresponding column from the design matrix. There are no restrictions on the design matrix; interaction terms can be included whether or not main effect terms are included, so non-hierarchical models can be specified and evaluated.

A nominal significance level of .05 was used as the criterion for elimination of a term. Two comments should be made about this criterion, however. First, the significance levels were calculated as if the data came from simple random samples (SRS) rather than complex multistage clustered samples. If the design effect is typically about 1.5 for this variable across the various samples, the nominal significance level of .05 would correspond to an actual significance level of about .10. The second comment can be put as a question: what is the appropriate criterion in an analysis such as this one where the objective is to summarize patterns rather than test hypotheses? It is not obvious how much weight should be given to type II errors (eliminating a term that belongs in the model) relative to type I errors (failing to eliminate a term that has no real effect). I have chosen to accept a moderately high probability of including a term that does not belong in order to keep the probability of a type II error within bounds, but probability values (again, based on SRS assumptions) are reported so that the readers can form their own judgments.

The details of these numerous steps are massive and not especially informative, so they are not included in this paper. Instead, the final model for the sets of groups defined by the various demographic variables are summarized by figures which display the trends over time for each subgroup as *predicted* by the final model. The reader should bear in mind, then, that the figures do not show actual curves but fitted means after removing nonsignificant effects and error terms. The actual means show considerable variation about the fitted curves, and trends would be difficult to detect through the noise. The trends that are shown are statistically significant, but they are also very weak in terms of explanatory power.

## Findings

### OVERALL TRENDS

The initial analysis was for all respondents combined, not for any subgroups, and was intended to detect the significance of overall trends in the data. The initial model included terms for the organization that collected the data (SRC or NORC), two terms for year (measured as years and square of years after the initial study in 1956), and all interaction terms

involving the organization and the year terms. Both linear and quadratic terms for year and the corresponding interaction terms proved to be significant ( $p < .01$  for each term), thereby confirming the existence of an inflection point during this period. The trends are plotted in Figure 1.

The decline in overall happiness from 1957 to the early 1970s is shown to be significant. Although there is only one study for each organization conducted before 1970 included in this analysis, both sets of studies reflect a parallel pattern. This drop is followed by increasing levels of happiness during the 1970s, more strongly evident in the studies conducted by NORC than in those done by SRC. There is, moreover, a consistent discrepancy between studies done by the two organizations: people responding to NORC studies said they were happier, on the average, than respondents in SRC studies. This discrepancy was also noted by Smith, who looked for possible explanations. His conclusion was that the most likely explanation lies in the context in which the happiness questions were asked on individual studies. In several of the NORC studies from the 1970s, the happiness question came immediately after a question about marital happiness, the answers to which are strongly correlated with overall happiness. Context effects, then, may be rather powerful on this question. It is possible, though less plausible, that context could also account for the steeper trend observed for NORC data as compared to SRC data.

Also shown in Figure 1 are the observed means for each of the 15 surveys. These points show considerable variation about the fitted trend curves. There is substantial variation from survey to survey that is *not* explained by the combination of research organization and time trends,

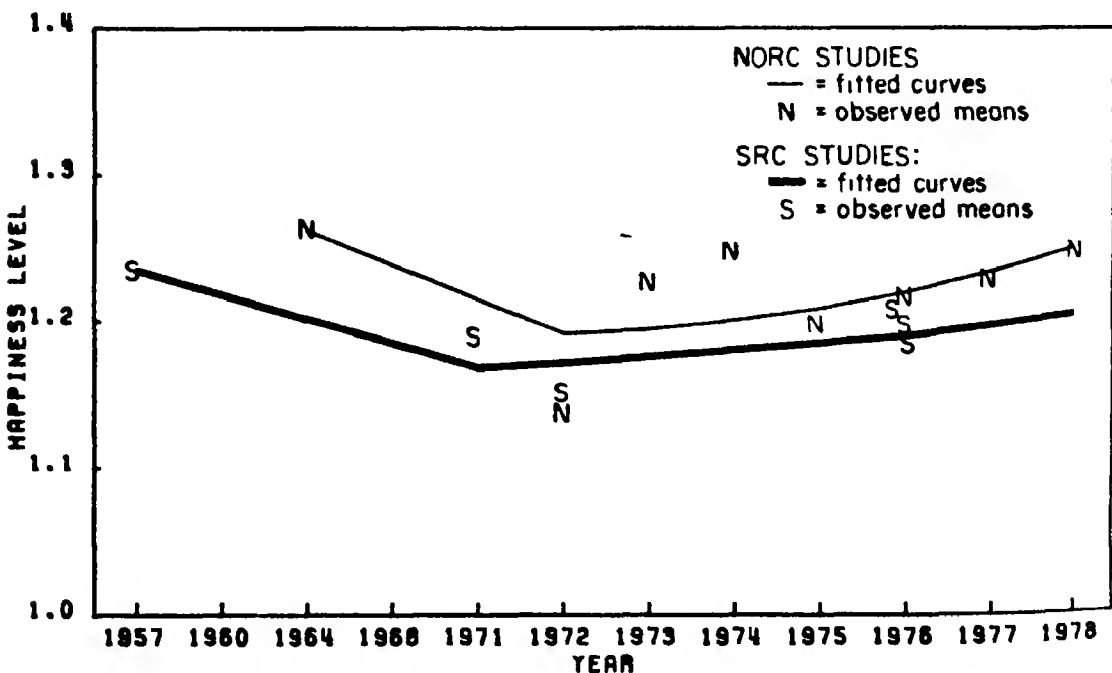


Figure 1. OVERALL TRENDS IN HAPPINESS, 1957-78

but is probably a consequence of the different contexts in which the happiness question was asked, or of seasonal variations. This lack of fit is also reflected in the size of the  $X^2$ , the test statistic used to assess the statistical significance of observed data from a model. (The  $X^2$  value used here is the Wald statistic, which like other test statistics would have a central chi-square distribution if the model is correct, so that the probability of observing a value as large as or larger than the value of  $X^2$  actually observed can be calculated for the chi-square distribution with appropriate degrees of freedom. The difference in the observed  $X^2$  values for two model specifications is used to assess the statistical significance of terms omitted from one of the models relative to the other, although this is not well justified statistically, unless the full model provides an adequate fit to the data.) The model we have specified has a  $X^2$  value of 28.78, with 7 degrees of freedom, a value that is very unlikely to have been observed were the model correct ( $p = .0002$ ). That is, the deviations from the fitted values observed in Figure 1 are significant. On the other hand, the terms that are included in the model do explain a substantial proportion of the variation between surveys: if we specify an "equal probability" model, with no terms for time trend or for organization, the associated  $X^2$  value is 82.13 with 12 degrees of freedom.

#### INCOME LEVELS

We next looked at trends in happiness for particular subgroups of the population. Figure 2 shows trends for groups defined by relative income. (The curves in Figure 2 are fitted rather than observed values.) The model

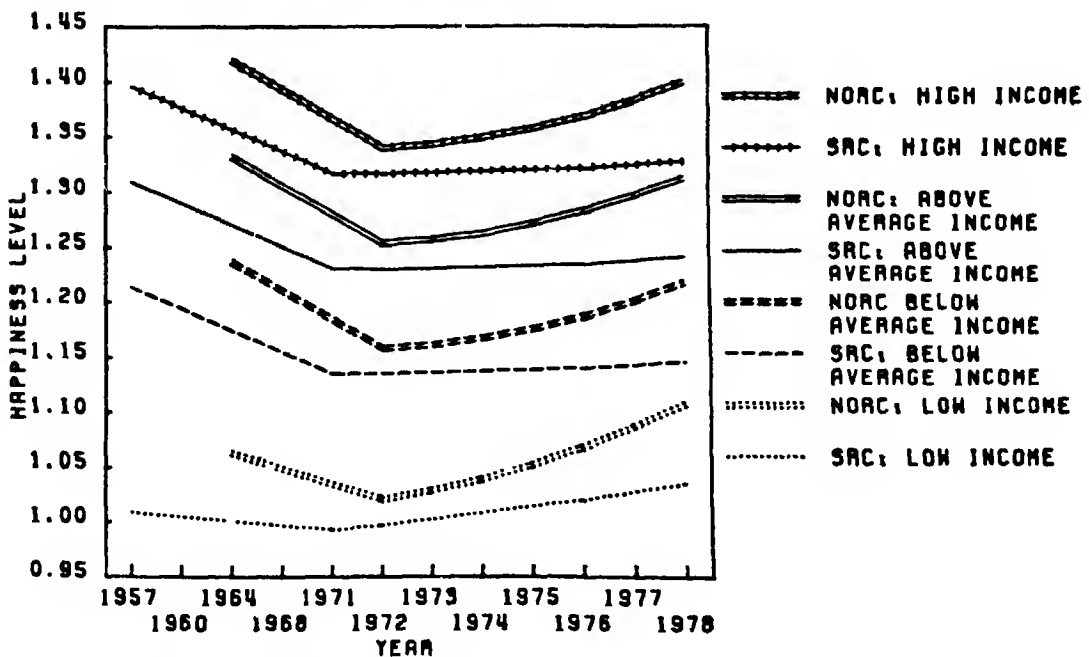


Figure 2. TRENDS IN HAPPINESS FOR DIFFERENT RELATIVE INCOME LEVELS, 1957-78

summarized by this figure is a reduction of a model which included the following set of terms: linear and quadratic year, terms for three of the four income quartiles, organization that did the survey, and all 11 two-way interactions involving these terms. The  $X^2$  value associated with this value is 74.99 with 34 degrees of freedom ( $p = .00006$ ), so there remains significant unexplained variation. (On the other hand, the  $X^2$  value for the "equal probability" model is 928.03, with 51  $df$ , so most of the variation is explained by the model.) The final model is a reduction of this initial model: all but three interaction terms were eliminated, with an increase in the  $X^2$  value of 5.46 ( $df = 8$ ,  $p = .707$ ). The remaining interaction terms are linear year  $\times$  lowest income quartile, and linear and quadratic year  $\times$  organization; each of these interaction terms, as well as all of the terms for main effects, are significant ( $p < .01$ ).

The difference reported in Campbell et al. is evident once again, for SRC data: the decline in happiness from the 1950s to the early 1970s is seen for all income levels except those in the low income category (i.e., those with family incomes in approximately the lowest 25 percent of the overall distribution for each study). There is a suggestion of a slightly smaller decline for the low income group on the NORC data as well, but the difference from other income groups is not as great as observed for SRC data. What is most intriguing, however, is that for SRC data there is little or no trend in happiness in the 1970s for those not in the bottom quartile for family incomes. In other words, for three of the four income quartiles the upward trend observed on Figure 1 for all respondents to SRC studies has disappeared. There is a slight upward trend, however, for the lowest income level.

NORC data, unlike SRC data, show increasing levels of happiness during the 1970s for all income levels, albeit a steeper trend for the lowest income level. One interpretation of this discrepancy follows from our speculation about the overall trends, where we raised the possibility (without corroborating evidence) that the steeper trend observed in NORC data compared to SRC data might represent an artifact of different contexts in which the happiness question was asked in different studies. Perhaps, then, if there is such a context effect, it is this which we see reflected in the differing trends for income groups. If so, it is more likely that context effects have suppressed a trend in the SRC data rather than produced an artifactual trend on the NORC data: the NORC studies are the General Social Surveys, which tend to have consistent contexts for the happiness item from year to year, whereas the SRC studies were diverse, producing widely different contexts for the happiness question from study to study.

The final observation to be made from Figure 2 is that data from both SRC and NORC confirm a consistent decline in the discrepancy of the happiness reports between those with low incomes and those with higher incomes. By 1978 the gap between those with low and moderately low

income was only about half to two-thirds what it was in studies before 1970. This confirms and extends the trend observed earlier (Campbell et al., 28) that differences between income groups in their happiness reports have been declining.

#### EDUCATION

As we mentioned above, an interpretation offered by Campbell et al. for the difference in trends for lower and higher income levels was that, although economic conditions may have improved for people at all levels, the value placed on economic well-being may have declined relative to non-materialistic values, at least among those who are well-enough off that they need not be concerned about basic survival needs. Since another indicator of socioeconomic status is attained education level, further insights into the validity of the interpretation of the differential trends across income levels may be gained by examining trends among those with different amounts of reported education (See Figure 3, SRC data only). The original model had the same set of terms as that specified for income, except that education levels were substituted for income quartiles. The  $X^2$  value for this model is 57.71 with 34  $df$  ( $p = .0067$ ). The reduced model for which fitted values are displayed in Figure 3 eliminated all interaction terms except three: the grade school education group by year, and organization by linear and quadratic year. This reduced model had an  $X^2$  value that was 17.99 larger than that for the original model ( $df = 8$ ,  $p = .021$ ). (None of the omitted terms was individually significant at the  $p < .05$  level, but in combination there is a marginally significant increase in lack-of-

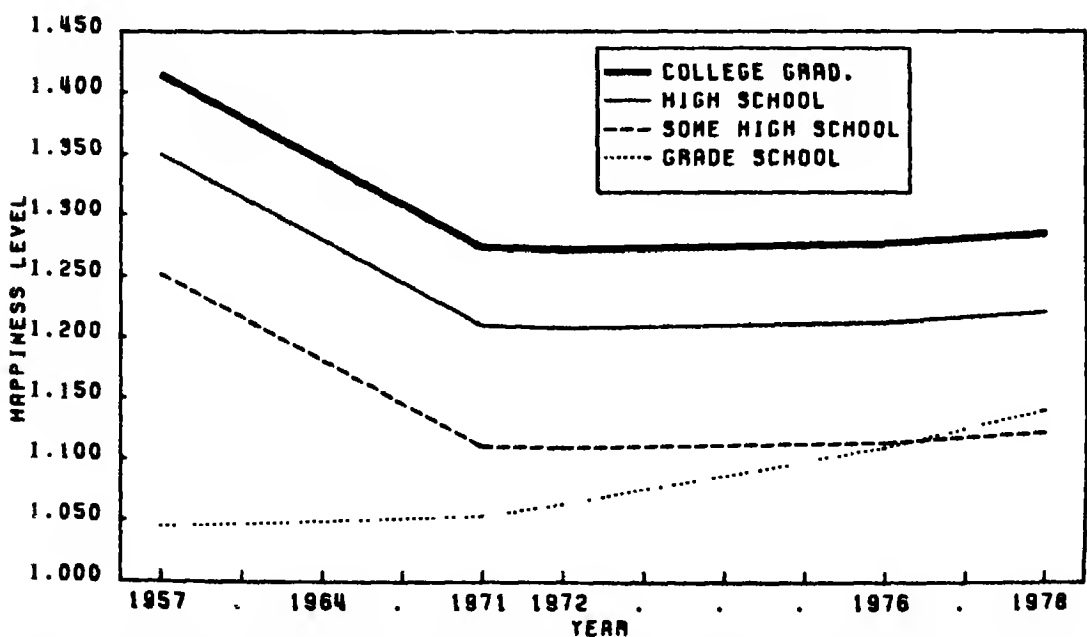


Figure 3. TRENDS IN HAPPINESS FOR DIFFERENT EDUCATION LEVELS, 1957-78

fit.) All of the terms retained in the reduced model have significant effects ( $p < .01$ ).

Persons with at least some high school education showed a decline in mean levels of reported happiness up to the early 1970s, followed by a period of either no change (SRC data) or a slowly rising trend (NORC data). Those with only a grade school education or less, on the other hand, showed little or no trend between the mid 1950s and the early 1970s but a comparatively large increase in happiness during the 1970s. The end result is elimination of a rather substantial difference in expressed happiness between respondents with minimal education and those with at least some high school; and the overall spread between education subgroups has been cut in half over the period shown. It is very likely that this narrowing of the gap for different levels of education reflects the same basic phenomenon observed for income groups. The expansion and improvements in the social welfare system and in the Social Security system and related medical care for the elderly (who make up a large proportion of those with little education) may have contributed to the security and well-being of those at the bottom of the ladder; on the other hand, those with more education and whose material needs are less in question may be more sensitive to other changes in our society, such as political events, environmental quality, and energy supplies. The data presented here are too sparse to test these possibilities, but the observed trends provoke such speculations. In a similar vein, Campbell suggested that the greater mobility of the different and well educated makes them more aware of, and sensitive to, events in the broader environment.

#### AGE

Trends on happiness for groups defined by age are shown in Figure 4. The original model again included all main effects and two-way interaction terms and had an associated  $X^2$  value of 69.36 ( $df = 34$ ,  $p = .0003$ ). Six of the interaction terms were retained in the reduced model: linear year by each of three age groups, organization by the youngest age group, and organization by linear and quadratic year. The  $X^2$  value of this reduced model was 9.27 higher than that of the original model ( $df = 5$ ,  $p = .099$ ). One of the interaction terms (organization by quadratic year) had a nominal significance level of  $p < .02$ , while the other terms all were significant at  $p < .01$ .

The predicted curves for each of four age groups are shown in Figure 4 (SRC data only). The differences are again substantial: the average happiness of those less than 65 declined from the 1950s to the early 1970s, during which period the happiness reported by those age 65 and older increased, at least according to SRC data. The rising trend for the older people continued in the 1970s, while the happiness of younger people

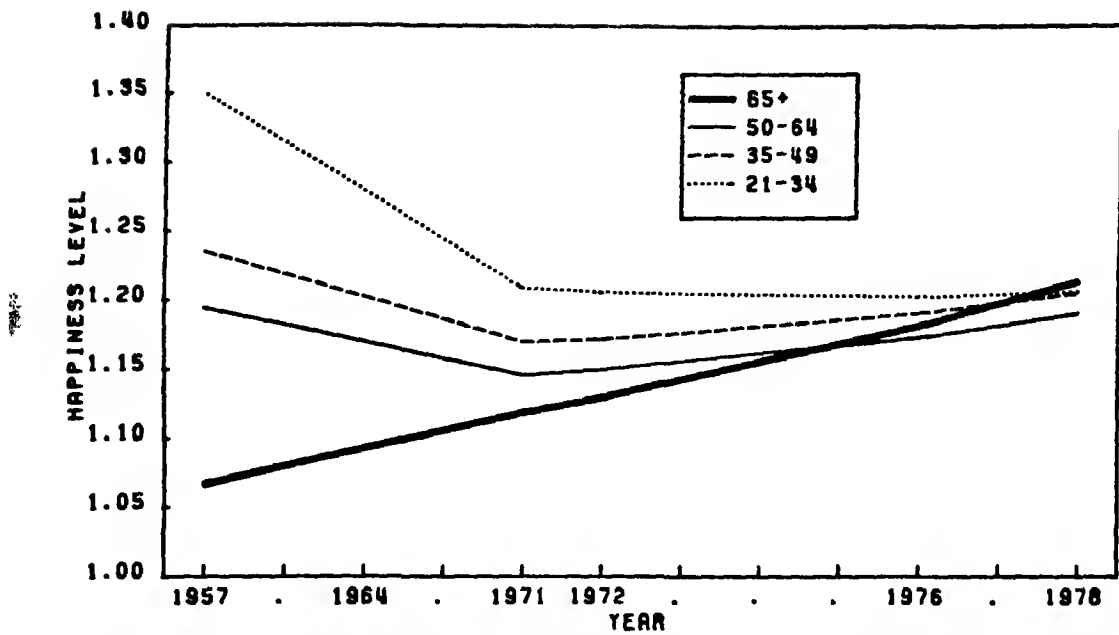


Figure 4. TRENDS IN HAPPINESS FOR DIFFERENT AGE LEVELS, 1957-78, SRC DATA

stayed about constant (for SRC data only; NORC data show a gradual upward trend for those age 35-64, but less steep than that observed for respondents age 65 and older). These differences in trend have changed the cross-sectional age patterns substantially: in 1957, there was a wide gap in happiness according to age, with those under 35 expressing considerably more happiness than older people. This difference essentially disappeared by the late 1970s; indeed, NORC data suggest that the youngest people have become the *least* happy.

#### COHORT

Glenn reports changes in the age pattern resembling those observed in Figure 4, based on NORC and Gallup data. Glenn suggests that the explanation for this change may lie in the emergence of cohorts who are unhappier than earlier cohorts, perhaps because they have had more difficulty in attaining their expected economic and occupational success, or perhaps because they have less ability to cope with frustrations. The data from these studies are not well-suited to a systematic analysis of age, period, and cohort effects (because of the intervals between the studies and the bracketing of age in the 1957 study). Instead, we have simply analyzed the trends on the happiness item for different sets of birth cohorts. The original model with all main effects and two-way interaction terms had an associated  $X^2$  value of 56.80 ( $df = 33$ ,  $p = .006$ ). Only the organization by linear and quadratic year interaction terms were not significant ( $X^2 = 3.81$ ,  $df = 2$ ,  $p = .149$ ): all other terms were kept in the reduced model. Fitted data for

SRC surveys only are shown in Figure 5 (NORC surveys lead to the same general conclusions).

While no conclusions can be drawn statistically, some observation can be made by comparing Figures 4 and 5. The average level of happiness reported by members of the earliest cohort group, those born in 1902 or earlier, has been consistently lower than that of later cohorts. Moreover, the trend for this cohort has been one of slowly rising levels of happiness throughout the years covered by these surveys—these people have reported somewhat more happiness as they have gotten older, although they never reached the level of those born later. The respondents themselves, of course, are not the same from study to study, but they are in each case a sample of the surviving members of their cohort. It is possible, however, that the observed trend might reflect a higher mortality rate for unhappy individuals rather than increased happiness among the surviving members of the cohort. The rate of increased happiness within this cohort is smaller than the trend observed in Figure 4 for those age 65 and older at the time of each study. This difference in slope, along with the persistently lower happiness reported by the early cohort compared to later cohorts, suggests that much of the increasing happiness reported by people age 65 and older is contributed by an influx of new cohorts with greater happiness.

Another observation from Figure 5 is that the decline in reported happiness from the late 1950s to the early 1970s is greater among those born in the years 1923–42 than it is among those born in earlier years (Figure 5 shows data from SRC studies only; data from NORC show this pattern even more clearly, with the strongest decline among those born in 1943–62 and successively smaller decrements among earlier cohorts, and

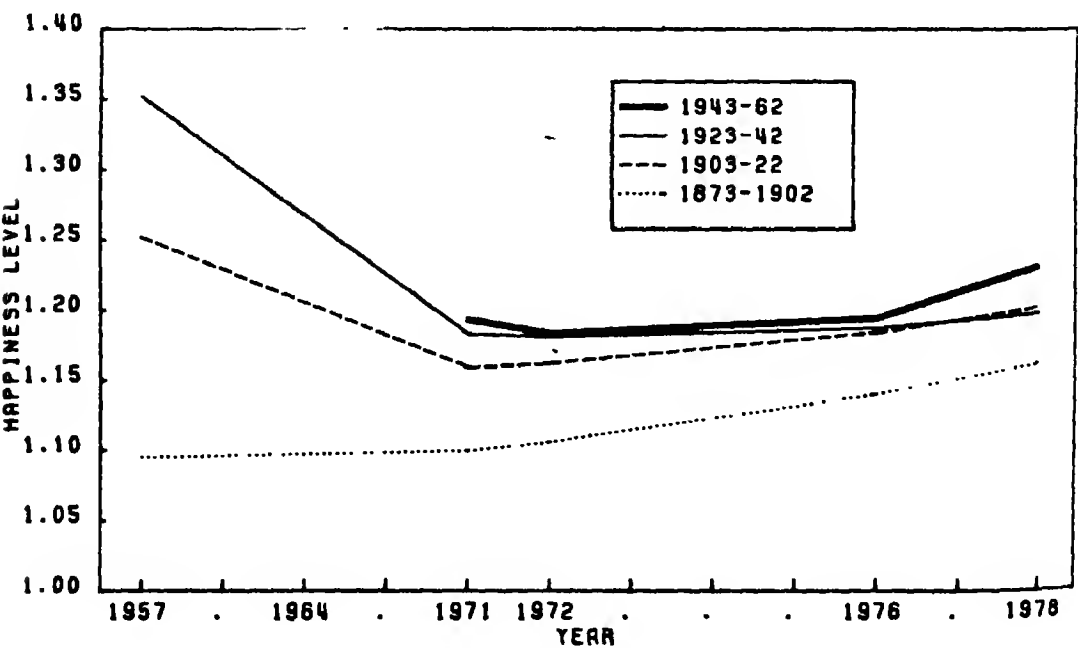


Figure 5. TRENDS IN HAPPINESS FOR COHORT GROUPS, 1957-78, SRC DATA



even a slight upward trend over this period among those born before 1903. Indeed, the post-1943 cohort fell from the most to the least happy of the four sets of cohorts.) This apparent age-by-period interaction effect suggests that whatever it was that produced the period effect during the 1960s, those factors had the greatest influence on the younger members of the adult population. This pattern, along with the observation from NORC data that since the early 1970s the cohort of persons born in 1943 and later may be substantially less happy than earlier cohorts, once again reminds us of the hypothesis suggested by examination of trends for income groups: we may here be seeing evidence of the emergence of "post-materialistic" values. Younger people, in particular, may have undergone a shift in values in the late 1960s, becoming more concerned about political, ecological, and human relations problems. (The low levels of happiness observed among the latest cohort in NORC data is not replicated either in the SRC data or in Gallup data, as reported by Glenn (Table 12.12), a difference with no apparent explanation. In the absence of corroborating evidence, the existence of a cohort difference should not be taken too seriously.)

The trends for birth cohorts in Figure 5 should also be compared to the trends for educational groups in Figure 3. A plausible interpretation of the gradual trend toward increasing happiness observed among those in the earliest birth cohort (those born before 1903) is that it is a reflection of the trend among the least educated respondents and as such is another reflection of the improved material well being of those at lower income levels. If this is correct, very little evidence remains for an influence of age on happiness. For those born after 1900, Figure 5 shows almost constant levels of happiness during the 1970s, and the rising trend among those born in the 1800s is interpreted as a period effect, not an age effect. Rather than a direct effect of age, we have seen evidence of an interaction between age and period such that younger people were more affected by historical factors that influenced happiness levels during the late 1960s.

## Discussion

In the introduction to this paper, I asserted that a critical test of the usefulness of social indicators lies in the information they provide about important trends over time. The happiness question is one of the very few measures of the subjective quality of life that has been asked over a long enough time period to allow us to begin to test its usefulness.

We know that average levels of reported happiness have changed over time; there appears to have been a decline in happiness levels from the mid-1950s to the early 1970s, followed by a gradual recovery. These time trends, however, are very weak. Smith (Table 5) reports annual changes in the percentage of respondents who say they are "very happy" ranging

between about 0.5 percent and 2.5 percent over periods when the trend seemed to be linear. The multiple correlation between year and average level of happiness across the 15 studies analyzed in this paper (after controlling on the organization which conducted the study) is less than .04, meaning that trends over time (including curvilinear trends as captured by linear, quadratic, and cubic terms on age) account for about 0.1–0.2 percent of the variance in reported happiness. This is not a level of correlation that merits much weight as a guide for public policy.

If we ignore the weakness of the trends and look for explanations for the direction of the trends, we probably would look first to changing economic conditions that may be correlated with happiness levels. Davis has considered this and, after extensive analysis of happiness trends over time has concluded, somewhat facetiously, that the answer to the question "Does economic growth improve the human lot?" is "Yes, indeed, about .0005 per year."

The answer, however, may be more complex than a simple relationship between economic conditions and happiness, no matter how weak. The trends observed in this analysis for groups defined by socioeconomic status suggests that happiness has been rising faster for those at the bottom: those with the least education, the lowest incomes, and the oldest birth dates. We have speculated that the explanation for the smaller increases observed for the more advantaged segments of the population may lie in changing value structures; as basic needs are satisfied and economic security becomes of less concern, higher level needs may take on greater importance. The quality of the environment, the relationships between people and with public officials, the quality of life in future years and for our children may be sources of greater concern to many people today than was true in the past.

Inevitably, however, we must acknowledge the weakness of the relationships that we are discussing. The trends, and especially the differential trends, in the answers to the happiness question are intriguing, but how real and how important are they, after all? Are we limited by the low reliability and validity of a single, simple question about a complex topic? Or are the determinants of the subjective quality of life so diverse and so individualistic as to render subjective social indicators a poor basis from which to infer social trends? The answers are incomplete, of course; but there are clues available from cross-sectional studies.

Multiple classification analyses (i.e., dummy variable regressions) were used to find the explanatory power of a set of ten demographic variables with respect to several measures of life satisfaction and happiness in the 1978 Quality of Life study. These demographic variables included the age, race, sex, religion, and life cycle stage of the respondent; the respondent's educational attainment, the family income level, the employment status of the respondent and the occupational status of the head of

the household; and the size of the place of residence. The explanatory power of these variables with respect to the happiness item was 7.8 percent (6.4 percent after shrinkage to better estimate the population value). This is somewhat higher than the proportion of variance (6.3 percent) explained with respect to a single item measure of the satisfaction. This suggests that the happiness item is not a peculiarly unreliable measure of well-being, at least as far as single-item measures are concerned. Could we do better, though, by using a multi-item index in order to increase the reliability of the measure? Two indices were examined in the same way: the affect balance index, developed by Bradburn, which consists of 10 items concerning recent positive and negative feelings; and a domain satisfaction index, which is a summative index of satisfactions with 10 domains of life, such as marriage, family, work, housing, and friendships. With respect to the affect balance index, the 10 demographic variables explain 8.2 percent of the variance, only very slightly greater than their explanatory power with respect to the single-item happiness measure. Their explanatory power increased somewhat, to 10.2 percent, with the index of domain satisfactions. It appears, then, that we might be able to do somewhat better in finding trends on well-being over time if we had more reliable, multi-item measures available over a span of years rather than the single-item happiness measures; but there is little basis for thinking that the improvement would be more than marginal.

Another possibility is that the happiness question measures short-term mood (cf. Campbell et al., 7-11) and as such is less affected by economic, political, and cultural trends than measures of satisfaction, which may have a more cognitive basis. If so, however, one would also expect satisfaction measures to vary more in relation to current conditions, such as family income, employment status, and size of place of residence. In fact, however, we have seen that *less* of the variation in a single-item measure of life satisfaction is explained by a set of demographic items than is true for the happiness item. Moreover, for the 1978 data set the bivariate relationships with individual demographic characteristics, including income and race, are generally higher for happiness than for satisfaction.

Subjective social indicators still have only a short history, and final conclusions about their usefulness would be premature. As individual-level variables, the validity of self-reported evaluations and satisfactions seems to be well-established (cf. Campbell et al., Chap. 6 in particular). It is their meaningfulness at the *aggregate* level—i.e., as social indicators—that is more open to question. Within cross-sectional data, weak relationships between such subjective reports and socioeconomic status have been reported and confirmed (cf. Andrews and Withey; Campbell et al.). Moreover, the relationship between objective conditions and subjective evaluations of those conditions are generally weak, both at the individual level (Rodgers) and the aggregate level (Schneider). With respect to trends across

time, the data are still sparse, but in this paper and in an earlier paper by Davis, weak but possibly meaningful trends have been observed in the answers given to a question about happiness.

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# Employment and Mental Hospitalization: The Case of Buffalo, New York, 1914–55\*

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## ABSTRACT

*This paper explores the connection between employment trends and state mental hospital admissions first presented by Brenner in *Mental Illness and the Economy*. The analysis is based on employment data for the Buffalo SMSA and admissions to the SMSAs only major public psychiatric hospital for the years 1914 to 1955. We employ a series of specifications in time-series analyses. Our findings contrast with Brenner's; unlike his analysis, ours indicates that employment is positively related to admissions. Consistent with other previous research, hospital capacity is also found to be important in predicting admissions. We offer a number of interpretations for the results, in particular noting the need for the examination of the stressful effects of work itself, not merely its loss.*

The publication of Brenner's *Mental Illness and the Economy* in 1973 was an important event in the development of psychiatric epidemiology. Using data describing admissions to New York State mental hospitals over a long historical period, Brenner reported a significant negative relationship between the level of manufacturing employment in the state and admissions to various state facilities. Hollingshead's forward (Brenner, a) predicted a "lively discussion" of Brenner's findings, a prediction partially borne out by the more than 75 citations in the *Social Science Citation Index* by mid-1980, and the citation of the findings in medical sociology textbooks (Coe; Mechanic).

The reasons for this interest are not difficult to understand. Bren-

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ner's work added new dimensions to the central finding of psychiatric epidemiology, the relationship between social class and mental illness (Goldstein). By linking economic change to mental hospitalization, the work addressed important theoretical questions. It emphasized the relevance to psychological distress of political economy and of the connections between economy and society. Brenner employed econometric techniques that were more sophisticated than those commonly in use at the time in sociology or public health (Dooley and Catalano). To the degree that the use of these techniques was successful, Brenner's work implies a negative causal link between employment and mental hospitalization.

On a more practical level, this research offered a model for predicting change in the need for psychiatric services. The powerful techniques employed by Brenner could be of great value to mental health planners and evaluators. Thus, Sclar and Hoffman have examined how economic decline might lead to the increased use of mental health services in a metropolitan area. Catalano and Dooley (a) have raised the implications of this type of research for programs to prevent mental illness. Brenner's subsequent work (b) demonstrates its political meaning by examining the health costs of unemployment for a Congressional committee debating basic national economic policy. Thus, Brenner's work was perceived to be of both theoretical and practical importance.

### **Reexamination of Economic Change and Mental Illness**

The research following Brenner's work has raised important questions about the relationship between economic change and physical or mental distress. The predicted "lively discussion" has included elements of controversy (cf. Kasl, a, b; Marshall and Funch, a, b; Ratcliff). Eyer has challenged the notion that economic downturns produce illness, arguing instead that periods of boom are more harmful. A careful review of the growing literature on economic change and behavioral disorder (Dooley and Catalano) shows some support for Brenner's basic assertions, but points to many questions still to be answered with empirical research.

Brenner (a) claimed that no other factor (except for population growth) predicted hospitalization as adequately as the economy. Reexamining that position, Marshall and Funch (a) studied New York State employment and admissions data and found that, while economic conditions were negatively related to hospital admission, the capacity of the hospital system also predicted admissions.

This paper may contribute to the discussion of the long-term relationship between economic change and mental hospital admissions in two ways. First, to meet some of the criticisms of the highly aggregated character of previous research, we examine the relationship between economic

change and mental hospital first admissions in only one metropolitan area.<sup>1</sup> Second, we employ what we believe is a more satisfactory statistical approach than that used by Brenner.

In this study admissions to the Buffalo (New York) Psychiatric Center are studied for the period between 1916 and 1955, the same period that formed the core of Brenner's analysis.

Buffalo Psychiatric Center provides an ideal opportunity for a replication. Buffalo is the second largest city in New York. It was a large metropolitan center throughout this period. The hospital, first opened in 1880, was an established institution throughout the period of the study. During this period, the hospital, a medium sized institution relative to others in New York, was the chief source of public psychiatric care for the city of Buffalo and the surrounding counties of Erie and Niagara. The hospital was isolated from the rest of the mental health community: the two nearest state mental hospitals were 50 and 90 miles away. Finally, since Buffalo was primarily a manufacturing and commercial city, its economy would be well-reflected in the statewide economic series indicator used by Brenner.

Buffalo Psychiatric Center also provides an important focus for study because its experience appears in Brenner's analysis to diverge from the general pattern of an inverse relationship between the state of the economy and public mental hospitalization. In the section of *Mental Illness and the Economy* analyzing psychiatric admissions according to the patient's county of residence, the relationship between economic conditions in the Buffalo industrial area and admissions of patients from that area appears (in contrast with some other areas' patterns) to be either extremely weak or positive. Brenner's Table 19 (a, 100) portrays that relationship but omits Erie County, the largest and most industrial county in the Buffalo industrial area. The rationale for including or excluding series is not discussed in the text.

In the next two tables (Brenner, a, Table 20, Table 21), the analysis focuses on the number of consecutive years during which the correlation between economic conditions and mental hospitalization is less than  $-.65$ . These tables indicate that the correlation between Erie County male admissions and manufacturing employment was less than  $-.65$  for only 34 of the 46 years studied, while the Erie County female admission-employment correlation was more negative than  $-.65$  for only 22 of the 46 years studied. If the  $-.65$  cut-off point used by Brenner is adopted as evidence of a significant inverse correlation, the claim that, in this period in the second largest city in New York State, economic conditions were inversely related to mental hospital admissions, can only be supported by discarding evidence for 12 of 46 years of male admissions data, and for 24 of 46 years of female admissions data.<sup>2</sup>

In short, there is *a priori* reason to expect the pattern of the Buffalo Psychiatric Center to approximate that of the rest of the state mental health

system. The evidence that it does, however, is weak. Thus, the relationship of economic conditions to yearly admissions at the Buffalo Psychiatric Center merits additional examination.

## Data

For this study, economic conditions ( $E$ ) are measured by the same economic indicator used by Brenner (a), namely, an adjusted index of manufacturing employment.<sup>3</sup> Yearly sex-specific data on first admissions and hospital capacity were taken directly from the yearly records of Buffalo Psychiatric Center. Estimates of the yearly population of Erie and Niagara Counties (the counties surrounding and immediately adjacent to the Psychiatric Center) were provided by the New York State Department of Vital Statistics.<sup>4</sup>

To create a total first admission rate ( $AD$ ), male and female first admissions were added and that sum was divided by estimates of yearly Erie and Niagara County population. The male and female capacities of the Buffalo Psychiatric Center were added and divided by the Erie and Niagara County population to create a mental hospital capacity rate ( $CAP$ ). Unfortunately, reliable sex-specific estimates of the county populations were not available for several of the years in the period studied, making computation of sex-specific admission and hospital capacity rates impossible.

A preliminary perusal of the data suggested that the admission rate was somewhat lower during the years of World War II. To control for this possibility, a dummy variable (with a value of one during the Years 1942–45) was used as a predictor of admission ( $WW2$ ). The dependent variable was first analyzed in terms of 5 specifications.

The first of these specifications, discussed in an earlier paper (Marshall and Funch, a), is prompted by the assumption that the level of hospital admissions partially adjusts to changes induced by exogenous factors, including the economy, hospital capacity and World War II. This specification is recommended in a recent study of time-series data analysis techniques (Land).

The second through the fifth specifications represent possibilities suggested by Brenner. They are included to illustrate the consistency of the results obtained with those that would have been obtained by the use of trend control procedures such as those Brenner used. The second specification posits that the sum of residual external forces can be represented by a linear trend ( $ID$ ). Estimates based upon specification 2 are thus adjusted to control for a linear trend in the data. The third specification implies that control for external forces can be accomplished by control for the exponent of a linear trend ( $EXP(ID)$ ). The fourth specification suggests that residual external forces should be represented by the logarithm of a linear trend



( $\text{LOG}(\text{ID})$ ), and the fifth, that external forces can be most reasonably represented by the inverse of a linear trend ( $\text{IN}(\text{ID})$ ).

In Table 1, the rate of admission is regressed by the use of ordinary least-squares (OLSQ) on the indicators of the economy, hospital capacity, and World War II, and either the previous admission rate or one of the four trends. Estimates based on these specifications give no support to the notion that the economy depressed the rate of first admissions to mental hospitals. The estimated effect of economic conditions is positive, rather than negative. The results suggest that, during the years of World War II, hospitalization was depressed. The analyses provide somewhat inconsistent evidence that increases in capacity rates resulted in increases in admission rates.

Because of the possibility that autocorrelation may have confounded these results, separate analyses were performed using the iteratively generated autocorrelation adjustment procedure devised by Cochrane and Orcutt (CORC).<sup>5</sup> The results, not presented, supported the ordinary least-squares estimates.

As noted earlier, Brenner suspected that a relationship may have existed only over a part of the period studied, that the exogenous effects measured are not constant. Whether the effects of the economy and hospital capacity vary with time can be considered by including in the estimating equations terms representing the product interaction of time ( $T$ ) and the economy and of time and hospital capacity (Stolzenberg). If the estimating equation were written as follows:

**Table 1.** OLS ESTIMATE OF DIRECT EFFECTS ON YEARLY FIRST ADMISSION RATE, BY SPECIFICATION, 1916-55

Specification	Economy	WW2	Capacity	Previous Level	Trend***	R <sup>2</sup>	D.W.
1.	.01 (5.22)*	-16.74 (- 4.43)	.10 (1.55)	.27 (3.00)	--	.65	1.59
2.	.01 (2.84)	-17.31 (- 4.85)	.26 (3.09)	-- --	.55 (3.48)	.67	1.20
3.	.01 (4.53)	-16.56 (- 5.03)	.01 (.10)	-- --	.00** (4.54)	.72	1.35
4.	.01 (4.19)	-18.83 (- 5.26)	.27 (2.96)	-- --	15.56 (3.15)	.66	1.18
5.	.01 (5.43)	-19.85 (- 5.53)	.28 (2.90)	-- --	-417.95 (-3.00)	.65	1.18

\*t scores in parentheses.

\*\*0 to 2 significant figures.

\*\*\*The trends in specifications 2 through 5, respectively, are linear, exponential, logarithmic, and inverse.

$$(6) \quad AD = \infty + \beta_1 E + \beta_2 CAP + \beta_3 WW2 + \beta_4 AD(-1) + \beta_5 E \cdot T + \beta_6 CAP \cdot T + e_6$$

the total direct effect of the economy ( $\partial AD / \partial E$ ) would be equal to  $\beta_1 + \beta_5 T$ . The total direct effect of hospital capacity ( $\partial AD / \partial CAP$ ) would be equal to  $\beta_2 + \beta_6 T$ . The total effect of the economy would be zero at the point in time where  $T$  is equal to  $-\beta_1 / \beta_5$ . Similarly, the total effect of hospital capacity is zero at the point where  $T$  equals  $-\beta_2 / \beta_6$ . Whether these total effects are positive or negative at greater or lesser values of  $T$  depends upon the signs of  $\beta_1$ ,  $\beta_2$ ,  $\beta_5$ , and  $\beta_6$ .

To allow for the possibility that substantial but inconsistent effects are present in these data, we used this specification to analyze the first admission rate. We included, then excluded the lagged value of the admission rate. Also to correct for possible problems with autocorrelation, we used the Cochrane–Orcutt procedure for transforming the data.

Estimates based on this specification are in Table 2. It can be seen that the estimates are consistent across specifications. They indicate that the zero effect point for the economy is very nearly in the center of the series, between 1936 and 1938. Before that year the economy's impact was negative. After that year, this impact is estimated to have been positive. The effect of hospital capacity is estimated to have been positive over the entire period analyzed.

Taken together, then, these results provide consistent evidence, although that consistent evidence does not confirm what Brenner's analyses suggest. That is, we find, first, that the effect of economic conditions was positive, rather than negative. Even if a time-dependent effect structure is posited, it is estimated that the effect of the economy was positive over nearly half of the period analyzed.

Second, we found that, in Buffalo, the capacity of the hospital tended to exert a positive effect on the level at which people were admitted.

Third, we included a variable for the years during which World War II was taking place. We found that estimated war impact to be consistently negative.

## Discussion

It is intriguing that findings which can appear to be as strong as those discovered by Brenner on a systemwide basis do not pertain to more limited subpopulations and jurisdictions. The difference between these findings, the findings of Brenner's earlier study, and the Marshall–Funch replication of Brenner, might suggest that the use of econometric modeling techniques to represent complicated social processes should be undertaken with great care. These techniques can provide powerful insights into the

Table 2. ESTIMATES OF TIME-DEPENDENT EFFECTS ON YEARLY FIRST ADMISSION RATE BY ESTIMATING PROCEDURE AND SPECIFICATION

Estimating Procedure	Variable							R <sup>2</sup>	DW
	Economy	Capacity	WM2	Economy x Time	Capacity x Time	Lagged Value	Economy-No Effect Point		
1. OLS	-.05 (-2.83)*	.46 (4.47)	-12.09 (-3.48)	.00** (3.440)	-.01 (-2.46)	--	1937	.76	1.73
2. CORC ( $\hat{P} = .137$ )	-.05 (-2.39)	.43 (3.682)	-12.88 (-3.46)	.00** (2.95)	-.01 (-2.08)	--	1936	.77	1.97
3. OLS	-.05 (-2.50)	.43 (4.16)	-11.55 (-3.320)	.00** (3.09)	-.01 (-2.31)	.1 (1.22)	1936	.77	2.03
4. CORC ( $\hat{P} = -.352$ )	-.03 (-1.84)	.29 (3.04)	-8.15 (-3.02)	.00** (2.31)	-.00** (-1.86)	.49 (3.98)	1935	.81	2.21

\*t scores in parentheses.  
\*\*0 to 2 significant figures: calculation of no effect points based on estimates carried to more significant figures.

dynamics of social processes, but they need to be supplemented by careful historical documentation and by the study of individual level data. Each approach, we would suggest, is necessary. Statistical modeling techniques are elegant and efficient, but they may gloss over complexities. The historical technique may be less efficient, but the complexities of historical or organizational processes are the stuff of study. The study of individuals is difficult and costly, but less susceptible to many forms of bias than examination of temporal fluctuations among aggregates.

These findings, too, should be carefully qualified. Any of these estimates could be seriously biased as a result of our failure to include other, poorly understood but potentially powerful external forces in our estimating equations. The accuracy and validity of our indicators are also vulnerable to criticism. Although economic, admission, and capacity data such as these have been used in other studies, they are clearly imperfect indicators. The economy, for example, is not unidimensional. At any one time, certain sectors of the economy may be booming, while others are depressed. Similarly, administrative estimates of the number of beds an institution can adequately provide do not completely reflect a hospital's ability to offer care to people in distress.

The finding that World War II substantially decreased the level of admissions to the psychiatric center may suggest that something like an increase in social cohesion resulted in a decrease in admissions. An examination of annual reports from the hospital, however, presents a somewhat more administratively oriented picture. (New York State, 1942, 1943, 1944)

The report for the year ending in March 1942 suggested that a shortage of personnel resulted in a strain of resources. The following year's report included a reference to the large number of staff serving with the armed forces, and the 1944 report noted that a 33 percent staff vacancy rate due to the war was particularly burdensome. Alternatively, the drop in admissions may have resulted from the removal of a sector of the population whose members would otherwise have contributed substantially to the admission rate. Whether the decrease in the admission rate resulted to any great degree from changes in the social psychological state of individuals in the region surrounding the center is not known.

## Conclusions

The most striking finding of Brenner's work is sustained by our research, since a long-term link between mental hospital admission and the economy is observed. However, the evidence compiled in this study does not support Brenner's assertion of an *inverse* relationship between employment and mental hospitalization. Our findings are consistent with Eyer's research. This finding is suggested, though not emphasized, in Brenner's

original study. Several likely, alternative specifications were considered in an attempt to explain the inconsistency of Buffalo's mental hospitalization patterns with those of New York State taken as a whole.

It is possible that the environment around Buffalo is exceptional—that the experience of the Buffalo Psychiatric Center is a serious exception to a very general and pervasive pattern. Yet we know of no reason to accept this possibility. More plausible is the alternative that the relationship of mental distress to economic conditions and to the use of mental health facilities is more complex than has been suspected. It may even be that the findings of Brenner are confounded by aggregation biases and misestimations.

Future research should more closely consider the effects on admissions of changing hospital administrations. In an unpublished analysis of a four-year period (1974–1978) at the Buffalo Psychiatric Center, a period of considerable change in the in-care patient census, Stryjewski paid close attention to the unique effects of different hospital directors. He also considered a number of other administrative factors. Some of these have thus far been ignored in the empirical literature, e.g., the performance of other major care providers in the area, the internal administrative shifts in the daily operation of the institution, and policy changes mandated by state and federal agencies.

Future research should also assess the links between economic and social change and the full set of mental health effects, not merely first admissions to one inpatient facility. Readmissions and length of hospital stay may be even more affected than first admissions by such factors as changing economic conditions, family resources or toleration of deviant behavior (Brenner, *a*, 73). Study of these factors would be of interest to health care planners and providers. A careful analysis of the links between economic change and the full range of mental disorders is also needed because evidence now points to such links for mild forms of psychological distress (Catalano and Dooley, *a*) as well as the types of disorders leading to hospitalization.

Research is needed on the mental health effects of other dimensions of employment, not merely its presence or absence. The inconsistency of the relationship between employment and hospitalization may reflect the stressful character of work in a large metropolitan area like Buffalo, although we do not have the individual-level data necessary to confirm this.

Thus far, research of this type has not adequately considered the changing historical context of mental health care. Previous models have assumed that unemployment, hospital capacity and hospitalization were the same in 1916 as in 1955, yet we can be certain that they were not experienced as the same events for the individuals involved or for their community. Our Table 2 analyses present a limited consideration of changes over time in the impact of unemployment and hospital capacity on hospi-

talization. The findings suggest a change in the relationship between admissions and employment around 1937. At this time, changes in U.S. social policy permitted reforms such as industrial unions, enhanced unemployment benefits and social security, reforms which may have lessened the stress of unemployment. Buffalo is a heavily unionized area whose labor force may be shielded from some of the most stressful effects of unemployment by a variety of union-sponsored programs such as supplemental benefits to those laid off. This may help to account for the failure of these analyses to confirm the statewide data analyses.

Inasmuch as the purpose of such studies is the disentangling of epidemiologic processes, more studies of the social careers of individuals are needed. Panel studies, such as that of Kessler and Cleary, are required to provide definitive conclusions about the relationship between economic conditions and mental hospitalization. Retrospective case-control studies may be of more use than has been supposed in attempts to unravel the complex of factors leading to psychiatric hospitalization.

### Notes

1. We are indebted to Abbot Weinstein for calling our attention to the possible variance within the state system of mental hospitals.
2. A recent paper (Ratcliff), has noted that the procedure in question, an iterative selection of the longest time span during which a given relationship persists, may leave important questions unanswered. In a series of  $n$  years, there may be a series of  $n - k$  years marked by a certain characteristic.  $k + 1$  overlapping but different series of  $n - k$  length can be taken from the series. If  $k$  were small relative to  $n$ , then the dependence between the separate series of length  $n - k$  would be great and the use of the procedure would be convincing. In the data Brenner considered for Buffalo,  $k$  is large; it is over one-fourth the male series length, and over half the female series length. Brenner described this procedure as designed to assess the consistency of the relationship between the economy and mental hospitalization. To address the question of consistency, we shall suggest that the use of an interactive specification provides somewhat more interpretable estimates than the procedure presented by Brenner.
3. An index of manufacturing employment (New York State, 1959) in Buffalo was available for part of the period studied (1919-48). The correlation of this index with that of statewide manufacturing employment was in excess of .9.
4. These counties were responsible for over 98 percent of all first admissions during the census years which span the analysis.
5. See Hibbs for an explanation of procedures for estimating autocorrelation and transforming the data to adjust for its presence.

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# **The Epidemiological Network Survey: A New Tool for Surveying Deviance and Handicaps—A Research Note\***

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The Epidemiological Network Survey (E.N.S.) is a new research tool for assessing the prevalence of certain types of deviant behavior and handicapping states such as drug abuse, delinquency, and being victim of street violence. Based on the key informant method in anthropology, it employs randomly selected community respondents to provide information on groups of anonymous acquaintances. The instrument was developed to survey problems about which official records and self-reports have both proved inadequate in the past even when confidentiality is assured, alcohol abuse being one well-documented example of the latter.

The technique is relatively simple. One first asks questions concerning the informant himself and his experience with the target conditions. After that, he is invited to make a private list of acquaintances living within a designated area but not in the same household, such as relatives, friends, neighbors, work colleagues, and club members. He should possess a good general knowledge about the listed persons, but does not need to feel close to or like them. The names are written on a sheet with numbered lines which he keeps to himself, and at this point he is merely asked how many men and women are listed, a recheck being asked to ensure that all fall within the research parameters (area, age range, etc.). One then proceeds to ask about a number of target and other conditions in the following fashion: "Is there anyone on your list whom you know to have had, or to have shown signs of, \_\_\_\_\_ during the past X months?" (Preferably, the condition which is the main target of the research should at this stage be made less prominent by mixing it with others which can act as controls on the representativeness of the results). The final stage of the interview consists of asking about the broad social characteristics of the persons indi-

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cated to have each condition, and checking on the type of evidence on which the informal diagnosis was based, using printed lists of behaviors or symptoms.

Since each interview gathers information on numerous persons, one can in principle cover a given size of sample at much less cost than with self-report surveys.

### **The Target Population**

The people about whom information is collected are not the informants themselves, as in the usual self-report survey, but others. This has both advantages and disadvantages. Ordinary population sampling for self-report customarily fails to reach people whose work takes them out of town, those with no fixed residence, and those who refuse to be interviewed, the last often comprising a quarter of the designated sample. Since deviant and handicapped individuals are more difficult to reach than others, the foregoing failure becomes more serious when these are being surveyed than when more normal states are the target. However, the person who cannot be directly reached is quite likely to appear on another person's list of acquaintances, and hence be drawn into an E.N.S. sample, which thus should yield better results in this respect. Moreover, substituting an available respondent for an unavailable one should be less distorting with the E.N.S. than with a self-report approach. On the other hand, however, persons who have many acquaintances have a greater chance of appearing in an E.N.S. sample than persons with few acquaintances, so that it is desirable to assess whether one's target condition is likely to particularly affect either the person with many acquaintances or the recluse.

### **A Test of the E.N.S.**

A test of the instrument was undertaken in a Canadian mill town of 50,000 inhabitants, with sub-studies in a metropolitan district and a smaller rural center. There were five conditions explored: alcohol related problem, nervous breakdown, paralysis or palsy, serious lung problem, and chronic disability following work accident, the first of these being the main target. A total of 475 interviews were undertaken in the mill town, 435 of which were completed satisfactorily. Less than 5 percent were unwilling or unable to prepare the list of acquaintances, among which were a certain number of recent arrivals. The average number of acquaintances per list was 17.7, with males recording slightly more names than females.

To select informants, the town was divided into 25 districts on the basis of the census tracts and two selection modes were employed. In the

first mode, individual residents were chosen on the basis of every  $N$ th name on the voters' roll. Either the listed person or a specified alternate from the same household had to be interviewed, even if several visits were required. In the second mode, sometimes named the "random walk" method, the districts were divided into street blocks and the field staff instructed to interview the first willing person (two if the block was a long one) in each block, the only restriction being that the sex ratio of respondents obtained from any district should not fall outside 40–60 percent limits. With both modes, respondents had to be between the ages of 25 and 65, to have been resident in the town for at least a year, and not be a relative or close acquaintance of the interviewer. As a check on compliance, interviewers were warned that 10 percent of the sample would be re-contacted by supervisory staff. No significant differences between the results obtained by the two modes were uncovered with respect to the demographic characteristics of the informants, the sizes of their lists of acquaintances, and the characteristics (including frequency of target conditions) attributed to these acquaintances. Accordingly, it appears likely that the E.N.S. will provide savings not only in the number of persons covered by a single interview, but also in the cost of obtaining that interview.

We also had two types of interviewers, public health nurses and community college students. The nurses elicited a higher percentage of acquaintances with some disorder: 2.1 percent of acquaintances on their respondents' lists had a problem with alcohol as compared to 1.5 percent for the students. But their lists of acquaintances were generally shorter (25 percent with fewer than 11 names as-compared to 5 percent for the students) a factor which, as we will show, contributes to a higher percentage of positives. When the difference in length of list is allowed for, the students' results were as good as the nurses', permitting further economy; and inexperience can be compensated for by the fuller use of printed lists of features for each condition.

Table I gives the basic findings from the main trial survey, and also the results of assessing how far degree of intimacy is important. If the likelihood of an acquaintance being reported as suffering from a condition depended on the degree of intimacy, one would expect persons at the beginning of the lists to be more often cited than persons at the end, because they were more likely to be close relatives. The last two columns in Table I indicate the mean percentile ranks observed for persons named as having each disorder, and whether these mean ranks fell within chance range of the 50th percentile, as would be expected if intimacy were irrelevant. As can be seen, the difference was within the chance range for three of the five conditions, markedly outside for one condition, "serious trouble with the lungs" (a problem with lower visibility), and moderately outside for the primary target condition. Having a mean percentile rank of 44 instead of 50 suggests that the method used for prompting respondents as

**Table 1.** CONDITIONS ENQUIRED ABOUT, PERCENT OF ACQUAINTANCES AFFECTED, AND MEAN PERCENTILE RANK OF THESE ACQUAINTANCES IN THE ACQUAINTANCE LIST

Condition	Number of Positives	Percentage Affected	Mean Percentile Rank	Deviation from 50th Percentile
"Serious trouble with lungs"	90	1.20	35.1	$p < .01$
"Paralysis or palsy"	47	0.63	50.1	NS
"Nervous breakdown"	135	1.80	46.5	NS
"Troubles because of drinking"	141	1.88	44.0	$p < .05$
"Chronic disability from work accident"	68	0.91	44.7	NS

they made their lists was reasonably satisfactory, but that the last category used for the preparation of the list, "people you know well at a church or club" should probably be dropped. Thus, we suggest that for groups such as those studied here, one should aim at a maximum of 15 acquaintances per informant instead of 20 as was our goal then.

We had the possibility of checking the convergent validation for part of our data. One of the items on the prompt-card for "troubles because of drinking" was "received medical treatment for illness caused by drinking," and we were able to obtain access to the only local unit providing medical treatment for alcoholism. On the basis of that unit's records, 0.87 percent of the males aged 18+ in the community had received such treatment in the past year, and 0.18 percent of females. From the E.N.S., 0.72 percent of the male acquaintances and 0.18 percent of females were reported to have had such treatment, figures remarkably similar to those for people who were actually receiving treatment. A second approach to convergent validation consisted of comparing the E.N.S. rate for alcohol abuse as a whole with the proportion of informants who answered positively to the question, earlier in the interview: "Has your drinking been giving you any trouble during this past year . . . I mean things like physical illness, problems at work, family troubles, or tension and irritability?" One and a half percent of respondents indicated such trouble, and if one corrects the 1.88 percent indicated in Table I to allow for duplication, the resultant rate of 1.67 percent is insignificantly different. Finally, when trying out various versions of the interview schedule with some 150 respondents in a metropolitan area which has a mean alcohol consumption level about double that of the mill town, it was found that 3.6 percent of acquaintances were reported to have problems with alcohol, also about double the level in the mill town. The survey results, therefore, accord with information gathered by other means.

In another survey in a town of 10,000, we checked if a list of people

recognized as suffering from alcohol abuse by local medical professionals were more known or were less known within the population than another list of randomly chosen people. For this purpose, we mixed both lists of names and we asked a sample of 100 respondents to indicate the names of the persons they had spoken to during the last three months. The alcohol abuse group were significantly better known, which went against our expectations, but it was later found that their social class was much higher than the random list. Besides, many of these abusers happened to be local merchants. That effort at assessing whether alcohol abusers were more or were less likely to appear on acquaintanceship lists was thus a failure, but we think that the model is one which could profitably be employed with a little more care.

### **Conclusions**

The trial of the E.N.S. has satisfactorily shown that the instrument gains easy acceptance from the public when anonymity is guaranteed. The administration is easy and does not require high training, but we recommend that the list of acquaintances be limited to 15 names. Further research is obviously needed to know what kinds of extrapolation can be made from the results, but the technique appears to constitute an advance when self-reports are expected to be unreliable and when one seeks to cover a relatively large population sample at low cost.

# A Research Note on the Demography of Occupational Relocations\*

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Explanations for job-related migration most often assume that migration is effected by an impersonal market process whereby geographic differences in wages and salaries overcome the reluctance of individuals to change their place of residence. Consequently little is known about job-related migration in which employers do not rely on impersonal market forces, but instead exert more direct control by linking employment and opportunity structures at origin and destination locations. This note shows that relocations are a frequent and increasing part of migration phenomena and provides baseline demographic information about the extent and characteristics of occupational relocations in American society.

For past time periods, the March 1963 Current Population Survey remains one of the best sources of demographic information about occupational relocations. This survey estimated that among males aged 18 to 64, 268,000 intercounty and 28,000 intracounty changes of residence occurred because of an occupational relocation during the preceding 12 month period. The occupational distribution of these relocated men clearly illustrated that relocations occurred within all major occupational categories.

More recent information on the extent of occupational relocations comes mainly from industry sources such as moving and storage firms and personnel publications. Two important points emerge from a review of these sources. First, relocations are almost completely viewed as a phenomenon related to salaried employees. Sociological literature on complex organizations, although silent on the frequency of relocations, likewise treats relocations only from the perspective of occupational mobility among upper echelon personnel. Second, the frequency of relocations within the United States during the 1970s was most often estimated to be approximately two to three hundred thousand employees per year.

In a preliminary effort to incorporate consideration of occupational relocations into both research on migration and complex organizations, this research note raises these three questions.

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1. What is the frequency of occupational relocations in the contemporary United States and how does this compare with other estimates?
2. Do occupational relocations appear to have a net effect on the population distribution of the United States or do relocations suggest self-replenishing circulation?
3. What are the characteristics of employees and their families who are occupationally relocated?

### Data and Methods

The data for this research were obtained from the national sample public use tapes for the years 1973–77 of the Annual Housing Surveys (AHS) collected by the U.S. Census Bureau for the Department of Housing and Urban Development. When appropriately weighted the AHS is representative of U.S. households and the sample size ranges from about 50,000 in 1973 to about 70,000 in 1977. The questionnaire schedule as well as various definitions (e.g., metropolitan and non-metropolitan) have been relatively consistent throughout this time period.

Of major interest for this paper is a question asking respondents in households in which the head had moved during the previous 12 months "What is the main reason . . . (head) moved from his previous residence?" Interviewers were instructed to record all the reasons given and then inquire about the main reason for moving. This main reason was coded into one of 31 categories (29 in 1973). Most of the reasons for moving concern family changes, housing items, and the neighborhood issues because most households were local movers. Seven of the reasons are treated by the Census Bureau as economic reasons and these are: job transfer, entered or left U.S. Armed Forces, retirement, new job or looking for work, commuting reasons, to attend school, and "other" economic. This paper analyzes those households reporting that the head of the household moved from his or her previous residence because of a job transfer. Respondents were excluded if they were known to have moved from outside the U.S., although this exclusion was not always possible with the 1974 data.

All households were asked additional questions which provide descriptive characteristics such as age, education of head of household (not asked in 1973), race, previous residential location, income, etc. All income figures have been converted to 1977 dollars using the Consumer Price Index. A major deficiency in these data is that there is no information on occupation.

## Findings

Table 1 shows the AHS estimates of the number of relocated household heads and of persons associated with such relocations. These data show that during the mid-1970s an average of about 800,000 employees were moved each year. About 750,000 were males aged 18 to 64. Compared to the estimated number of 296,000 similarly-aged males who transferred during the 1962–1963 period, this figure indicates that number of relocations has dramatically increased. During the same period, the size of the 18 to 64 male labor force increased from about 43 million to 52 million and although this moderates the impact of the increase to some degree, it demonstrates that the rate of occupational relocations has more than doubled. Table 1 also shows the yearly variation in the number of relocations ranging from 726,000 in 1975 to a high of 914,000 in 1977. Although corporate surveys appear to have considerably underestimated the extent of relocation, the finding that 1977 was a “record year” for relocations has been noted by personnel managers.

Estimating the number of persons associated with such relocations is less direct because the AHS is a post-move survey. Some households were created or household sizes may have changed either during or since migration occurred. For the “persons relocated” estimates it was: (1) assumed that persons becoming household heads in conjunction with the relocation, regardless of the number of persons in the household at the time of the AHS survey, moved alone; and (2) that households keeping the same household head relocated with household members present at the time of the AHS survey. On this basis an average of about 2,400,000 persons annually were associated with a relocation during the mid-1970s or slightly greater than 1 percent of the U.S. population, a number which

**Table 1.** NUMBER OF HOUSEHOLD HEADS AND NUMBER OF PERSONS IN HOUSEHOLDS (IN THOUSANDS, WHERE HEAD WAS RELOCATED BY YEAR U.S.A. 1973–77)

	Year					Total
	1973	1974 <sup>a</sup>	1975	1976	1977	
Number of household heads	761	832	726	797	915	4031
Percent of total	18.9	20.6	18.0	19.8	22.7	100
Number of persons <sup>b</sup>	2329	2442	2205	2283	2686	11948
Percent of total	19.5	20.4	18.5	19.1	22.5	100

<sup>a</sup>Movers from abroad included in 1974.

<sup>b</sup>Certain assumptions are made in estimating the number of persons. See text.

could easily effect a change in the population distribution of the United States.

Table 2 displays the distributions of relocated household heads and persons in those households by metropolitan and non-metropolitan migration streams and region. Turning first to the stream totals, almost one-half of the relocations of both household heads and persons occurred between metropolitan areas, resulting in no net change of the metropolitan population. Although a somewhat greater number of household heads moved in the metropolitan direction and a few more persons moved in the non-metropolitan direction, these differences are small enough to be accounted for by sampling variation. Thus these data show that in spite of the movement of almost 12 million persons, relocations had little impact on the size of the metropolitan and non-metropolitan populations of the United States during the 1973 to 1977 period. In other words, with respect to the metropolitan/nonmetropolitan dimension of population distribution, occupational relocations represent circulation rather than redistribution.

Table 2 also suggests regional variation in the levels of occupational relocations. The South was the destination of 41.5 percent of all relocations although this region contains only 31.9 percent of U.S. households. By contrast the Northeast was the destination of 12 percent of all relocations but contains 22.5 percent of all households. Unfortunately the AHS data do not allow tabulation by the region of origin and thus the data are equivocal as to whether the South has both a higher origin and destination for occupational relocations or whether this propensity is limited to destinations. The same is true of the West census region. However other estimates of inter-regional migration for roughly the same period have documented a net flow of persons to the South and West at the expense of the Northeast and North Central regions. It is likely that occupational relocations are part of this movement, complementing notions that climatic and other non-occupational preferences account for the movement of persons toward the "sunbelt."

In Table 3 selected characteristics of relocated male labor-force-aged household heads are presented along with the characteristics for comparable nonmovers. Income of the household head (in 1977 dollars) is higher by about \$3,500 and on the average relocated persons have completed about two more years of schooling. Considering the difference in average age these incomes and education differences are not great since it is well known that both characteristics vary systematically with age. In cross-sectional comparisons both income and education are positively associated with age at younger ages, then negatively associated at older ages. In spite of the fact that the household head's income is less for the nonmover population, family income for this group is greater. This difference is most likely due to more employed spouses among the nonmover group and the lower employment rates for spouses among relocated households is proba-



**Table 2.** NUMBER OF HOUSEHOLD HEADS AND NUMBER OF PERSONS (IN THOUSANDS) OCCUPATIONALLY RELOCATED BY MIGRATION STREAM AND DESTINATION REGION U.S.A. 1973-77

Migration Stream	Destination Region (Number and Percent)										Stream Totals	
	Northeast		North Central		South		West					
	Heads	Persons	Heads	Persons	Heads	Persons	Heads	Persons	Heads	Persons	Heads	Persons
Metro to nonmetro	89 18.3	307 21.2	133 15.9	415 16.1	281 16.8	933 18.5	121 11.7	352 12.2	623 15.5	2009 16.8		
Nonmetro to metro	60 12.4	180 12.4	111 13.2	311 12.1	293 17.5	828 16.4	169 16.3	441 15.3	632 15.7	1759 14.7		
Intermetro	245 50.5	701 48.5	358 42.5	1137 44.2	723 43.3	2114 41.9	496 48.0	1421 49.3	1823 45.2	5374 45.0		
Interstate nonmetro to nonmetro	24 4.9	84 5.8	68 8.0	217 8.4	126 7.5	438 8.7	54 5.2	145 5.0	271 6.7	884 7.4		
Intrastate nonmetro to nonmetro	19 3.9	55 3.8	119 14.1	368 14.3	163 9.7	506 10.0	86 8.3	269 9.3	387 9.6	1199 10.0		
Intra metro	49 10.0	119 8.2	53 6.3	136 4.9	86 5.2	222 4.4	108 10.4	257 8.9	295 7.3	724 6.1		
Region totals for relocations	485 12.0	1446 12.1	841 20.9	2574 21.5	1671 41.5	5042 42.2	1034 25.6	2885 24.1	4031 100	11948 100		
Regional household head distribution in percent	22.7		26.7		31.9		18.7		100			

Notes: 1973-77 Annual Housing Surveys combined with metropolitan definitions of 1970. Region household head distribution calculated from 1973-77 AHS series D publications.

**Table 3.** SELECTED CHARACTERISTICS OF MALE LABOR FORCE, AGED 18-64  
OCCUPATIONALLY RELOCATED AND NONMOVER HOUSEHOLD HEADS U S A 1973-77

Characteristic (Standard Deviation)	Relocated	Nonmover
Average age	34.1 (9.7)	43.8 (11.8)
Average family income <sup>b</sup>	\$19,140 (11,012)	\$19,392 (11,859)
Average income of head <sup>b,c</sup>	\$15,510 (9,479)	\$12,083 (9,454)
Average education of head <sup>c</sup>	15.1 (2.5)	13.2 (3.3)
Percent nonwhite heads	6.2	9.8
Percent households with head and wife	84.2	89.3
Percent homeowners <sup>d</sup>	40.0	77.4
Percent different head	9.6	--
Percent SMSA residents	67.5	68.2

<sup>a</sup>Based on a 20 percent random sample of the AHS sample.

<sup>b</sup>In 1977 dollars based on the Consumer Price Index. Income of head from wage and/or salary only.

<sup>c</sup>Excludes 1973.

<sup>d</sup>Includes condominiums.

bly an effect of the relocation itself. Relocated households are more likely to be renters, a characteristic shared with migrants in general.

## Conclusions

It has been widely assumed and in fact sometimes advocated that market forces provide the connecting process by which the geographic disjunction between persons and jobs available is in some sense resolved. Although not wishing to make the case that occupational relocations are totally outside the market process, directed intraorganizational transfers and market induced interorganizational migration have important differences. Non-market allocation of labor (broadly defined) in contemporary times has in the main been ignored excepting processes associated with certain forms of migrant and foreign labor. During the mid-1970s about 800,000 persons were relocated per year, a number suggesting that this form of non-market labor allocation is both widespread and has increased substantially since the early 1960s.

The prevalence of occupational relocations also suggests that at the individual level the notion of market mechanisms effecting migration may sometimes be inappropriate. Migration research in the human capital tradition explicitly models migration as a micro-economic process. In this perspective, factors which attach individuals to particular firms, such as vested pension rights, particularistic opportunity structures, personally useful communication networks and so forth, are thought to inhibit migration and the market allocation of labor. With directed relocations the opposite may be the case in that these same factors may create organization dependence thereby stimulating migration by inducing individuals to comply with relocation offers.

It has been suggested that the benefits of migration may not be as apparent as they were when the predominant flow of migrants was from rural to urban areas, since urban areas where most persons now live have a wide range of occupational possibilities. If this is indeed the case, the increased use of occupational relocations may be an organization response to the inability of corporations to rely on traditional market inducements. Specific organization dependencies have contributed to the usefulness of this directed labor allocation process.

Over the 5 years analyzed, the impact of occupational relocations upon the metropolitan and nonmetropolitan population distribution of the United States was slight. Although large numbers of persons were moved in various directions, the process was one of circulation rather than redistribution. It is not clear whether the same statement would apply to the distribution of population among the major regions of the United States. It is certain that the growing sun belt regions have been the destinations for a disproportionate number of relocations. If these regions have been the origins as well, it would be important to determine why industries in these two regions use relocations with greater frequency. However, because it has been documented elsewhere that migration into these areas is an important factor in their growth, I suspect that relocations into these areas have been part of this migration and thus have contributed to the redistribution of population toward the sun belt regions. Thus it may be incorrect to ascribe the relative decline of the Northeast and Northcentral regions to unalterable climate differences; it may, instead, be at least partially attributable to elements of corporate policy. Unfortunately, little is known about corporate uses of personnel relocations and much less how such practices could or do result in a regional redistribution of population.

# Alternative Methods of Measuring Role/Identity: A Research Note\*

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In a recent article, Burke and Tully present and illustrate a general procedure for measuring role/identities. The procedure is based on symbolic interaction theory and requires use of semantic differential rating scales. Respondents rate the role/identity of interest and appropriate counter-role/identities on the same set of bipolar adjectives. Discriminant function analysis is used to generate the set(s) of item weights that best discriminate among the role/identities. In cases where more than one dimension is common to the role/identities, multiple discriminant functions will be generated. The discriminant functions are assumed to represent the dimension(s) that distinguish social stereotypes of various role/identities. The item weights based on the discriminant analysis of the abstract role/identities categories are then applied to respondents' self-ratings of the same adjectives and can be used to calculate individual role/identity dimension scores. As Burke and Tully note, this measurement procedure has several important properties in that the technique (1) is theoretically grounded and integrates the concepts of self and role, (2) generates quantitative scores suitable for multivariate data analysis, (3) permits assessment of the multidimensional structure of role/identities, and (4) reveals the substantive meaning of the characteristics or dimensions that generate quantitative role/identity scores.

Although the Burke-Tully measuring procedure has a short history, variations in the measurement technique already have appeared. At least three available papers (Reitzes and Mutran; Reitzes and Burke; George et al.) are based on the standard Burke-Tully procedure just described. Two other papers (Mutran and Burke, a, b), however, use a variant of the Burke-Tully procedure in that respondents do not rate the role/identities of interest. Rather, the self-ratings of respondents in different roles are compared and mathematically discriminated. It is then assumed that the differences in self-ratings represent typical (as opposed to stereotypical) identity

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differences associated with role occupancy. This variant procedure was described by Burke and Tully as an alternative technique for the measurement of role/identity (note 5). Burke and Tully also caution, however, that the two techniques may generate different results.

The purpose of this report is to compare the stereotypical and typical methods of measuring role/identity. This empirical illustration will take the form of calculating both stereotypical and typical role/identity scores for the same respondents. These two role identity scores will then be compared in terms of (1) dimension content (i.e., the number and nature of the adjectives that distinguish role/identity categories), (2) item directions and weights (i.e., values of the discriminant function coefficients), and (3) relationships with variables that, on theoretical grounds, should be significant correlates of role/identity.

The specific role/identities chosen for this illustration are age role/identities—the components of identity that are related to middle and old age. The focus on age role/identity has both advantages and disadvantages for the purpose at hand. The disadvantage is that any observer-imposed demarcation of age groups (as is required in calculating typical age role/identity scores) is ultimately arbitrary. There are at least two advantages of an examination of age role/identity, however. First there is a relatively broad body of literature describing the hypothesized and empirically substantiated correlates of age role/identity in later life (see Cutler; Peters for reviews). Second, age identity traditionally has been measured by a single-item, direct question that requires respondents to choose the age category that best suits them. Previous research indicates that respondents are willing and able to identify the age categories to which they belong. The availability of a measure of age identity, distinct from both the stereotypical and typical age role/identity measures derived from the Burke–Tully procedure, will provide another basis of comparison.

## Methods

### SAMPLE

The sample consists of 341 men and women, age 47–96, residing in central North Carolina. Subject names were obtained from a list of middle-aged and older research volunteers maintained by the Duke University Center for the Study of Aging and Human Development. Questionnaires were mailed to the 524 persons, age 45 and older, registered in the subject pool. The 341 respondents included in this analysis comprise those persons who returned questionnaires with complete data on all the variables of interest. Thus, the data analysis is based on 65 percent of our original sampling frame.

## MEASURES

A single-item measure of age identity was included in the questionnaire. Respondents were asked: "Which of the following terms best describes you . . . middle-aged or old?" The two other measures of age identity were derived from a series of semantic differential measures, all with the same set of bipolar adjectives: nervous-relaxed, secure-insecure, worthless-valuable, free to do things—not free to do things, useful-useless, look to the future-look to the past, effective-ineffective, satisfied-dissatisfied, shaky-steady, active-inactive, respected-not respected, sick-healthy, unsure-confident, refreshed-tired, and happy-sad. The referents included "An Old Person," "A Middle-Aged Person," and "Myself."

The other variables in the analysis included age coded in years; education categorized from 1, "grade school or less," to 7, "graduate degree"; occupational prestige ranged from 1 to 9 based on the first-digit coding scheme of the Census Bureau; total family income was coded in 7 categories with "7" representing an annual income of \$25,000 or over; gender, widowhood, and retirement status were dummy variables with one representing women, widowed, and retired. Health was measured in a number of ways. First, respondents were asked to rate their own health from poor to excellent. They also reported the number of days during the past three months that illness or disability rendered them unable to perform their usual activities, the number of times they had visited the doctor in the past three months due to illness, and the number of hospital admissions during the past three months. Attitudinal measures included the Life Satisfaction Index (LSI) developed by Neugarten et al. and the Affect Balance Scale (Bradburn). The latter instrument taps relative transitory affect and generates three separate scores: quantity of positive affect, quantity of negative affect, and the ratio of positive to negative affect.

## Results

### STEREOTYPICAL AGE ROLE/IDENTITY

In order to construct the measure of stereotypical age role/identity, respondents' ratings of "An Old Person" and "A Middle-Aged Person" on the fifteen bipolar adjectives were analyzed using discriminant function analysis. This procedure generated a linear combination of weighted items that best discriminated between the ratings of an old person and a middle-aged person. The discriminant function analysis indicated that twelve of the adjectives significantly discriminated between stereotypes of the two age categories. Table 1A presents the standardized and unstandardized discriminant function coefficients for the twelve adjectives. The discrimi-

Table 1.

## A. Stereotypical Age Role/Identity, Significant Discriminant Functions Coefficients\*

Items	Standardized weights	Unstandardized weights
Insecure-secure**	-.326	-.211
Worthless-valuable	.078	.056
Not free to do things-free to do things	.142	.089
Useless-useful	-.102	-.070
Look to the past-look to the future	-.407	-.234
Ineffective-effective	-.107	-.072
Dissatisfied-satisfied	.298	.204
Shaky-steady	-.153	-.101
Inactive-active	-.292	-.178
Not respected-respected	.254	.200
Sick-healthy	-.242	-.175
Unsure-confident	.144	.102
Constant		1.753

## B. Typical Age Role/Identity, Significant Discriminant Function Coefficients\*

Items	Standardized weights	Unstandardized weights
Worthless-valuable	-.345	-.267
Not free to do things-free to do things	.514	.379
Ineffective-effective	-.574	-.455
Inactive-active	-.463	-.396
Not respected-respected	.629	.611
Tired-refreshed	-.299	-.204
Sad-happy	.446	.356
Constant		-.655

\*All coefficients are statistically significant,  $p \leq .05$

\*\*High score pole of item is given in right column.

nant function in Table 1A has a canonical correlation of .70 and a Wilks' Lambda of .51, distributed as chi square. The function is highly significant ( $X^2 = 441.89$ ,  $df = 12$ ,  $p \leq .0001$ ), and every item comprising the function makes a significant independent contribution to the explained variance.

Interpreting the discriminant function coefficients is relatively straightforward. In the present analysis, a positive score predicts "old." Thus, in this sample, old people are stereotypically viewed as less secure, more valuable, more free to do things, less useful, more oriented toward the past, less effective, more satisfied, less steady, less active, more respected, less healthy, and more confident than middle-aged persons. It is interesting to note, in light of earlier suggestions that age identity as old is a negative self-appraisal (cf. Rosow), that age is characterized by these respondents in both positive and negative terms.

The discriminant function coefficients are used to construct respondents' stereotypical age role/identity scores by multiplying each respondent's self-ratings on each adjective referring to Myself by the appropriate unstandardized discriminant function coefficient, summing these products across the twelve items, and adding that sum to the constant.

#### TYPICAL AGE ROLE/IDENTITY

In order to construct the measure of typical age role/identity, respondents' ratings of the referent Myself on the fifteen bipolar adjectives were analyzed using discriminant function analysis, using a cutpoint of age 65 to distinguish "old" from "middle-aged" respondents. The discriminant analysis thus generated the linear combination of weighted items that best discriminated the self-ratings of respondents age 64 and younger from those of sample members age 65 and older. Table 1B presents the standardized and unstandardized discriminant function coefficients for the seven adjectives that significantly discriminated the self-ratings of the two age groups. The discriminant function in Table 1B has a canonical correlation of .43 and a Wilks' Lambda of .82, which is highly significant ( $X^2 = 66.73$ ,  $df = 7$ ,  $p \leq .001$ ). Each item in this discriminant function also is statistically significant.

As was true in the analysis of stereotypical age role/identity, the adjectives that discriminate between the two age groups suggest that old age has positive as well as negative characteristics. In this sample, respondents age 65 and older rated themselves as less valuable, less effective, less active, and less refreshed than respondents age 64 and younger, but the older respondents also rated themselves as more free to do things, more respected, and happier than the younger group. The discriminant function coefficients presented in Table 1B then were used to construct respondents' typical age role/identity scores.

At this point, it is possible to compare the content of the stereotypical and typical age role/identity measures. Seven adjectives discriminated only stereotypical ratings and two adjectives discriminated only the typical ratings (i.e., the self-rating of the two age groups). Five bipolar adjectives are common to both discriminant functions: worthless-valuable, not free to do things-free to do things, ineffective-effective, inactive-active, not respected-respected. Thus, there is moderate overlap in the content of the stereotypical and typical discriminant functions, although the former contains a significantly greater number of items than the latter. In terms of the common items, the direction of all but one of the five adjectives is in the same direction in both equations. The absolute values of the item weights also differ considerably across the two discriminant functions, and are consistently much larger for the typical age role/identity function.<sup>1</sup>



## CORRELATION ANALYSIS

Table 2 presents the correlations between the two measures of age role/identity, the conventionally used single-item measure of age identity, and the other variables which previous theory and research suggest are correlates of age identity. Examining first the relationships among measures of age identity, the two variations of the Burke-Tully role/identity measurement technique are related at a statistically significant level ( $r = .56, p \leq .01$ ). Given the considerable overlap in the content of the two measures, this relationship is expected. In that both measures are derived from the same conceptual framework, however, the relationship is lower than might be expected. This emphasizes the point made by Burke that the location of

**Table 2.** CORRELATION COEFFICIENTS OF AGE ROLE/IDENTITY MEASURES WITH OTHER VARIABLES

Variable	Stereotypical Age Role/Identity	Typical Age Role/Identity	Single-Item Age Identity
Stereotypical age role/identity	--	.56**	.26**
Typical age role/identity	.56**	--	.30**
Single item age identity	.26**	.30**	--
Chronological age	.26**	.41**	.49**
Gender	-.02	.04	.02
Education	-.04	-.10*	.05
Occupational prestige	-.09	-.15**	-.03
Income	-.22**	-.26**	-.21**
Retirement status	.24**	.33**	.29**
Widowhood	.14**	.17**	.06
Self-rated health	-.46**	-.25**	-.21**
Days sick or disabled	.34**	.13**	.07
Visits to doctor	.28**	.07	.11*
Hospitalizations	.10*	.01	.12**
Life satisfaction	-.22**	-.10*	-.12**
Positive affect	-.15**	-.09	-.19**
Negative affect	.06	.10*	-.08
Affect balance	-.13*	.02	-.07

\* $p \leq .05$

\*\* $p \leq .01$

identities in semantic space may vary, depending on whether we are talking about individual conceptions of typical or stereotypical characteristics. Both variations of the Burke-Tully role/identity measurement technique are significantly correlated with the single-item measure of age identity, but the correlations are of only moderate magnitude. In that all three measures are intended to tap the individual's age identity, the intercorrelations could be expected to be considerably stronger.

The relationships among the age identity measures and the other variables help to clarify the meaning of the variations in age identity measurement. The stereotypical age role/identity measure is significantly correlated with eleven of the fifteen hypothesized correlates of age identity in middle and later adulthood: chronological age, income, retirement status, widowhood, all of the health variables, life satisfaction, and positive affect. The typical age role/identity measure is significantly correlated with ten of the hypothesized correlates: chronological age, the three standard indicators of socioeconomic status, retirement status, widowhood, two of the health variables, life satisfaction, and negative affect. The single-item measure of age identity is significantly related to eight of the correlates: chronological age, income, retirement status, three of the health variables, life satisfaction, and positive affect.

All of the correlation coefficients are in the direction suggested by previous research. Self-identification as old is positively related to chronological age, being retired or widowed, higher levels of illness-related inactivity, higher numbers of doctors visits or hospitalizations, and greater levels of negative affect. Self-identification as old is negatively correlated with income, educational attainment, occupational prestige, self-rated health, life satisfaction, and positive affect. The fact that the three age identity measures are differentially related to a number of the other variables suggests that age role/identity may be multidimensional. In order to test this proposition, a canonical correlation analysis was performed in which the three measures of age identity were simultaneously related to the other variables examined in Table 2. The results of the analysis are presented in Table 3.

Two weighted linear combinations of variables were significantly related to the three measures of age identity. The first variate has a canonical correlation of .589 ( $p \leq .0001$ ) and identifies a linear combination of variables similarly related to all three measures of age identity. That is, although the coefficients for the three age identity measures are not identical, they all are positive and of similar magnitude. Thus, the first variate appears to be a linear combination of variables which is related to the common elements of the three age identity measures. The second canonical variate, in contrast, distinguishes among the three age identity measures. *In particular, the stereotypical age role/identity measure is distinguished from the two identity measures that are based upon self-evaluation. These*

**Table 3.** CANONICAL CORRELATIONS AND SCALING EQUATIONS FOR THREE IDENTITY MEASURES

	Canonical Variate	
	I	II
<u>First Set of Variables</u>		
Chronological age	.335*	-.625
Gender	-.090	-.219
Education	.078	-.070
Occupational prestige	.016	.106
Income	-.214	-.285
Retirement status	.274	-.258
Widowhood	-.035	.141
Self-rated health	-.481	-.487
Days sick or disabled	.019	.135
Visits to doctor	.108	.369
Hospitalizations	.074	-.078
Life satisfaction	.105	.051
Positive affect	-.256	.032
Negative affect	-.024	.182
<u>Second Set of Variables</u>		
Stereotypical age role/identity	.576	1.070
Typical age role/identity	.249	-.723
Single-item age identity	.493	-.536
<u>Canonical Correlation</u>		
Chi square	146.877	64.063
d.f.	42	26
p	.0001	.0001

\*All coefficients are standardized.

results suggest that there are multiple dimensions common to the three measures of age role/identity.

## Discussion

The primary purpose of this paper was to compare variations of the Burke-Tully procedure for measuring role/identity. Stereotypical and typical age role/identity scores were calculated for the same respondents and compared in terms of (1) dimension content, (2) item directions and weights, and (3) relationships with variables that, on theoretical grounds, should be significant correlates of age role/identity.

The results indicated that variations in the application of the Burke-Tully procedure do make a difference. That is, the stereotypical age role/identity and typical age role/identity measures clearly are not equivalent as evidenced by their correlation—.56, a statistically significant but modest correlation. The two measures of age role/identity, as well as the conventionally used single-item measure of age identity, also were differentially correlated with a number of variables that have been hypothesized as significant correlates of age identity during middle and later life. These correlational differences suggest that public images of old age are tied to at least two broad classes of referents: chronological ones and social/personal factors.

This logic can be applied to the pattern of correlations between the stereotypical, typical, and single-item age identity measures and the other variables in Table 2. All of the age identity measures, but especially the single-item measure, are significantly correlated with chronological age. Similarly, all three age identity measures, but particularly the stereotypical age role/identity measure, are strongly related to life satisfaction and at least one component of affect. Beyond these similarities, however, each measure of age identity appears somewhat more strongly related to certain types of referents than to others. Thus, the single-item age identity measure appears most closely related to chronological referents, as evidenced by the strong correlations with chronological age and retirement status (a social event greatly influenced by chronological age criteria). The stereotypical age role/identity measure, on the other hand appears more closely related to social and personal correlates of aging, including loss of the work and spouse roles, and all of the health variables. The typical age role/identity measure generally exhibits the same pattern as the stereotypical age role/identity measure. However, the typical age role/identity measure is uniquely related to socioeconomic status variables—it is the only age identity measure significantly correlated with education and occupational prestige and exhibits the strongest correlation of the three age identity measures with income and retirement status. The three measures of age identity thus may be sensitive to somewhat different aspects of the aging process. These substantive findings reinforce the multidimensionality empirically demonstrated by the canonical correlation analysis.

Conceptual differences between the two variations of the Burke-Tully measurement procedure also are of interest. The stereotypical measurement technique rests on interactionist perspectives of the development and maintenance of self. The individual's role behavior is assumed to be heavily influenced by his or her perceptions of appropriate role behavior and by the behavior or persons occupying complementary roles. Identity is developed by internalization of the perceived behaviors and attitudes associated with role occupancy (Burke and Tully). The stereotypical measure of role/identity clearly maintains a direct link between broadly-endorsed im-

ages of what role occupants are like and the characteristics of individuals. The measure of typical role/identity, however, simply assumes that observed identity differences between occupants of complementary roles are due to occupancy in those roles.

Conceptually, then, if stereotypes reflect broadly endorsed images of appropriate role behavior and identity characteristics, observed differences between role occupants should be related to stereotypical differences. This expected pattern holds true for age role/identity. Five of the adjective pairs used in this study significantly discriminate both stereotypical images of role categories and typical differences in self-perception. The fact that a larger number of items discriminates stereotypical images of age roles suggests that stereotypes represent a broad, general notion of role/identities, part of which is actualized and observed as significant differences between role occupants.<sup>2</sup>

Our data suggest that not all observed differences in identity characteristics need also be significant discriminators of stereotypical images of role categories. Thus, the typical age/identity measure included two adjective pairs not included in the stereotypical discriminant function. There are several possible reasons that differences in role/identity could be observed in the absence of broadly-endorsed images of role categories. First, there may be characteristics of role/identity that are not sufficiently dramatic or poignant to be captured by social stereotypes. Second, social conditions that influence the identities of persons in a given social role may, in some sense, counteract or nullify prevailing social stereotypes. In that stereotypes are by definition abstractions and hence removed from the social circumstances of role occupants, social conditions may generate observed differences among role occupants that are distinct from, or even at odds with, stereotyped images of role categories. Finally, some theorists, especially Allport, contend that stereotypes inevitably are inaccurate. From this perspective, differences between stereotypical images of role categories and observed differences in the characteristics of role occupants would be expected.

In summary, the results of this study suggest that the method used to measure role/identity makes a difference. Different methods reflect different conceptual assumptions and generate different empirical results. Consequently, comparisons across studies and interpretations of data must take such measurement issues into account. Of even greater interest, however, our results suggest the possibility of measuring different aspects of role/identity—each of which is conceptually and empirically meaningful.

## Notes

1. An anonymous reviewer suggested that the item differences in the stereotypical and typical age role/identity measures may be only a result of the stepwise procedure that attempts to derive a best fitting model, thereby emphasizing minor variations in the correlation matrices of

the stereotypical and typical adjectives. We examined the issue by limiting the analysis of the typical self description to only those adjectives that discriminated between the stereotypes of old and middle age. Again only seven adjectives emerged. The first five adjectives in Table 1B remained the same, with little variation in their weights. Two other variables emerged: shaky-steady and unsure-confident. Their coefficients were  $-.378$  and  $.468$  respectively.

2. Five of the seven items that discriminated stereotypical images of age role categories, but not typical age role/identity characteristics focus upon the frailty and fragility of older people. On the average, respondents in this sample were very healthy. Thus, it is not surprising that typical or observed self-ratings of such items did not differ significantly across age groups. This suggests that sample characteristics may influence typical role/identity scores more than they influence more general stereotypical images of role categories.

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# **"Structuralism" or Structure: A Comment on Mayhew**

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Bruce Mayhew (a,b) has written a provocative challenge to sociology in his two recent articles on "structuralism versus individualism." Sociology, he argues, has yielded meager results because it has answered the wrong questions, trying to explain social forms by asking how individual people behave within society. Structuralism may save sociology from this misplaced individualism.

Mayhew urges a change of direction. Sociologists who comment on social structure by looking at the correlates of individual behavior many protest many details of his critique. Even so, we probably should agree that sociology often spends much time investigating the small parts and assuming some larger whole. Clearly individualism is not enough. Is structuralism of the kind Mayhew prescribes a proper alternative?

Rather than individualism versus structuralism, let us consider the matter of individualism versus the study of social structure. Mayhew himself begins with this basic issue, even though his suggestions for a structuralist sociology rest on assumptions that are not necessary for a simple study of structure. Mayhew has additional ideas about science and human nature. Science studies the predictable; indeed, Mayhew's principal criticism against individualism as science is its low predictive power (339, 362-3, 630-3). We cannot accurately know and predict individual behavior, thoughts, or choices with social research. In fact, meaning and choice have no place in sociological analysis. Instead, sociology should concentrate on structure networks, which are easier to predict than individual behavior. But is this conclusion inevitable? Considering the logic of Mayhew's argument suggests a model for the study of social structure rather different from the structuralism he proposes.

Following Marx, Mayhew views society as "the sum of interrelationships in which individuals stand with respect to one another" (338); these interrelationships comprise the structure of society, which is the proper subject of sociology. Unfortunately, each scholar has his own idea of structure and the Americans who engage in structuralist analysis often

contaminate that work with individualist traits (357–58). Actually, Mayhew finds that he cannot present a clear model for structuralism in sociology because there is so little of it in evidence. One book on graph theory, he asserts, presents more social structuralism than all of American sociology (368, note 28). In contrast with structuralism, Mayhew has sketched individualism in sharp detail. Since he defines the unknown structuralism in opposition to the known individualism, he leaves us with a brand of residual structuralism, that is, analysis of social structure that does not share the traits and assumptions of individualism. Furthermore, these traits and assumptions are not even the essentials of individualism, but only its common correlates (335–58). In his total turn away from individualism, Mayhew may have abandoned more than he must and much more than he should.

Mayhew's discussion of Max Weber illustrates this point. Weber is indicted as an individualist, and therefore outside the realm of proper sociology. First Weber defined sociology in terms of the subjective meanings held by individual social actors (365–6, note 6). Yet Weber's research examined ideology in institutions. Mayhew notes this inconsistency, but remains thoroughly anti-Weber throughout.

Clearly, he sees other problems with Weber's work, like the use of ideas common in individualism, but not essential to it: "subjectivism and voluntarism are two of individualism's worst problems, just as they were for Max Weber" (634). For Mayhew, these two traits are the ideological underpinnings of individualism, the sources of an anthropomorphic illusion that has lured sociologists into the study of how people freely choose to shape the social structure (634–40). But are these assumptions really the essence of individualism? Regression analysts both study individual behavior and use this method to explain social structure, but they do their research without assuming subjective meaning or bringing subjective interpretations to their analysis. Nor do they assume free choice by human subjects. Weber did assert that sociological understanding involves interpretation of meanings present in society. But his notion of scientific methodology was far too Kantian to allow him to introduce the idea of freedom into his theoretical frame of reference. Marx, on the other hand, defined society in terms of structure, but studied structures with the hope that they would yield some understanding of the possibility for human freedom within structural limits. Yet Marx seems to be one of the heroes (albeit a possibly contaminated one?) in Mayhew's pantheon.

For Weber, of course, and for other sociologists concerned with human meaning and freedom within a structure, meaning is a property of the group. Certain individuals may or may not subscribe to a given meaning. Individual behavior may or may not imply private agreement with some meaning; it may be the simple result of conformity to a group. And what eventually happens in society may be very different from what people



mean or intend. But human behavior, taken on the average, does tell us what is allowed in a group and this behavior provides one clue to group-held meaning. The symbol systems constructed by a group provide another clue. So we can treat the questions of whether some meaning such as rationality, exists for a group quite separately from the question of whether a particular person pursues means to some goal (635–6).

Mayhew accepts symbol systems or social ideologies as proper subjects for structural analysis (347). But he has not assigned a place for symbolic content. In fact, the specific content of interaction is of no interest whatever (345). Mayhew's real objection to Weber, therefore, is not Weber's individualist definition of social action, but his long-term analysis of Western rationalism (634, 635, note 7). Mayhew quite properly objects to the way some individualists simply assign motive or meaning when they observe behavior. From this he argues that sociology would be much the better if we all assumed that meaning does not exist. This leaves us with symbol systems without symbols, because without meaning symbols stand for nothing.

Mayhew's escape from meaning thus leads him to structuralism rather than the study of social structure. He presents an abstract, mechanistic model of relationships. He defines social structure as a "communication network mapped on some population" (338) and argues that the map should be analyzed as a series of (point/line) graphs (340–5; 368; note 28). We can trace the direction of communication (lines) and not concern ourselves with the qualities of people or groups or symbols or other objects (points). Mayhew's own work on dominance relations in work-group triads shows that structures of dominance communication persist even as individuals change position in the triad (341; Mayhew and Gray).

Clearly such structuralism reveals the mechanistic nature of structure. But does it tell us all we care to know? Does it tell us when and how structures prevail? Mayhew himself has doubts. His own study of artificial groups shows that patterns between individual points prove less stable than the overall structure, at least in transitive structures. In other words, people or groups change their dominance over some other people or groups, even though one point remains most dominant, one least dominant, for the set of points (Mayhew and Gray).

In larger structures (with more points and lines), this instability may prove even more important. If the pattern between points can shift, then the total structure can shift too, as it might in multiparty coalitions. To handle larger structures, we need to know more than a simple definition of the points and the direction of communication along each line. Mayhew would introduce three sets of variables: "(1) individual characteristics of the participants, (2) structural biases toward stability or instability, and (3) a variety of structural or 'setting' variables" (Mayhew and Gray, 187). But Mayhew has also attacked these variables in the work of individualists. For

instance, he clearly states that the only relevant traits a structuralist would use to describe people are age and sex, and sex should be used only in studies of fertility (359).

Mayhew's theoretical work (published nine years after the empirical piece) may present his more recent thought. But since he does not offer other examples of structuralist sociology (uncontaminated by individualism), we are left with the work on triads as the best sample of his type of research. As one applies this model to empirical problems, the need to expand the range of variables becomes apparent. Without describing individuals *per se*, we might still want to know what kinds of people (or groups or symbols or other objects) are involved in a structure. These individual traits are really shorthand for structural relationships in the past. If we find that certain ethnic groups congregate at certain points, we have learned something about social structure. If the various traits used by individualists are completely irrelevant to structure, then we should expect these traits to be randomly distributed among people (or groups of symbol makers) at various points in a series of "communications."

The size of the population is also crucial (334; Mayhew and Gray, 183). As the number of points increases, so does the number of possible patterns between pairs of points. Thus, the range of possible variation depends on the size of the network—a structural property. But what about the likelihood of variation? What about its likely direction? To explain the balance between random possibility and what really occurs, one may need to include meaning and some degree of free choice. For instance, people or groups in a dominance network may form an alliance with others like themselves. What defines this likeness? Are there any defining qualities besides age (359)? An individualist might notice that people from the same religion, race, class, or sex often band together. Someone concerned with social structure might suggest that the patterns of the past have helped to shape some common meaning, so affecting who allies with whom. Of course, that meaning will change over time as new structures change the relationships around religion, race, class, or sex for a given population.

Looking at concrete problems, then, we conclude that a full study of many macrostructures must include some allowance for meaning. At the very least, people or groups recognize some aspects of structure and take these for granted in relationships. Even a biologically based structure, like the communication network of viral disease in a human group (347), creates a meaning structure within the group. History reveals that people expecting the plague relate differently than do those who expect death from old age. Some social patterns exist with a meaning, such as fear of the plague, as well as with the incidence of plague itself. Even Mayhew's example of dominance in a very simple structure implies some meanings held by members of each work triad. Since the triads competed for gain, the research assumed some meanings about gain, goal seeking, coopera-

tion and competition. Mayhew's analysis shows the errors that may follow from a stress on meaning without structure. But neither his arguments nor his example convince us that sociology should be the study of structure without meaning.

It may be just as difficult to rid all studies of social structure from any assumptions about free choice. Structures often contradict each other. Within some group, religious norms may encourage large families, while fiscal interests dictate small ones. In the past century, family size has changed all over the world, but at different times and at a pace that cannot be predicted as a mathematical balance between the opposed forces for different family size. After all, it takes only nine months to reverse a birth trend. In studies of structure, one may ignore the forces impinging on each individual family, but we do concern ourselves with the total population size (338–9) that results from the private choices made in individual families. Someone may examine the limits imposed by structure without listing the correlates of large and small size for individual families. For many studies, the correlates and free choices that remain after these correlates are counted may both be treated as random noise. But when people study contradictory structures or changing ones, they should be aware that groups may shift position in the structure for reasons of their own, for reasons we may call free choice, since we will never number all the possible causes. The total effect of these shifts is structural instability (Mayhew and Gray).

These comments presuppose that the study of social structure is more complex than the structuralism Mayhew proposes. His structuralism maps one communication network at a time; most structures contain many such networks. In macrosociological studies, one may not be able to map each network, tracing lines of communication from point to point. It may be necessary to find trends as networks that intersect, or grow over time. Morris Janowitz's (a, b) work on the growth of the welfare state provides one example. The welfare state structure depends on strong economic growth, surplus, and the need to include lower-class people in parliamentary decisions and military service. Because the state needs lower-class people, it uses its surplus wealth to provide them with free schools, free clinics, and other goods. In time, people come to expect the goods and share a common understanding about state obligations and private rights. As more and more people choose to demand these rights, this demand expands both upward among less needy groups and outward to include more goods. This structure thus evokes common meaning and reflects private choice. The networks of communication here are far too complex for a point/line graph, but surely the problem is a structural issue that demands urgent attention in American society.

In sum, Mayhew has warned about how individualism distracts us from a proper study of social structure. He has further cautioned us that assumptions about subjective meaning and free choice often associated

with individualism lead to tautological reasoning, low predictability, and other theoretical *cults-de-sac* (350–7). But the structuralism Mayhew offers as an alternative leads instead to an overly simple description of a very abstract social world. We suggest that students of social structures build a model for study based on the nature of structure, not on the perceived sins of individualism. This model of macro-level structures would have to include some place for common meanings and the possibility of free choice among some people and groups within the structure.

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# The Dual Economy and Labor Market Segmentation: A Comment on Lord and Falk

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A recent paper by George Lord III and William Falk, "An Exploratory Analysis of Individualist Versus Structuralist Explanations of Income," presents methodological problems that may be intrinsic to the testing of hypotheses derived from neo-Marxian analytical categories. My objections pertain as well to several other papers on the relationship between individual earnings and American industrial segmentation (e.g., Beck et al.).

As Lord and Falk correctly suggest, American Marxists codified a fundamental premise of *Capital*, the logical tendency toward capital concentration in industry, and developed a structural representation of advanced capitalism. The resulting so-called dual economy is characterized by a monopoly sector of large, capital-intensive oligopolies; a competitive sector of smaller, more labor-intensive competing firms; and the state (Baran and Sweezy; O'Connor). With others, Lord and Falk have hypothesized that monopoly sector employment is superior in wages, benefits, and opportunities for advancement to jobs in the competitive sector, thus joining together a neo-Marxian dual economy model with a theory of labor market segmentation (see also Beck et al.; Tolbert et al.). Lord and Falk can then use NORC data on individual earnings to (1) affirm, seemingly, the existence of a two-sector economy, and (2) apparently demonstrate that structure (economic sector and class position) explains some variation in individual earnings, although less than a group of human capital variables, education, job experience, etc.

The comparatively weak effect of structural variables on income was unexpected by the authors, given others' findings (Beck et al.; Bibb and Form). Among possible reasons for this result is an underlying conceptual inadequacy. I will also argue, however, that current data and sociologists' methodological preferences preclude the determination of full structural influences on earnings.

research (e.g., Beck et al.). Public data-collecting agencies assume intra-industry uniformity; their data thus obfuscate such intra-industry differentiations as degree of capital concentration. Marxian scholars have undertaken little cross-industry research on capital concentration precisely because available measures are methodologically inappropriate. A factor analysis like that of Tolbert et al. improves only marginally on its source when a single factor, e.g., employee earnings, is at issue, as in the Lord and Falk paper. Moreover, capital concentration, fundamental to the dual economy conceptualization, is measured only *indirectly* by means of available data like employee assets, receipts, advertising budget, etc. (Tolbert et al.).<sup>2</sup>

### Placing State Employees and Women Workers

Two specific criticisms of Lord and Falk's research derive from their use of the Tolbert et al. index. First, the authors assume that job markets are similar for monopoly and state sectors—higher paying, tolerant of unionization, etc. But neo-Marxian literature suggests that the causes of state and monopoly sector firm largess are quite different, and that the state's is necessarily limited (O'Connor). A crude measure of state employee position, relative earnings of employees in government and industry, indicates that government and government enterprise employees earn more annually than those in only four general industrial categories—wholesale and retail trade, finance, etc., service, and agriculture (U.S. Bureau of Census, 425). State fiscal crisis has resulted in the "ghettoization" of some jobs, e.g., hospital services, low-ranking military, and their disproportionate allocation to women, minorities and young people. Indeed, several industrial categories likely to be in the state sector, to manifest secondary labor market characteristics, and to be filled disproportionately by nonwhite, female and youthful employees—Hospital, convalescent institutions, Educational services, Museum and other non-profit firms—do show up as periphery (competitive) sector firms in the Tolbert et al. index. Lord and Falk's seeming misplacement of many state employees has minimized the impact of industrial sector on earnings. But a portion of the secondary labor market that Braverman predicted would result from the "deskilling" and "degrading" of monopoly sector jobs may also be misplaced.

A variety of data suggest that the authors have also underestimated the impact of sector placement on women's earnings, again, because of their reliance on the Tolbert et al. index to classify firms as monopolistic or competitive. Women's employment—low-paying, low in benefits, generally non-unionized, and low in skill levels—cuts across traditional industrial categories in much the same way as do other minority jobs. For example, women are more likely to hold clerical positions than any other; in

1979 more than a third of full-time women workers were in the category, clerical (U.S. Department of Labor, b). There is, of course, no government industrial classification for clerical workers, nor have Tolbert et al. factored in the number of clerical workers in an industry in computing their core/periphery (monopoly/competitive sector) index. However, about 1/3 of clerical workers are in firms with more than 2,500 employees (U.S. Department of Labor, b), and most are concentrated in service, wholesale and retail trade, finance, etc. (see Table 1). Tolbert et al. place several service industries in the core (monopoly) sector; all retail trade in the periphery (competitive); most finance in core (monopoly). Clerical employment is about 82 percent female (U.S. Department of Labor, Table 16),<sup>3</sup> and lower paying for female than male workers (see Table 2). Indeed, many industrial categories with high concentrations of clerical workers, e.g., finance, are surprisingly low in earnings (see Table 3). It appears that many women clerical workers occupy a secondary labor market position within monopoly sector and state firms. The same may be true of female employees in custodial and food services. Thus, a structural characteristic—employment in the core (monopoly) sector—explains little about low female earnings in some occupations. Again, it is possible that monopoly sector clerical workers are experiencing the work degradation hypothesized by some neo-Marxian theorists of economic segmentation.

## Conclusions

It is significant in the annals of Marxism that American scholars have been eager to translate Marx's insights into ahistorical structures and models that can be evaluated empirically. Sociologists have the technical skills to extend this tendency, and in the process, may violate the methodological and theoretical exigencies of Marxism. That is, there are American academic Marxists who now insist that historical processes must be understood in their full complexity (Ollman). Capital concentration is such a process, having taken many forms historically and by industry. The dichotomization of the economy and of labor markets may thus be an intrinsically frustrating endeavor—always yielding results that are somewhat deceptive and obsolete. Some Marxists propose analysis of smaller units, e.g., an industry or group of industries, rather than the economy as a whole (Aronowitz; see, e.g., Edwards). The results may be more meaningful for both our understanding of American political economy and stratification and for the advance of research on these topics.

Table 1. EMPLOYED PERSONS BY INDUSTRY AND OCCUPATION, OCTOBER 1980 (IN THOUSANDS)

Industry	White Collar Workers					Blue Collar Workers				Service Workers			
	Managers, Profes- sional, Technical					Oper- atives, Except Trans- port				Non-farm Laborers			
	Total Employed	Managers, Adminis- trators, Except Farm	Sales Workers	Clerical Workers	Craft and Kindred Workers	Oper- atives, Except Trans- port	Transport Equip- ment Oper- atives	Non-farm Laborers	Private House- holders	Other Service Workers	Farm Workers		
Agriculture	3,501	71	39	5	77	46	24	293	--	15	2,873		
Mining	931	133	63	8	144	217	290	29	--	11	--		
Construction	6,205	187	751	26	461	3,496	281	768	--	42	--		
Manufacturing	21,383	2,428	1,663	526	2,733	4,090	7,854	944	--	406	--		
Durable goods	13,929	1,709	964	220	1,599	2,717	4,602	576	--	210	--		
Nondurable goods	8,454	719	699	316	1,133	1,373	3,252	368	--	196	--		
Transportation and public utilities	6,420	573	685	73	1,492	1,370	1,484	430	--	156	--		
Wholesale and retail trade	19,892	425	3,840	4,141	3,515	1,490	991	1,198	--	3,639	--		
Wholesale trade	3,951	168	824	924	816	381	213	240	--	30	--		
Retail trade	15,941	256	3,016	3,217	2,699	1,109	776	958	--	3,609	--		
Finance, insurance and real estate	5,896	350	1,097	1,308	2,650	131	11	72	--	260	--		
Service	28,648	10,694	2,161	203	5,264	1,510	758	487	1,063	6,253	--		
Private households	1,246	23	2	--	14	--	--	108	1,063	34	--		
Other service industries	27,402	10,671	2,159	203	5,250	1,510	758	379	--	6,219	--		
Public adminis- tration	5,056	1,030	711	5	1,787	245	57	138	--	1,045	--		

Source: U.S. Department of Labor (a, 36).



**Table 2.** MEDIAN USUAL WEEKLY EARNINGS OF FULL TIME WAGE AND SALARY WORKERS, 16 YEARS OF AGE AND OVER, BY SEX AND OCCUPATIONAL GROUP SECOND QUARTER 1979 (PRELIMINARY)

Occupation Group	Women	Men
Total	\$183	\$295
Professional and technical workers	261	375
Managers and administrators, except farm	232	386
Sales workers	154	297
Clerical workers	180	287
Craft and kindred workers	189	305
Operatives, except transport	156	257
Transport equipment operatives	194	277
Nonfarm laborers	166	220
Service workers	138	203
Farm workers	125	153

Source: U.S. Department of Labor (c, Table 13).

**Table 3.** HOURLY AND WEEKLY EARNINGS IN CURRENT DOLLARS BY INDUSTRY GROUP, JANUARY TO MAY 1979

Industry Group	Gross Hourly Earnings	Gross Weekly Earnings
All industries	\$6.02	\$213
Manufacturing	6.54	262
Mining	8.32	355
Contract construction	9.02	324
Transport, public utilities	7.90	313
Wholesale trade	6.25	242
Retail trade	4.47	135
Finance, insurance, real estate	5.19	188
Services	5.27	171

Source: U.S. Bureau of Census (420).

## Notes

1. Marxists have also criticized Braverman's work ". what lies behind much of the thesis of degradation is an anterior standard of craft, combined with a tendency toward overgeneralization that prevents investigators from making a concrete, historical study of the differences among industries, of the definition of skills and of the mediation of cultures" (Aronowitz, 144)

2. Non-Marxists too have suggested segmented labor markets. Many government sponsored

job training programs of the 1960s and 1970s were based on an assumed linear relationship between increased skill and training requirements and income within the traditional occupational hierarchy displayed in Table 3. Critics have since argued that training fails to enhance worker mobility from lower-paying labor market segments, because higher-paying market segments are impermeable (Sassen-Koob)

3. In a recent survey of 21 occupations in firms with more than 50 employees, women constituted 98 percent of clerical workers (U S. Department of Labor, c, 3)

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# Dual Economy, Dual Labor, and Dogmatic Marxism: Reply to Morrissey\*

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In commenting on our article, Morrissey raises two objections. First, she points to an "underlying conceptual inadequacy" in our work, and second, to a problem inherent in the particular method we chose to investigate a set of theoretical questions. We will address both of these issues and, in addition, provide our own critique of the previous work on the basis of rethinking some of the issues which we originally raised.

## Conceptual Inadequacy or Misunderstanding?

Morrissey's comment deals primarily with an alleged "conceptual inadequacy," a criticism which reveals a misunderstanding on her part. Her argument stems from the necessary distinction between what we refer to elsewhere (b) as "dual economy versus dual labor market." Morrissey incorrectly states that we have confused the issue of segmentation by assuming "that monopoly and competitive sector industries have, respectively, primary and secondary labor markets," an error she reiterates. She then sets out to correct this alleged transgression by providing us with her interpretation of the current debate among the neo-Marxists on the structure of the secondary labor market.

Unfortunately Morrissey makes the same errors in interpreting these theories as she did with our work. Morrissey suggests that the neo-Marxist debate is that found between Braverman, with his "deskilling" thesis, and the work of Baran and Sweezy, with their thesis of "monopoly capital." She then interjects Sassen-Koob's conceptualization of the labor market and declares it a third conceptualization. We see this "debate" as a creation of Morrissey's rather than the authors she cites, for these theories are not competing to the degree which she asserts. The "deskilling thesis" and the "monopoly capital thesis" are complementary rather than antithetical as

\*The authors wish to acknowledge the helpful assistance of Troy Harris and Lori Olivier in the preparation of this statement.

Morrissey contends. In his foreword to Braverman's work, Sweezy readily states that their work "was not calculated to give a complete picture of the form of society under study" (ix). He further refers to Braverman's work as "a serious, and . . . solidly successful, effort to fill a large part of (the) gap" left in the work done by him and Baran.

O'Connor has shown us that the development of monopoly capital is not as all-pervasive and uniform as Baran and Sweezy have suggested. O'Connor describes sectors of the economy (competitive, monopoly, and state) based on *industry* specific differences. There has been a greater concentration and centralization of capital in those industries which are capital intensive, and these industries are oligopolistic as compared to those industries which are labor intensive and organized in a competitive fashion. We refer to this as a "dual economy" and it is this framework within which we place our own work as well as that of Beck et al.; Bibb and Form; Bluestone; Hodson; O'Connor; Oster, Tolbert, et al.

There are many implications of such an economic structure, not the least of which is the possible impact on the creation of segmentation of occupations or the creation of several labor markets. The existence of segmentation in the labor market, that is segmentation along occupational rather than industrial lines, is not a new idea, and it probably existed prior to the development of monopoly capitalism to any appreciable degree. It is first discussed as far back as 1874 by an orthodox economist, J. E. Cairnes, whose ideas were further developed by Kerr in 1954. These ideas have found renewed support among contemporary political economists, particularly those referred to as "institutionalists." Piore, Doeringer, Harrison, and even neo-Marxists such as Gordon, Edwards, and Reich have used this conceptualization of a "dual labor market." While there are obvious similarities between the dual economy and the dual labor market theories, they are neither synonymous nor antithetical. Quite simply, occupational sectors and industrial sectors exist apart from each other yet they are quite obviously related in that they are both part of the U.S. economy which we are investigating. We have illustrated this relationship elsewhere (Lord and Falk, b) as shown below:

Occupational Sectors	Industrial Sectors	
Secondary Primary	Competitive	Monopoly

Morrissey's criticism is then extended to the Tolbert index of level of oligopoly in each industrial category the measure we employed in operationalizing the dual economy. In general Morrissey's dislike of the Tolbert index is based on its weakness since it is constructed from *indirect* measures. We too would prefer a direct measure of oligopoly and if Morrissey

or any one else can develop one we will be happy to use it. For now we stand by our previous statement that the Tolbert index is the "most thorough analysis of factors influencing the relative level of oligopoly in the various industry groups."

Her critique next turns to two points. First, she takes issue with our placement of the state industries in the monopoly sector and interestingly, she sites O'Connor to support this criticism. We would agree that the state and monopoly sectors are quite different in their historical development; but one could not have developed to its present state without the other.

The first basic thesis presented here is that the growth of the state sector and state spending is functioning increasingly as the basis of the growth of the monopoly sector and total production. Conversely, it is argued that the growth of state spending and state programs is the result of the growth of the monopoly industries. In other words, the growth of the state is both a cause and effect of the expansion of monopoly capital (O'Connor, 7-8).

The point we made earlier was that "since the state sector and the monopoly sector function the same (*with regard to our dependent variable, income*) we treat these industries as an integral part of the monopoly sector in the present paper" (a, 379, emphasis added). We believe that the work of O'Connor, rather than showing us in error, gives added support to this position. O'Connor, when referring to wages, states: ". . . state sector rates tend to be tied to monopoly sector productivity . . . (and) there is a general tendency for state sector wages to be driven up to the level prevailing in the monopoly sector"(30). It seems that Morrissey is not only guilty of misreading our work but also that of those she calls on to bolster her argument against us.

The one point on which we agree with Morrissey is that the placement of several industries in the competitive sector rather than in the monopoly sector may have reduced the impact of sector on earnings. The Tolbert index places several industry categories in the competitive sector which may rightly belong in the monopoly sector—these include hospitals, convalescent institutions, educational services, museums, and other non-profit firms. These industries fall exactly on the dividing line for the Tolbert index and additional data may properly place them in the monopoly sector rather than the competitive. This is an empirical question still open to research.

### The Inherent Methodological Problem

Our second major pitfall, according to Morrissey, is the use of current methods and data which "preclude the determination of full structural influences on earnings" and which generate "ahistorical structures and

models that can be evaluated empirically." These charges are leveled but not discussed by Morrissey. While we would agree that our models and methods cannot explain all of reality, we disagree with the stance taken by Morrissey and other neo-Marxists that these methods are inherently bourgeois, employed in the service of a continuing apology of the current structure. (In a similar vein, see the exchange between Gorelick, and Bowles and Gintis.) We suggest instead that the methods can and are being used to begin to ask and answer questions which arise from a Marxian approach to social reality.

Interestingly some of the best recent neo-Marxist works have been those which have relied heavily on the results of "Bourgeois" scientists. Bowles and Gintis in education, Baran and Sweezy in economics, and recent works by Wright, and Wright and Perronne on the class structure in the U.S. have all turned to such "ahistorical" studies. We would agree with the statement of Baran and Sweezy in their introduction to *Monopoly Capital*:

... the dictum of Hegel which we have chosen as an epigraph for this book retains its undiminished validity: The truth is the whole. To be sure, there are also an infinitude of small truths which American social scientists have been diligent, and often successful, in pursuing. Having borrowed liberally from their findings ... we would be the last to belittle them. But just as the whole is more than the sum of the parts, so the amassing of small truths about the various parts and aspects of society can never yield the big truths about the social order itself (2-3)

In short, we agree that the method we used has its weaknesses, as does any method. We take the position that *no single method can be used to totally explain social reality*. Each method available to us as social scientists should and must be put to use as appropriate. The important issue remains: what questions do we choose to ask about social reality? (See Stolzman and Gamberg on this.) Failing to confront this issue, we have the methodological tail wagging the theoretical dog.

### Return to the Question of Structure

The major issue of our article, as we saw it, was the importance of understanding structural influences in the stratification process as opposed to the individualist influences found in status attainment research. Yet we ourselves fell victim to the orthodox representation of education as an individually attained form of human capital rather than seeing education as a structural phenomenon. We feel this was the single greatest error in our previous work and one which we need to reexamine here.

Our position is much like that of Rosenbaum's about the structure of opportunity. Rosenbaum is one of several revisionist writers to discuss the myth of equal opportunity as it is found in education. Students do not

begin with a clean slate at the start of each school year; rather, their successes and failures have a cumulative quality to them which becomes irreversible over time, especially once they enter high school and the effects of tracking cap the process. Success in school, then, has a structural component, it is not merely the result of the individual's strivings for grades and recognition by teachers. It is not merely an individual accomplishment parlayed into economic success. Rosenbaum characterizes the educational process as a tournament in contrast to the more customary contest metaphor. His reasoning is that "when you win, you win only the right to go on to the next round; when you lose, you lose forever." In other words, the opportunity for advancement is, itself, a structural phenomenon and the probabilities for success or failure are associated with one's origins (also a structural consideration).

Although not without its critics (especially Ravitch), this tendency for school structure to mirror economic structure has been called the "correspondence principle" (Bowles and Ginitis). In this way the stratification system is produced anew each generation with the assistance of the schools. The opportunity realized or denied in school is extended into the labor force on the completion of schooling. Again, it is a structural phenomenon. As to the relationship between schooling and economic success (or in a more sociological vein, schooling and social mobility), Spring's observation seems salient: "Education is no longer a means of getting ahead; but is a means for staying in the same place" (78). Class position is simply reaffirmed through the schooling process. Clearly, education can be conceptualized in other than human capital terms.

## Conclusions

In this brief rebuttal of Morrissey's comment, we have tried to show that we have not been guilty of "conceptual inadequacy" but to the contrary, Morrissey has been guilty of a misreading and misunderstanding, not only of our work but the work of other segmentation researchers as well; that our choice of methods was not made in ignorance of epistemological considerations (while fully agreeing that our position or any position is not without its shortcomings); and that education could be conceptualized in more structural terms as opposed to the individualistic operationalization we gave it in our article. As a final note, we would encourage Morrissey and all social scientists to be mindful of Feyerabend's "plea for tolerance in matters epistemological." Truth is hardly one-dimensional. How pristine one's view of Marxism is may determine whether one is willing to labor on empirical research with Marxian underpinnings. For us and others making this choice, we hope that this will not entail excommunication from some religion of which we were not previously aware. Marxism has become an

objective set of dogmatic and unalterable categories and classifications which no longer fit the reality it is attempting to analyze. It has been described as "an empty shell held together by dogmatic slogans" (Piccone, 3). While the theories and work of Marx are of indisputable value to the social sciences today, we can only take the same stance today Marx did nearly a century ago and proclaim "what is certain is that I am no Marxist" (McLellan, 443).

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# Salvation and Survival on Skid Row: A Comment on Rooney

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The article by James F. Rooney (b) last year on skid row "rescue missions" makes an important contribution on a little-known subject in a major journal of our discipline. While it is not the first article on the subject in a prominent social science journal, it broadens the theoretical context suggested in earlier work by extending the distinctions between formal and informal goals (Blau), or official and operative goals (Perrow), to cover a logical extremity of such classical ideas in organization theory: namely, the organization that succeeds by failing.<sup>1</sup> The skid row rescue mission, as Rooney demonstrates, is a clear example of this phenomenon, but by no means the only one. One cannot fault him for his theoretical interpretation, or for any of the *data* offered in the lengthy ethnographic description leading up to the theoretical discussion and justification in the final third of the article. I do respectfully suggest, however, that Rooney has needlessly encumbered his article, particularly the descriptive portion of it, with an underlying theme of power inequity and superior-inferior relationships. The effect, I think, is to exaggerate greatly the perniciousness of the hapless mission leaders and preachers, while underestimating both the resourcefulness and the guile of many skid row denizens.<sup>2</sup>

Much of this unintentional distortion derives, I think, from a lack of reference in the article to any natural comparison groups in the description of the missions and their typical *modus operandi*. By natural comparison groups here I mean other *religious* enterprises (including regular churches) and other *agencies dealing with a skid row clientele*. The comparison Rooney *does* make, at the end of the article, between missions and other kinds of organizations that succeed through failure, is appropriate enough for *theoretical* purposes; but it has the effect also of lifting the missions out of their *natural* contexts to a level of abstraction that reveals only part of the existential reality. The characterizations offered under these circumstances, about what the men have too little of, or about what the missions do too much of, leaves the reader to wonder about the implied but unanswered question, "Compared to what?"

Where the mission as *religious* organization is concerned, for exam-

ple, we are told how the mission preachers and administrators harangue the men with sermons that are moralistic, judgmental, and probably irrelevant (909–10); that they tend to take an “institutional stance” behind the pulpit that contrasts somewhat with their private or informal personas (908–9); and that they are given to attitudes or posturing in manner and dress that bespeak a certain sanctimoniousness (911–2). The men in the audience, for their part, react with indifference, at best, and more often with cynicism (913), bitterness and resentment (912). Not surprisingly, there is “a very low conversion rate” (909). The reader may become so incensed at this scenario that he may overlook, as Rooney apparently does, that *churches in general* constantly have to contend with sanctimoniousness, bureaucratic expediency, and unintentional misanthropy on the part of the clergy, as well as with the cynicism, occasional hostility, and frequent opportunism on the parts of many who attend religious services. I don’t mean to gloss over the obvious differences between a skid row mission and a neighborhood church, but neither should we forget the similarities. Even the brand of theology underlying the sermons at the rescue missions is not inappropriate, considering the religious and social backgrounds of the skid row parishioners, as Rooney acknowledges (909), though he seems offended (I think inappropriately) by the emphasis on individual responsibility and free will in the theological premises of evangelical Protestantism (908–9). The common use of this emphasis in many rehabilitation programs (not just in rescue missions), as part of an ideology of *recovery*, does not necessarily imply related moralistic assumptions about the *etiology* of either poverty or alcoholism. In other words, skid row preachers know, as Rooney himself seems to recognize (908–9), that many factors beyond individual volition contribute importantly to alcoholism and poverty; but none may be as important as volition itself in *recovery*. Furthermore, the emphasis on the weaknesses of the flesh in evangelical and fundamentalist preaching (e.g. “irresponsible self-indulgence,” 909) is by no means applied only to the poor and the drunk; it is a classical Pauline indictment and warning about human nature more generally.

Whatever may be the brand of religion preached in the mission services, one can look on the mission also as a self-proclaimed *recovery or rehabilitation organization* (905). As far as this function is concerned, Rooney seems to see the “success through failure” of the missions as somehow expressing a structural, economic imperative to furnish a perpetual supply of cheap and docile labor, and to hide from government statisticians a significant increment of unemployment or underemployment (915, 918, 919). From this perspective, the mission staffs are seen, wittingly or not (921), as agents of exploitation for an unfeeling economic system, a perspective that greatly exaggerates both their power and their perversity. Of course, there is considerable variety in the attitudes and motives of mission personnel, as there is among skid row men themselves. However, our data

(Bibby and Mauss) make it clear that the great majority of those who "answer the call" to missionary service on skid row are themselves conscientious but hapless drop-outs or wash-outs from the mainstream of clerical vocations. The skid row mission, in other words, is a place where the skidders of the clergy meet the skidders of the rest of the world. There is a third party, moreover, that is centrally involved in the work of the mission, and which Rooney almost totally overlooks: this is what might be called the uptown supporters, who are important both for their financial support and for their moral support in the form of regular visits and participation in the mission services and activities. All three of these parties, the men, the mission leaders, and the uptown supporters, have their own needs and interests, which converge in the transactions and exchanges that take place in the rescue mission.<sup>3</sup> It is this very *success* in the attainment of those respective tripartite goals, of course, which undermines and leads to the *failure* in formal organizational goals, as Rooney explains. I submit, however, that it is neither necessary nor useful to go beyond such a parsimonious explanation and add, as Rooney does, the abstractions and rhetoric of "the positive functions of poverty" (915). Even at a more micro-cosmic level, I would argue, the transactions among the various parties are better understood in terms of *symbiosis* than in Rooney's "terms of mutual cross-purposes" (921).

Having apparently lost sight of a comparative perspective (rescue missions compared with other rehabilitative agencies on skid row) Rooney leaves the reader with an exaggerated picture of the impact of the mission's ministrations on the self-concepts of the skid row men.<sup>4</sup> In one respect, he even compares the missions to total institutions in the "degrading and humiliating" outcomes of their efforts, and he characterizes the evening gospel service as a "degradation ceremony" or a "devestment ceremony" (911). Such pernicious outcomes are apparently a natural consequence of assumptions on the parts of mission leaders that skid row men have gotten where they are mainly through "excessive drinking and lack of ambition," and that their shortcomings are "a clear manifestation of sin" (908). Accordingly, even though many of the men are not alcoholics, and presumably most of them are in their present predicament for "structural" reasons over which they had no control, the mission preachers nevertheless demand total abstinence from alcohol and urge their audiences to seek "regular and continuous employment," as well as to accept Christ (907-9).

However insensitive and irrelevant such ministrations may seem on their face, they are not really terribly different from what other rehabilitative programs feature, especially those for the skid row clientele. Given the prevalence (though not universality) of alcohol abuse in that clientele, the emphasis on abstinence in mission services does not seem totally inappropriate. Abstinence is characteristically demanded by all government-supported programs for "public inebriates" (as skid row people are fre-

quently called in such programs). It is the watchword also of such highly respected programs as Alcoholics Anonymous; and the A.A. would not, I think, find itself arguing very strenuously with the missions' perhaps exaggerated assumption that the dependency seen on skid row is attributable mainly to "irresponsible self-indulgence, most frequently involving excessive drinking" (909). Also in A.A. can be seen an ideology which, like the missions', emphasizes such notions as hitting bottom and admitting one's degraded condition, falling back on the redeeming strength of a "higher power," asserting one's God-given free will to turn one's life around (starting with abstinence), and the like. Such strictures not only permeate the Twelve Steps of A.A. but can be found, in somewhat more secularized rhetoric, to be sure, in a great many other highly touted recovery or rehabilitation programs (Ward). I have even seen the well-known Synanon game adapted by treatment programs for "public inebriates" and others with results easily as punishing to the self-concept as any mission's ear-beating service. Indeed, the emphasis which Rooney places on the degradation ceremony in mission services tends to exaggerate greatly the part played by the mission in "formally symboliz(ing) loss of status in a secular society dedicated to economic success through individual achievement" (911). If the mission service were somehow the rite of passage or otherwise the genesis for the skid row way of life, then it might be more deserving of the opprobrium which Rooney attaches to it. In reality, however, the symbols of lost status permeate that way of life well outside the doors of the mission, not only through treatment at the hands of police, storekeepers, bloodbanks, and each other, but, indeed, at the hands of many public welfare agencies and social workers operating well-intentioned, and scarcely religious, government programs for skid row indigents. Every such rehabilitation program, furthermore, could fairly be characterized as being in the business of conversion, for, like the missions, they too seek to "change the values of deviant individuals by converting them," and to "restore them to economically useful positions" (914). Also like the missions, these secular treatment and rehabilitation agencies rarely get *bona fide* converts.

This brings me to the final point of this critique: It is true enough, as Rooney says, that many of the skid row men react to missions and their ministrations with cynicism, resentment, and hostility (912-913). (Our data [Wolfe] indicate actually that simple indifference is probably the most common feeling.) Be all that as it may, Rooney surely knows, as we have learned, that a posture of contempt, cynicism, or manipulative defense, is understandably the typical skid row response to nearly every new program that comes along to rehabilitate them. As they do not authentically accept and act out the role of either convert or seeker in their transactions with missions, neither do they truly accept the sick role when they enter treatment programs (Fagan and Mauss), or (typically) the trainee role in job rehabilitation programs. Of course, we all need to keep in mind the caveat

that "the skid row way of life" means somewhat different things to its various adherents. However, our data indicate (Wolfe) that by no means do all skid row men "define their dependency as caused principally by structural forces" (909). A large minority, furthermore, would not voluntarily leave their way of life, even if they could get a new start or stake. Many others actually express a positive self-concept and considerable satisfaction with the way they are living (Wolfe). As Rooney (a) has so ably explained elsewhere, skid row men for the past 20 years or so (unlike an earlier, more activist period) have come to be comprised mainly of those who "passively seek to avoid the demands of society and have withdrawn from all major responsibilities" (a, 33). Skid row is now a "refuge for drop-outs from the working class who have psychic disabilities, a significant proportion of which involve alcoholism" (a, 34). From this refuge, many of the men circulate in and out, evoking the extremely appropriate image of the "open asylum," also suggested by Rooney (a, 34).

Survival in this open asylum requires considerable resourcefulness and calculation in providing for one's minimal needs for food, clothing, shelter, and recreation (especially booze). The rescue mission must be understood as simply one segment of a skid row survival cycle, and probably not a terribly important segment, except for the few who accept a term as "mission stiff" or "cold-weather Christian" (913). Other elements in the cycle include occasional work (907); panhandling; regular pensions (907); public assistance or welfare (which our data [Wolfe] indicate may help support as many as a third of the skid row indigents); municipal shelters and drop-in centers; and petty theft.<sup>5</sup> Even the missions, quite *apart from* the food or shelter made contingent on attendance at mission services, frequently provide, free for the asking, used clothing, meals (or meal tickets redeemable elsewhere), and some beds on a first-come basis up to a limit of two or three nights per man per month. To be sure, if a man can get what he needs from these various other sources without enduring the earbeatings and condescension of mission services, he will do so; and, as it turns out (907, 914), the latter are actually not a very frequent resort in the overall cycle (see also Miller; Wolfe). When such resort is had, it may be deliberately included in the survival cycle as a period of time out or R and R from the battle of the bottle, during which a man can recover a measure of health and stamina before returning again to the streets (907-8).<sup>6</sup> The men have learned, too, that the quality of food and lodging varies considerably from one mission to another, and their preferences get built into the survival plan. It was well known in Seattle that most of the missions changed bedsheets only once a week, and the seasoned regulars knew which mission to visit which nights in order to ensure clean sheets every time.

Let me not be misunderstood: no one approves of misery or indignity for the indigent, no matter how or why they got that way. Nor am I suggesting that life on skid row is actually rather rosy and romantic. It is at

best Dickensian and at worst Hobbesian. But one oversimplifies and overdramatizes the plight of the skid row man to see him as the pitiful pawn of pernicious preachers; and one greatly underestimates his survival skills and resourcefulness to see him as helplessly manacled on a miserable merry-go-round of missions. Nor is it fair to the missions and their staffs to see them as some kind of insensitive skid row outreach program of the Moral Majority.

## Notes

- 1 The scientific literature on skid row missions is small. As Rooney's references indicate, there is Wiseman's well-known book on the rehabilitative (but not religious) aspects of the Salvation Army, and the more recent dissertation by Hinnchs (see also Fedewa). The only previously published article on the subject in a scientific journal (Bibby and Mauss) was somehow overlooked by Rooney, a puzzling omission considering the apparent commonality of some of the theoretical derivations (Blau, Perrow) in the earlier and the present article. Rooney is one of the most fully informed and seasoned scholars on the skid row phenomenon today, and one ought not take issue with him frivolously. The comments and criticisms to follow here come out of several years' work of my own, with collaborating graduate students, on Seattle's "skid road" during the 1970s (see, e.g., Bibby and Mauss, Fagan and Mauss, Mauss and Fagan, Miller, Wolfe). Besides the many hours, collectively, of observation in rescue missions and skid row streets more generally, our data included mission records and newsletters, interviews with mission leaders and uptown supporters, and more than 100 systematic (but informal) interviews with skid row men, conducted mostly by a member of our team who easily passed as a skid row man himself, and who, like Rooney, rode a lot of rails, slept in a lot of missions, and hung around on a lot of streets (Miller, Wolfe). Considering the variety of data that all of us generated in our various ways over the years, and the sustained focus on one city, our Seattle data are probably more intensive and extensive than Rooney's on any one city, but the breadth of his data and experience from various cities are clearly greater than ours.
- 2 There is a well-known danger in ethnographic research (and in social work), to which Rooney may partially have succumbed, of identifying too closely with the natives or clients and with their accounts or definitions of reality.
- 3 This level of analysis is explored in detail in Bibby and Mauss, where there is also a suggestion for applying to each of these three parties Glock's various types of deprivation leading to religious involvement (429).
- 4 One of our more interesting findings about the self-concepts of skid row men was that about a third of them had quite strong or positive self-concepts, but that strength of self-concept was *inversely* related to a measure of subjective rehabilitation potential (Wolfe).
- 5 The growing influx of younger men into skid row during the past decade from various sources has added a new "hustle" to the survival cycle: drug dealing.
- 6 In some cities, a much more attractive alternative to the missions for the R and R respite has appeared: municipal detoxification centers. In Seattle, as in many cities for the past few years, public drunkenness has been decriminalized, so that "public inebriates" can now have regular access to the good food, hygiene, and clean sheets of the detox center for several days per month at public expense. While each stay at the detox center is supposed to culminate in enrollment in an alcohol treatment program (and, one assumes, eventual conversion to a better way of life), skid row men are actually circulating in and out of detox centers at a recidivism rate four times that which earlier occurred in and out of the drunk tank (Fagan and Mauss). In view of this, one can only endorse Rooney's concluding query (922) as to whether organizations that "benefit directly from their failure" are worthy of continued support (922). Keeping in mind, however, the comparative perspective that we have here urged on our readers, I would start by looking first at the expensive tax-supported rehabilitation programs for public inebriates and indigents before I would focus on the (mostly) privately supported rescue missions!

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# Reply to Mauss

JAMES F. ROONEY, *Catholic University of America*

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Professor Mauss has raised a number of issues regarding the validity of the interpretation of the field data in my participant observation study of rescue missions in skid row areas of the United States. The majority of the issues raised are important points for consideration, particularly regarding appropriate comparison contexts and the interpretation of the themes of power and inequality.

I am not completely without a comparative framework in my focus on the rescue missions. I have worked with alcoholics, principally from skid row, in the Philadelphia House of Correction, and noted how dependent the institution was on the experienced labor of 50 to 100 regulars who, although sentenced from 30 to 90 days at a time, spent the greater part of the year incarcerated. The precise concept of "success through failure" jelled in my mind, however, when I was employed in a rehabilitation and relocation program for skid row men prior to urban renewal of the area. The directors bungled the operation thoroughly, and although operating at 300 to 400 percent over the original budget, were bailed out by the city's redevelopment authority for the lack of an alternative. Their ignorance, lack of planning, and mismanagement resulted in organizational growth and success. The concept was born, then, while working in a relocation program, not while observing in a mission.

My major criticism of Professor Mauss' critique is his failure to take into account the fact that all interaction of skid row men and mission personnel takes place within the context of an invidious stratification system. Skid row men are regarded as complete failures, unwilling even to assume the minimal responsibilities of supporting themselves. As such, they are considered the most degraded of deviants and are perceived as a group outside the limits of the legitimate social order. All analyses of the relationship of skid row men and representatives of any outside organization must take cognizance of the perceived outcast status.

Mauss validly points out that all churches must contend with sanctimoniousness, bureaucratic expediency, and misanthropy. My analysis could profit from such comparisons. But because the prestige differential is so much greater between skid row men and mission preachers than be-

tween most clergymen and their congregations, the sanctimoniousness stands out so clearly that a comparison group is less necessary.

I certainly do agree with Mauss in his distinction on the place of volition in the etiology of problem conditions as distinct from recovery from their effects. Although one need not hold moralistic assumptions regarding the etiology of poverty or alcoholism, volition is crucial for recovery. Nevertheless the carping emphasis on volition in the situation of such great prestige differential between the parties gives it a very different connotation. The stark contrast of the pious "self-assured saved" and the "outcast sinners" is a most noteworthy characteristic of this interaction system. To fail to recognize this context is to miss the key factor in the interaction system. I have not abstracted the behavior out of its natural context to a nebulous or distorted plateau, but on the contrary, have attempted to describe behavior within its meaning context, one which the men in the benches feel.

Another part of this pious context which Mauss mentions is the emphasis on complete abstinence from liquor, which he states is not totally inappropriate. I certainly do agree that strict abstinence is nearly always necessary for recovery from alcoholism, regardless of the treatment regimen. However the situational context intrudes again. From their Fundamentalist values, the mission preachers look on *all* alcohol consumption as an absolute moral curse. In contrast, Alcoholics Anonymous as an organization takes no position on alcohol in society while never relinquishing its emphasis on abstinence for its members. The rabid moralism of the former position is unmistakably infused into the tone of many sermons.

Mauss is again very correct in pointing out that skid row men receive psychic punishment and degradation from the police, judges, store owners, and many social workers. Derogation certainly is not limited to the missions. In fact, Wiseman (85) points out that a jail sentence for drunkenness is itself a recognition of social disgrace. The derogation received from the missions, however, is distinctive in that it rests in the context of supernatural sanctions in which many men have some minimal belief. Other organizations do not invoke the sanctions of this type in their derogation of the men. Furthermore, it was stated in Note 3 (922) that mission preachers operate with great fervor in contrast to secular rule enforcers who approach their tasks in a more routinized, detached manner. I recognize that there is actually a difference of degree between mission directors and secular rule enforcers in their motivating moral fervor, but this difference is important in terms of the degree of derogation in that religious norms are perceived as absolute.

I agree with Mauss that every treatment program does seek to reform its clients in various ways, that many clients utilize service programs for multiple private purposes, and that only a small proportion are *bona fide* converts. Such data support the general model of organizational failure

which I proposed: continuance of a program due to failure to diminish its target problem. And the lack of *bona fide* converts is covered under the discussion of facilitating conditions for continuous failure, specifically that motives of the two parties be at cross-purposes (920).

I did not intend to imply that all skid row men are helplessly snared on a misery-go-round of missions. The paper reported that of skid row inhabitants, approximately one-third attended services at missions during a single year and that only 6 to 8 percent attended frequently (907). Certainly the majority of skid row residents successfully avoid the missions, and as Mauss observes, the vast majority of those who do attend, use them as a springboard to other activities of their own choosing. The fact that missions are used as a place of temporary survival as a result of a variety of problem conditions and are way-stations to multiple future objectives is the key factor in their serving a multitude of functions such as harboring the unemployed, supplementing inadequacies of the welfare system, and motivating men to seek low paying jobs as a lesser of evils. Mission directors are aware of some of these functions. But aware or not, these are phenomena stemming from the operation of the mission system. And many of these functions parallel the positive functions of poverty as explicated by Gans. Mauss knows very well that the actions of all of us have some intended and unrecognized effects.

My omission of relationships to sponsoring church organizations, boards of directors, and individual supporters was intentional. I am aware of these relationships through systematic interviews I conducted with mission directors. Since my data were not nearly as extensive on these relationships, I focused on what I could describe and analyze well: the men and the mission preachers. The policies and activities of sponsoring and supporting groups in this nexus are well worthy of future analysis.

I do appreciate Armand Mauss' interest in this research. The issues he raises are important ones and I am grateful for the opportunity to clarify some unexplicated issues regarding the context in which the idea developed and to expand on the source of the prestige difference between skid row men and the representatives of community institutions.

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# Book Reviews

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## **Sociological Theory and Research: A Critical Approach.**

*Edited by Hubert M. Blalock, Jr.. New York: Free Press, 1980. 448 pp. \$22.95.*

*Reviewer: NEIL J. MACKINNON, University of Guelph*

This is a book of issues and exhortations embedded in essays selected from papers delivered at the plenary and thematic sessions of the 1979 Annual Meeting of the American Sociological Association. The theme of the 1979 meetings, under the direction of Hubert M. Blalock, was the relation between theory and research. Part One of the book consists of four chapters derived from the plenary sessions, focusing on issues in general sociology, Part Two, twenty-six chapters from the thematic sessions, dealing with issues in particular substantive areas of the discipline. What are some of these issues and exhortations? The following are those that seem to me to appear most often in these thirty chapters.

### **1. Measurement**

This is perhaps the most commonly raised issue in the entire book. On the conceptualization side, some contributors complain about the most fundamental problem—that involving the definition of particular substantive areas. Gary Marx, for instance, criticizes those who deny that there are any differences between collective and conventional behavior. Johnson laments “the inability to achieve consensus on the very definition of the term [secularization] itself” and concludes that, “in short, the field is a mess.”

Other contributors point to problems in defining particular concepts. Keyfitz, for example, describes “demographic transition,” as “a concept of ill-defined boundaries and uncertain characteristics.” In his chapter on the life cycle, Pearlín points to the problem of “different meanings acquired by similar terms and by the proliferation of different terms to convey similar meanings,” making it difficult to appraise what the field really knows. A number of contributors specify limitations on the application of particular concepts. For example, Form states that such central concepts as productivity and authority “are necessarily crude when applied to all industries.” Wilson points to the problem of whether the term “ethnic stratification” means the same thing when we move from industrial to primitive societies. Others attempt to sharpen concepts in their particular substantive areas—for example, the concept of “occupations” by Udy and by Moore.

On the operationalization side of measurement, we have Moore arguing for the importance of “a satisfactory operationalization of structural variables,” Frisbie alluding to measurement problems in diachronic research in ecology, and Elinson

pointing out the problems in developing health status measures. Others express concerns about reliability and validity (Ryder, for example). Finally, Blalock warns us against the temptation "of substituting relatively simple operational indicators for theoretical constructs without paying careful attention to the underlying measurement model." In this connection, he offers the frightening prospect that "an apparently simple form of behavior, such as discrimination, aggression, or avoidance, requires for adequate conceptualization auxiliary measurement theories *containing as many as twenty or thirty variables*" (italics added).

Among the exhortations offered is Blalock's that "sociologists need to work *together* on these problems. We can ill afford to go off in our own directions, continuing to proliferate fields of specialization, changing our vocabulary whenever we see fit, or merely hoping that somehow or other the products of miscellaneous studies will 'add up.' The plea, then, is for a sustained effort to clarify our theoretical constructs. . . . It is particularly important, he argues, that we add unmeasured variables to our models to bring implicit assumptions out into the open, and that we theorize more explicitly about linkages between measured and unmeasured variables.

## 2. A Reaffirmation of the Sociological

Despite the growing interest in social psychology and the flights into psychologism that have sometimes characterized our discipline in recent decades, a common theme across the various chapters has to do with an emphasis on macro or societal factors in the explanation of social phenomena. We have, for instance, Blau's persistent argument that social structures have emergent properties, and that "the prime objective of sociology is the study of social structure and the processes that generate and change it, which implies that our units of analysis are collectivities, not persons, because only collectivities have social structure." This reaffirmation of the sociological is reflected in various substantive chapters. Meyer argues that treating education as "only interactional or organizational neglects the institutional or societal components of its structure and history." Bidwell states that the "neglect of organizational and institutional variables makes the school effects literature inadequate sociologically." Ryder argues that, contrary to the suspicions of reductionist sociologists that "properties of the collectivity are myths invented by Durkheimians," fertility is a collective property and calls for explanation at the macroanalytic level in terms of other properties of the collectivity. Wilson writes that the "social control perspective" in race and ethnic relations suggests the need to consider institutional arrangements and the way these affect social values. Toby asserts that "the best starting point for explaining the genesis and control of deviant behavior is sociocultural analysis"—only after that can the causal input of personality systems add further insight. Clausen refers to the importance of social support systems of the Durkheimian genre in his treatment of stress. While lamenting that about three-quarters of the theoretical domains he reviewed in the sociology of religion "deal not with social phenomena but with individual psyches," Hammond argues that "religion, it is being rediscovered, is a social fact." Even in the most psychologistic of the subfields covered in this book, game theory, Friend and Laing acknowledge the importance of the social and organizational context of human

behavior, pointing to "relevant institutions that are important sources of constraint" on the decision-making process in collective action

### 3. Historical and Comparative Research

Coupled with the emphasis on the macro is an exhortation for more historical and comparative research. Swanson asserts that sociologists need to become equipped to work in a comparative framework, as one method of fostering a general sociology. Form stresses "the need for occupational sociologists to do historical research if they want to answer historical questions." Kevlitz treats demographic models as special cases of historical explanation. Williams states that "the recurrent calls for cross-cultural, comparative, or cross-national research [in ethnic relations] are well founded."

### 4. Idiographic Versus Nomothetic Research

Although usually conjoined, the argument for more historical work does not necessarily entail one for comparative research, which is the basis for a nomothetic social science. Portes' argument that "the extrapolation of survey research methodologies to complex structural problems" is inappropriate because of the uniqueness of national histories, and his opposition to cross-national tests of the different perspectives in development theory is compatible with a rejection of a nomothetic approach. In contrast, Swanson singles out historicists who believe "that the very principles by which collective relations operate vary in kind from one historical period or situation to another" as an impediment to a general sociology. What Keyfitz has in mind in treating demographic analysis as historical explanation is the "covering law" model of Hempel—clearly a nomothetic position.

Some authors admit idiographic problems, but maintain a nomothetic stance. Frisbie, for instance, points to "idiographic factors that create problems because they cannot be incorporated into a general or nomothetic framework" explaining the growth of cities. While Williams stresses that we must "specify the context within which a given generalization can be expected to hold true" in the study of ethnic relations, he also argues that "a necessary aim of a scientific sociology is to establish generalizations that transcend dated and localized descriptions . . . and these generalizations cannot be established or falsified solely on the basis of studies of a single culture or society."

### 5. Ideology Versus Research

A number of authors complain that their substantive areas are plagued by too much ideology and too little research. For instance, Form asserts that "European sociology has long engaged in ideological disputes that often go nowhere because so many sociologists would rather argue than do research": "the function of ideology," he says, "is to simplify the world and the function of research is to discover its complexities." Williams points to one-sided theoretical frameworks

"resulting in many ad hoc or 'ideological' interpretations that are not articulated closely with systematic comparative data." Rock berates the warring schools in deviance theory for being "ultimately embedded in a metaphysics which resists final proof." Similar complaints are made by Johnson about secularization theory and by Clarke about the sociology of education.

## **6. Paradigmatic Proliferations and Squabbling**

Closely related to the ideological issue, largely as cause to effect, many contributors lament paradigmatic dissensus in the discipline. Rock, for instance, refers to the sociology of deviance as "a most factious discipline," within which theoretical debate "is not wholly disinterested." Homans would consider this state of the discipline as "scandalous if it were not so ridiculous," and suggests that sociologists refrain from the search for general propositions "because they unconsciously fear they would lose their cherished identities." On the side of exhortation, Blalock argues that "it is crucial that we learn to resist overplaying our differences at the expense of common intellectual interests" and that "an idealization of dissensus is self-defeating." Swanson's argument for a general sociology can be conceived of as an important counter to the proliferation of paradigms within the discipline.

## **7. Demythologizing the Discipline**

A number of contributors attempt to rid their particular subspecialties of myths, many of which have been mistakenly treated over the years as sociological truisms. Leik, for instance, asserts that such small group ideas as Bales' dual leadership principle are either invalid or must be seriously qualified when applied to natural groups. Riley reports that only a few hundred of the thousands of past studies on aging meet the minimum criteria of empirical validity, perpetrating such erroneous generalizations as the "life course" and "age reification fallacies." Pearlin discusses the fallacious view that life transitions in later life lead to dramatic emotional changes; emotional stress, it seems, is most likely among the young. Form rejects the idealized version of the guild system and the "skill degradation" hypothesis perpetuated by many students of occupational sociology as romanticizing about the past, and argues "that the alleged link between the spread of machine technology, work routinization, and worker alienation may be a myth waiting to be exposed." Killian attempts to rid the area of collective behavior of the "irrational collective behavior" model "that the critics both created and attacked," which he sees as "a grotesque caricature" of what the area is about.

## **8. Explanatory versus Descriptive Research**

Those contributors who at least implicitly bring up this issue quite clearly favor explanation over description. Swanson's argument for a general sociology and Homans' for theory construction are quite explicit arguments for an explanatory sociology. Frisbie asserts that urban ecology has "too often been satisfied with



description alone and too little interested in explanation" Wilson says he is "struck by how much of the work [in comparative ethnic relations] has been descriptive and typological in nature and how little of it incorporates basic theoretical propositions." Hammond concludes from his survey of the literature in the sociology of religion for the last twenty years that about 80 percent of it is atheoretical "data-dredging, the turning over of ever more stones," while "science properly is the *reduction* of empiricism, not its endless elaboration." Hawley implies that demography is too descriptive by asserting that "when the population student turns from description to explanation" he must draw on other social science disciplines.

This book represents an important statement about the state of the discipline at the end of the seventies. It should (although it probably will not) be read *in its entirety* by all graduate students and by those of us whose reading since graduate school has yielded to the ever-increasing pressure towards specialization. All of the chapters are well-written, and it would be unfair to criticize the book for a lack of continuity from chapter to chapter, given the number of subspecialties represented.

A book of this scope and diversity is difficult to summarize. I have attempted to ferret out those issues that appear over and over again throughout the volume. None of these issues is new and it is a sad commentary on the discipline that they have been with us these many years. Nonetheless, some of the exhortations here might prove effective if they were to be taken seriously by working sociologists—in particular, the appeals to rid the discipline of ideological dissensus and paradigmatic parochialism, to derive general sociological concepts and principles from the ever-proliferating number of substantive areas, and to work on the refinement of measurement and the incorporation of measurement models into our theories. If they are not taken seriously, we have the bleak prospect offered by Blalock "that sociology in the year 2000 will be no more advanced than it is today, though perhaps it will contain far more specializations, theoretical schools, methodological cults, and interest groups than, even today, we can readily imagine."

### **Passing on Sociology: The Teaching of a Discipline.**

By Charles A. Goldsmid and Everett K. Wilson. Belmont, Ca.: Wadsworth, 1980. 432 pp. \$21.95.

Reviewer: RIECE MCGEE, Purdue University

Before I begin, disclosure is in order. I am not at all sure of the propriety of my doing this review, and agreed to do it only when the book review editor urged me to open with the following statement: The reader should be aware that both authors of this work are close, personal friends of long standing, I have worked closely with both of them on the ASA Projects on Teaching Undergraduate Sociology, I once substituted for Everett Wilson in his course on teaching graduate students to teach, from which course the authors began the work that eventuated in this book; I use the book myself as a required text in a seminar modeled on Wilson's; I was paid by its publisher to review the manuscript in two early versions and I am quoted on the dust jacket praising it; I am cited in it in several places. One might assume, then, that I did not approach the present task in a mood of total objectivity.

Such an assumption would of course be erroneous, and I hope that any sociologist interested in the teaching of sociology and its improvement will accept as objectively accurate my statement on the dust jacket that "*Passing On Sociology* is the best thing of its kind ever written," for it is the simple truth.

People familiar with the literature on college teaching will probably compare Goldsmid and Wilson with McKeachie's *Teaching Tips: A Guidebook for the Beginning College Teacher* (1978), and that is a work to which it is closely comparable. But the two books are not by any means identical or even exchangeable. (In fact, I require both for my teaching seminar, as do at least some others who teach similar courses. McKeachie's book, as its title suggests, is a brief handbook of helpful hints for the novice teacher. It is invaluable for that purpose, and useful even for the experienced teacher. It also contains some review of the research literature, but this appears largely in justification of the "tips" it offers.

*Passing On Sociology* is much, much more. It is a sociology of sociology teaching, an immensely *sociological* reference, justification, and ideology for the dedicated and reflective teacher. (I stress its "sociologicality" because that quality is notably lacking in many of the discussions we have about our work.) It tells us, through sociological analysis, what we do in our teaching, why we do it, what we could (and sometimes should) do, and why. It explores the research literature on college teaching more thoroughly than any other work I know, summarizes it and draws conclusions from it in ways that allow readers to judge what might sensibly be attempted in their own classrooms. (Goldsmid's base in the enormous bibliographic effort of the ASA Undergraduate Teaching Projects is apparent here.)

Underlying the entire enterprise are a set of powerful convictions that illuminate the work and describe its nature: humane knowledge is important to civilization itself, and thus teaching is important; significant societal tasks should be undertaken rationally and from as wide and solid a knowledge base as possible, and thus we should know what is known about teaching and think about it seriously; means should be rationally connected to ends rationally selected, and thus we should organize our teaching in that way; anyone of normal intelligence can learn to be an effective teacher, and thus there is no need or excuse not to be.

No better description of the book can be provided than that offered by the table of contents. Part One—Problems and Positions—consists of three chapters, "The Current Context of Sociology Instruction: Where do we Stand?"; "Dual Roles and Double Standards: How can we Reconcile Them?"; "Our [the authors'] Persuasions." Part Two—What Are The Ends We Seek?—addresses both goals of sociological content and such goals as teaching critical thinking. Part Three—What Are The Means We Use?—discusses constraints on the means of instruction, common problems of sociology instruction, two common tools (syllabus and textbook), three course patterns (lecture, discussion, and "personalized system of instruction"), vivifying the class session, and research findings on the means of instruction. And Part Four—Are The Means Effective In Gaining Our Ends?—talks about the evaluation of student and instructor achievement.

In some respects, this reads like the contents page of a number of other books on teaching, and so it is worth stressing again that this book is much more. In every instance, the chapters stress the teaching of sociology in particular (although of course much that is said is generalizeable to other disciplines). Every chapter is solidly based on substantial research (of which there is a great deal more than most

sociologists know), but the book is more than an encyclopedia of results. This is a discussion among colleagues about what is known and done about teaching, not a reference book. It is guided throughout by the kinds of questions that serious teachers ask themselves in the broken hours before dawn, or ask each other over coffee when they can no longer contain their perplexities and anxieties about their work. It is also a book that many of us will want to read again and again, for information and, more importantly, for hope—for the affirmation that improvement is possible, and that we are not alone in our doubt and, sometimes, despair.

All of this, which I say with sincerity and fervor, does not mean that I think the book is unflawed. One minor problem, brought to my attention by my graduate students, lies in its use as a text. Because it was not written as a textbook, it is not organized like one, and so some topics (the lecture class, for example) appear in more than one place. Thus if chapters are assigned whole, the book gives the false appearance of redundancy. I do not regard this as important: once recognized it is easy to get around. Two other flaws are more significant, in that they may affect how readers will respond to the work.

One of these is the authors' refusal to admit that some students are unreachable, that no matter how conscientious and well prepared the instructor, no matter how diligently he or she works at teaching, there will be some students who will not respond because they do not wish to, some who are hostile to ideas that disturb traditional certainties and to instruction because it involves changing *them*. Some students simply do not belong in college or, at least, in sociology courses. It is not a service to our colleagues to deny this, and Goldsmid and Wilson do so by assumption and implication, which means the teacher must shoulder the blame for all failures of learning. I reject that, and think that teachers should be told outright that they can't "win them all," and that it may not be their fault if they don't.

An even more important problem, in my opinion, is a matter of style or tone. The authors admit that the book is a counsel of perfection and I have told them repeatedly that it speaks in places with the evangelical fervor and moral self-righteousness of an Old Testament prophet. They address this criticism in the Preface, but I do not think they took it seriously. I fear that many readers will come away from the work feeling guilt for not being better teachers than they are, and despair because they cannot take advantage of the multitude of suggestions they have been offered. One of my seminar students expressed it this way: "I really liked G & W, but it raised my guilt feelings like crazy. No one could possibly do half of what they suggest even if they worked at their teaching twenty hours a day."

This is a serious flaw. A little guilt may effectively motivate us to do better, but too much is paralyzing. When role models are perceived as too perfect, like the conventional Jesus, they make people feel bad about themselves and they will be ignored, because paying attention to them is painful. I fear that may happen to this book, and that would be too bad, because any work as deeply felt and carefully prepared as *this one*, and aimed at resolving a problem as significant, deserves to be heeded. The authors are fond of saying "Telling isn't teaching." Well, neither is preaching, and many of us don't like it. I sincerely hope that readers who have this reaction (and some already have) will try to look beyond it. Good sermons are, by definition, worth listening to. And, while this certainly does have sermonical qualities in places, it has a great deal more besides. Anyone who is or intends to be a teacher of sociology should have it. "It is not a service to our colleagues to deny this, and Goldsmid and Wilson do so by assumption and implication, which means the teacher must shoulder the blame for all failures of learning. I reject that, and think that teachers should be told outright that they can't "win them all," and that it may not be their fault if they don't."

**The Changing Academic Market: General Trends and a Berkeley Case Study.**

By Neil J. Smelser and Robin Content. Berkeley: University of California Press, 1980. 198 pp. \$16.00.

Reviewer: LIONEL S. LEWIS, *State University of New York at Buffalo*

The last part of this book's subtitle and intimations in its Preface ("we reveal in this volume a kind of information that is seldom made public") might raise the expectation that here is yet another breathtaking confession—as discomfiting as Martindale's (about Minnesota) or Silva's (about Wisconsin)—of intrigue and duplicity in a nationally prominent sociology department. Alas, such hopes will be disappointed. What we have instead is a sober, even pedestrian, report on the exhaustive search for four junior faculty members in 1975–76, while Smelser was chairman of the Berkeley sociology department. Although the account is thorough and candid, it is more memoir than scholarship, the kind of information that is generally not made public simply because it is so prosaic. It is hard to imagine that any book that includes a 22-page description of how a recruitment plan was devised could generate intense interest. Those who expect a sociological study in the probing and ironic spirit of Caplow and McGee will be disappointed. There is some irony in this book, but it is unintended.

Smelser and Content do make a ceremonial effort to put their account in a sociological framework, in fact, they use the Caplow and McGee study as their backdrop. But they minimize the importance of this groundbreaking work. They believe, first of all, that the academic marketplace has changed so much since the 1950s that Caplow and McGee's generalizations are no longer valid. Moreover, they believe that Caplow and McGee overemphasized the haphazard way that information about positions and candidates was obtained, the "personal influence among networks of colleagues," and the subjectivity of evaluations in the process of recruitment. At bottom, they find Caplow and McGee's assessment "cynical and denunciatory." Smelser and Content feel that their data are more informative, but, in spite of their industriousness, many readers will disagree. The first two of the book's eleven tables, for example, show that candidates from highly ranked departments are more likely than others to apply to a highly ranked department. The next two tables show that candidates from highly ranked departments are evaluated more favorably than others by faculty in a leading department. All in all, there is little that is new here, but the book deserves our attention because its basic weaknesses are so typical of what is currently passing as the sociology of higher education, an activity that seems to have abandoned some of the basic precepts of sociology.

Caplow and McGee concluded their book by urging that the search for new faculty be made more open and rational. The Berkeley sociology department became one of the leading departments (according to some rankings, *the* leading department) in the country in little more than a decade after its founding in 1952 by using informal and unsystematic recruitment procedures, but nevertheless it decided to do just as Caplow and McGee recommended. Smelser and Content chronicle the great effort to make the search fair. One cannot but be impressed by the comprehensiveness and attention to detail both of the search and of the description of it. A mountain of information accumulated on the 285 candidates.

Everything was dutifully recorded and presumably read. A magnetic-card typewriter and mini-computer were brought into use. The burden on committees and on the chairman mounted. There were visits, meetings, and deliberations. And, of course, it all took a great deal of work, time, and money: the secretarial staff had to be reorganized and the administration had to supplement the departmental budget. Faculty were expected not only to cooperate, but also to become active in all phases of the search. In case readers miss the point, Smelser and Content repeat each step at least twice. The book and the search are much alike: painstaking, fair-minded, and unimaginative.

When the ritual ends the department at Berkeley has made its choice: two of its new members will be from the University of Chicago, one will be from Harvard, and one will be from Stanford. "We feel confident," Smelser and Content write, "in asserting that these economic, political [the added salience of this dimension], and administrative changes [a more complex bureaucratic structure] constitute the major lines of deviation from the Caplow and McGee model." So much for Caplow and McGee's recommendation.

Given the authors' interpretation of their experience, one might expect them next to assault affirmative action, but either their logic or their courage fails them, and they are quite circumspect on this question. In spite of their conviction that "the result was—and, we believe, would again be—virtually the same as if the entire process had never been undertaken," they only timidly "question the potential of somewhat 'mechanical' requirements . . . for changing the fundamental process that is at work." The fact that the Berkeley sociology department went through this elaborate rigmarole and ended up hiring from Chicago, Harvard, and Stanford does not give Smelser and Content pause. They attribute the outcome to the high quality students and the excellent training one can receive at these institutions. The inadequacies were in the candidates from lesser schools (only 6 of 81 were rated as strong, compared to 29 of 136 from top-ranked schools), not the perceptions of the evaluators.

According to Smelser and Content, the academic coin of the realm is prestige. It is thus inconceivable to them that a first-rate department would not hire the best people: "As we noted earlier, the value of excellence in science and scholarship is paramount in research and graduate-training centers, and these centers strive continuously to increase their relative prestige by maximizing that excellence." They ignore the possibility that excellence and prestige may not be the same thing, or that institutional prestige can be attained by means other than demanding excellence from faculty.

They are open and above board in acknowledging "that so much of our work seemed to be wasted. . . . The final result of the search . . . was much the same as it would have been if we had written letters to colleagues in the dozen leading departments and asked them to name their best students." Perhaps, but their next sentence is: "We were aware of this at the time of the search." One gets the impression of an experiment that, if not designed to fail, was not given a decent chance to succeed. Smelser and Content write that "the entire process appears to have an overdetermined if not a predestined outcome."

That assessments of candidates are not as objective as one might wish is a conclusion that apparently does not have much appeal. Are letters from trusted friends given more credibility than they deserve? Of course not, say Smelser and

Content: these individuals must be objective, for they have their professional reputations to protect. But in fact, those who write inflated letters for their students, need not worry about later embarrassment. In the first place, failure will be explained away by a bad marriage, an addiction to alcohol or undergraduates, or chronic postgraduate exhaustion. Most importantly (and at the heart of the matter) sociologists should recognize that what looks like individual behavior is often merely a reflection of a social context; that the social milieu has a direct effect on behavior. Applying this proposition, it follows that what young academics will contribute in the end is determined in large part by where they find their first employment. Given a modicum of competence, academic productivity is as much determined by departmental norms and by opportunities as by what individuals bring with them to a department. Simply put, differing emphases on research and teaching as well as the availability of facilities affect the production of books and articles. Given accumulated advantages, most good students from most graduate schools would probably perform similarly at Berkeley. I doubt that much distinguished what the faculty rated as the top 11 candidates from the next 24, or maybe even from the next 48. Smelser and Content seem to have reified differences (such as who recommended a candidate) that exist only in their minds. The question is, were there really objective criteria that set the leading candidates apart from many of the others?

Smelser and Content are convinced that academic performance can be evaluated "in accord with accepted canons of logical adequacy and empirical validity of knowledge produced, as well as canons of 'creativity,' of the generation of new knowledge and insights." They acknowledge that there are "variations in consensus on the criteria by which excellence and promise are assessed" and "criteria that are alternative to . . . universal criteria of excellence enter into the recruitment process." Yet, these candidates were largely untested; few had yet produced work for evaluation.

A paragraph from an essay written a dozen years ago seems particularly relevant to the Berkeley experience:

[Departments of sociology] seek to recruit the best possible staff of professional sociologists and, to that end, they seek to employ the brightest young sociologists entering the market from the graduate schools each year. Given a number of competitive applicants for a single position, departments will make their choice on the basis of an assessment of the potential quality of the applicants' performance rather than an assessment of the prestige of their school of origin or their academic sponsors. On the other hand, few departments could afford the expense of actually examining the total output of all of the graduate schools in the country. It is more economical to select a relatively small sample of prestige schools and to recruit from those schools more or less exclusively. On an actuarial basis it is probable that high prestige schools will produce higher quality candidates. Of course this technique works to the disadvantage of the best graduates of some urban state universities and other schools with lower status, and to this extent a variety of ascriptive factors operate in the talent market in the field of sociology. [It] is not direct prejudice . . . that is operating here, rather it is the *indirectly* ascriptive effect of a decision based on the perfectly rational economic grounds that it is inexpensive to utilize established social structures. (L. Mayhew, "Ascription in Modern Societies," *Sociological Inquiry* [1968] )

It is hard to believe that Smelser and Content were purposely engaged in a complex effort to affirm the dubious tenets embodied in this paragraph. Probably the similarity came about entirely by happenstance (there is no citation to this piece). But whatever the facts here may be, old wine is still old wine. Those who

were not persuaded in 1968 that ascription, because it is economical, serves a positive function, will probably not buy that claim now, either.

Smelser and Content are correct in arguing that excellence in science and scholarship can and must be determined in the marketplace of ideas. The value of their volume should be judged by that criterion.

**Social Science Research and Decision-Making.**

By Carol H. Weiss and Michael J. Bucuvalas. New York. Columbia University Press, 1980. 332 pp. \$22.50.

Reviewer: GEORGE H. CONKLIN, North Carolina Central University

The usefulness of social science research to policymakers in government is a subject which has been debated for many years. In light of the uncertain future of federal funding for behavioral and social science, any results which can show the usefulness of social science research to decision-makers in government would be welcome indeed. Weiss and Bucuvalas have gathered much information on the subjects from interviewing 155 decision-makers in mental health agencies, 50 members of mental health review committees, and 50 social scientists who have conducted mental health research. The results are surprising.

Any research project must limit its scope, so *Social Science Research and Decision-Making* is in fact limited to federal, state, and local decision-makers in ten states. Each respondent was asked to rate the usefulness of two studies out of 50 summaries of empirically based social science results. There were general questions also on the usefulness of social science work to decision-making without reference to specific studies.

Based on earlier research, Weiss and Bucuvalas expected to find that decision-makers would say that social science research was not useful to them for any of 19 reasons. Expected answers included the fact that social science research often contradicts current agency practice or explains results by variables such as social inequality or social class, over which a decision-maker has almost no control. It turned out few decision-makers felt social research not useful. Indeed, research results which contradicted the status quo were felt to be highly valued, especially if the perceived quality of the research was high. Decision-makers seem to evaluate studies en masse, storing findings away in their minds for the day when practical decisions need to be made.

Thus instead of telling us why social science research is not of much use to governmental decision-making, the respondents generally testify that, in the realm of informed opinion, social science research is of great use. Taken as a whole, social science research does govern decision-making by providing an educated, informed climate in which rational decisions can be made. Weiss and Bucuvalas are aware that many readers will not accept this conclusion: "Outsiders may scoff. It sounds like a courteous tale told to fob off a visiting social scientist. . . . But we do take it seriously." What began as an empirical study ends up with a suggested theory of mature public opinion guided by empirical sociology. Of course, there are no data to support the validity of these logical extensions. So, in the tradition of not going beyond the data, few elaborations are made.

In most studies the reader is given a chance to see the original questionnaire, but unfortunately, for some reason, Weiss and Bucuvalas have left out all but two of the research summaries on which the results of the book are based. One of the summaries they do include is of the 1967 and 1972 National Surveys of Youth which reports, among other things, that marijuana use has increased among youth. One wonders what a decision-maker can do about such results. Since each of the 155 respondents had only two studies to rate, presumably only a handful had this particular study. It would still be useful, however, to know the ratings for each report, for then the reader could at least have some idea how the dependent variables were ranked in practice. Without such information, the reader is left in the dark.

The conclusions of *Social Science Research and Decision-Making* thus turn out to be the correlations of opinions with other opinions. It would be possible at this point to state that no evidence is presented that social science research is actually used by respondents. Technically, this is true, since we do not know if respondents really do what they say and use social science research results in their jobs. Yet, when all is said and done, the authors could be correct. Taken as a whole, individual research reports may be neglected, but 100 reports of research findings may well have some influence. The results imply that current undergraduates ought to be introduced to the data of the social sciences in introductory courses, rather than being subjected to the usual mixture of theory and definitions. Sociologists will need to stop talking mostly to each other and to self-selected majors, and much more to the general public. If social science has any influence, it seems to stem less from individual targeted studies than from many replications over a long period of time, through informed public opinion.

### **Economy and the Class Structure.**

By Rosemary Crompton and Jon Gubbay. New York: St. Martin's Press, 1978. 248 pp. \$18.95.

Reviewer: LARRY J. GRIFFIN, *Indiana University*

Rosemary Crompton and Jon Gubbay are British sociologists who have been strongly influenced by the recent theoretical developments loosely labeled French structural Marxism. Using structuralist imagery and explanatory schema, Crompton and Gubbay attempt to extend the insights of contemporary neo-Marxism by developing and defending a "modified" Marxist model of class stratification, while recognizing that Marx's original formulation ("a gross oversimplification") is simply incapable of adequately comprehending the complex and often ambiguous class structures of advanced capitalist states.

The first four chapters present the basic premises of their views, criticize competing theories, and briefly discuss changes in the capitalist mode of production during the past hundred years or so. Crompton and Gubbay argue, first, that class relations are not to be confused with market relations or distributive processes (although later in the book these are allowed a "secondary" role in affecting class dynamics) and that class conflict is not to be confused with attempts to maximize the advantages of market power. Rather, class relations—and, hence,



class structures—should be seen as rooted ultimately in the structurally exploitative and antagonistic production relations inherent in capitalist economies. Weber and influential British “neo-Weberians,” such as David Lockwood, Frank Parkin, and Anthony Giddens, are criticized for missing or obscuring these points. These observations are generally unexceptionable, competent, and fair (it is acknowledged, for example, that the neo-Weberians have made some important contributions). Sometimes, however, they appear as assertions rather than demonstrations.

The real meat of the book is contained in Chapter 5 (“Economy and Class Structure in the West”), Chapter 6 (“State and Economy in the West”), and Chapter 9 (“The Political Economy of Class”). Crompton and Gubbay identify “class situations” in an *abstract* advanced capitalist production system by locations of the various flows of surplus product. Roles or positions in industry and some services generate the surplus product; others (in particular, services, commerce, finance, and rentier establishments) allow for its realization—i.e., conversion into money; still others (e.g., positions in state apparatuses or non-profit private enterprises) produce or reproduce the conditions necessary for the production or realization of the surplus. All such positions are controlled by capital, carry out tasks specified by capital, and exist to facilitate the accumulation process; they function as the “collective worker.” There is essential unity, then, among these positions (note: not individuals) which transcends sectoral location, specific task, position in the bureaucracy, productive/non-productive distinctions, and even public-private relations. Similarly, other positions, which organize the labor process, evaluate and discipline the “collective worker,” and are responsible for investment decisions, perform the “global function of capital.” Again, then, essential unity. By recasting Marx’s theory of classes into the functional activities of constellations of roles, Crompton and Gubbay preserve the essential Marxian dichotomy of “capital” and “labor,” even in the modern era when the size, structure, ownership, control system, and environment of the corporation differ substantially from those of the simple entrepreneurial firm.

In Chapter 7, the authors attempt further to demonstrate the applicability of their model of class situations by analyzing the class structures of Eastern European nations. Once again, their arguments are all too brief, but, in this case, they are controversial and tantalizing: e.g., state socialist societies (a term the authors avoid) are capitalist, exploitative, and class-stratified. A more complete analysis on this issue would have been welcomed. Crompton and Gubbay also include the apparently obligatory chapter on “Class and Inequality.” Beyond an extremely insightful outline of an as yet underdeveloped Marxian theory of income inequality, this discussion offers little that is new.

It is not entirely clear how this book will be used by empirical researchers (its explicit purpose). On the one hand, its exposition of the concepts of “collective worker” and “global function of capital,” while not entirely original (it owes a good deal to the work of Guglielmo Carchedi, a Dutch economist), and its Marxist interpretation of income distribution, will be useful for some Marxian studies. On the other hand, Crompton and Gubbay are quick to acknowledge that one should not mechanically apply their abstract conceptualization to actual societies, that the class situations of many work-related roles are structurally ambiguous (some, for instance, simultaneously perform the functions of the global capitalist and the collective worker), and that many secondary factors influence actors’ perceptions of

the class structure. Only empirical-historical investigations ultimately suffice, they argue. The authors are to be applauded for their honesty, of course, but their own qualifications suggest the limitations of a structural approach.

This brings me to my final question: Is it genuinely possible to integrate structuralism with the less-than-grand theories, the messy detail, and the extraordinary complexity of the studies of such Marxist-influenced historians as Hobsbawm, Hill, Genovese, Thompson, and Gutman? In other words, can structuralism, with its emphasis on analytical abstraction, theoretical formalism, conceptual elaboration, and logical purity, explain the real stuff of history, the attempts of living men and women to create the material and symbolic situations within which they labor and which they struggle against? Can structuralism—however flexible and circumscribed—accept the truth that classes are not slots in some static social space, but, in Adam Przeworski's brilliant phrase, are always in the process of being "organized, disorganized, and reorganized?"

**Class: Image and Reality in Britain, France and the USA Since 1930.**

*By Arthur Marwick New York: Oxford University Press, 1980 375 pp. \$19.95.*

*Reviewer: ROBERT V. ROBINSON, Indiana University*

Two views of class structure and class consciousness in advanced capitalist societies are considered in this book. One view—the Marxist position—is that classes are increasingly becoming polarized, the other, more conservative view is that class distinctions are declining. Arthur Marwick, a British social historian, brings cross-national and historical data to this debate, examining rich primary materials (literature, film, correspondence, scholarly works, government reports, social legislation, etc.) in Britain, France, and the United States from the 1930s through the 1970s. The author concludes that neither view is entirely correct. While class distinctions are increasing in the U.S. as racial distinctions become less salient, class distinctions are becoming less important in France and Britain as these societies become more mobile, affluent, and (like the U.S.) racially divided.

Marwick's conclusions are plausible, and the book well-written, but his argument is conceptually muddled. Crucial to any analysis of class structure and consciousness is the author's definition of class. Marwick rejects two objective sorts of definition: the Marxist one of social relations of production and multidimensional ones that combine occupation, income, and other dimensions of stratification. Instead, he uses a subjective definition: "I prefer 'class' to mean what people in everyday life mean by it, rather than what Runciman or Weber tell me I should mean by it." Although there is little agreement as to what constitutes objective class (e.g., ownership, authority, occupation, income), a subjective definition has even more serious limitations. First, individuals' subjective impressions of their class position often have little connection with their objective position. Recent research shows that rarely is more than a quarter of the variance in subjective class placement explained by objective characteristics. Yet Marwick, who may be unaware of this literature, seems to expect nearly perfect correspondence between subjective and objective class position, and at one point challenges the validity of a French study which found otherwise. Marwick's orientation cannot address the

nature of these objective-subjective linkages, including questions of how and why a class *in* itself begins to constitute a class *for* itself. His conceptualization thus does not allow the possibility of false consciousness: to the extent that people do not perceive classes, classes do not exist.

Second, recent research shows considerable variation within and between societies in the number of classes people perceived and in the nature of perceived class divisions (e.g., "money," "prestige," and "power" models). Moreover, the number and nature of classes perceived often depend on the objective position of the perceiver. Marwick chooses to ignore this research, as well as theoretical work by Ossowski, Lockwood, and others, and attempts instead to identify a single image of class structure.

Third, in reviewing primary materials for the class imagery they contain, Marwick is surprisingly restrictive in what he labels as "class" imagery. For example, in his discussion of "official" images of class contained in U.S. and British government documents and social legislation, only an explicit mention of the "working," "middle," or "upper" classes is considered to be class imagery. Thus, even though he claims to be using a popular, subjective image of class, in fact he is imposing his own class imagery on the primary materials he considers. He notes, for example, that "The [U.S.] Federal Housing Administration spoke not of social classes, but of 'middle income groups' and 'lower income groups.'" I would be the first to agree that income groups do not constitute classes, but in the U.S. the majority of people (70-80 percent in most surveys) hold a "money" model of class (e.g., "rich," "middle," "poor"). The terminology used by the FHA is not far removed from popular class imagery.

Finally, in discussing the "realities" of class, Marwick gives figures on inequality in education, occupation, income, and political power. Despite the fact that he has previously rejected definitions of class based on these indicators, he frequently equates these objective inequalities with class inequality in the subjective sense that he has been using. The single dimension of inequality that he clearly distinguishes from class inequality is race, and his treatment of the relation between class and racial inequality is reasonable. The relation between class and sex inequality, however, is given little attention, on the grounds that the allocation of women to classes is a "family" matter: women and children "belong to the social class of the husband and father." This ignores power differences within the family which may give women and men very different interests. It also discounts the paid and unpaid labor of women that contributes to their family's class position, and tells us nothing about the class position of women who do not live with men.

The issues examined in this book are important, and more cross-national comparisons and social histories in this area are certainly needed. Marwick uses a wealth of qualitative primary materials and offers some insightful and plausible conclusions, but the theoretical flaws in his work mean that we must still wait for a definitive treatment of the subject.

**Injury to Insult: Unemployment, Class, and Political Response.**

By Kay Lehman Scholzman and Sidney Verba. Cambridge: Harvard University Press, 1979. 393 pp. \$20.00.

Reviewer: PAUL BURSTEIN, Vanderbilt University

In *Injury to Insult*, Scholzman and Verba pursue the answers to three related, very important questions about American politics: Given that unemployment in the U S is both endemic and stressful, why have the unemployed been politically quiescent? Somewhat more generally, how may American exceptionalism—the weakness of working-class political organization—be explained? And, finally, what is the relation between personal economic strain and the mobilization of the disadvantaged?

The analysis is guided by a straightforward conceptual scheme delineating a series of steps through which “those who have interests in common become an effective political force.” If people are to mobilize, they must (1) share an objective condition, (2) experience the condition as stressful, (3) perceive it as shared by others and as an appropriate object of government action, (4) decide on a program for action based on shared policy preferences, and (5) mobilize. Scholzman and Verba see movement from step to step as necessary for mobilization, and devote most of the book to examining empirically the extent of such movement—what proportion of the unemployed feel stress, what proportion of those translate personal feelings into personal goals, etc. Other factors acknowledged to be potentially relevant, such as resource availability and feedback loops in the causal process, are not considered in detail.

Most of the data for this study come from a 1976 survey of 1,370 members of the labor force, residing in metropolitan areas. The unemployed were oversampled; the authors claim that their 571 interviews with jobless respondents are the largest extant set of interviews on the political and social views of the unemployed.

According to Scholzman and Verba, the findings show why the unemployed are not a significant political force. Unemployment is stressful, but there is little connection between it and ideology or class consciousness. The unemployed show little inclination to favor radical government policies to end unemployment; they are not especially active politically; and their political preferences do not differ greatly from those of others who share their class position, but are employed. As a group, the unemployed are neither class-conscious nor mobilized. Scholzman and Verba conclude, therefore, that they “have found little evidence of political action by the jobless to influence political outcomes,” and that the interests of the unemployed are hurt by their own quiescence.

This book provides some interesting data on the attitudes of the unemployed and on the differences (usually small) between them and the employed, but it is flawed in at least two major respects. First, the authors ignore practically all the theoretical work of the past 20 years on political organization, social movements, class structure, and social psychology. It is hard to believe that someone studying working-class political mobilization would ignore the work of Gamson, McCarthy, Oberschall, Perrow, Tilly, and Zald, but none of these is even cited, much less taken seriously into account. The resulting theoretical naivete leads Scholzman and Verba to adopt a research strategy largely incapable of producing results relevant either to

their questions or to other theoretical issues in political science or sociology. The implicit assumption behind the analysis in *Injury to Insult* (stated most clearly in Chapter 8 and the Epilogue) is that political change comes about when everyone changes his mind about something, and that, therefore, the appropriate research strategy is to conduct a survey. The idea that political mobilization is simply a function of attitudes has been thoroughly discredited, and a study design based on this assumption is not likely to be fruitful. Sociologists now know that political mobilization is fundamentally a process of *organizing*, involving complex dynamic relationships among social movement organizations, potential constituents and beneficiaries, target groups, political entrepreneurs, and others, all concerned about resources, strategy, and tactics. This implies that the most direct and appropriate way to study political mobilization is to study the groups and individuals involved. If Schlozman and Verba were familiar with contemporary theory, they might not express so much surprise at what seem to be hundreds of negative findings.

This book is methodologically as well as theoretically out of date, with regard to both data collection and analysis. Although most of the data are drawn from the authors' survey, there are some interesting attempts to use other kinds of data—congressional testimony, counts of demonstrations reported in the *New York Times*, etc. The study would doubtless have been improved if the authors had demonstrated any awareness of what we have learned about using such data in recent years, and put that knowledge to use.

In presenting the data, the authors apparently decided to keep the statistics as simple as possible, perhaps out of a desire to reach a wide audience. Almost all the data presented are raw percentages in two- or three-way tables or figures: there are few significance tests, measures of association, or multivariate analyses. The approach leads to differences between social groups being described as "big," "little," "surprisingly strong," or "surprisingly weak," without any hint of the standards according to which such conclusions were drawn. The presentation is tedious—there are 62 tables and 73 figures in the text—and makes one appreciate the economy of presentation made possible by multivariate analysis. The conclusions would have been more credible as well as more interesting if they had been based on such analyses and presented in about a quarter as many tables.

*Injury to Insult* deals with a topic of continuing importance. Unfortunately, because the authors ignore recent developments in theory and methods, their approach is inappropriate for their questions, and their data can contribute little to our understanding of American politics.

### **Making Ends Meet: How Families Cope With Inflation and Recession.**

By David Caplowitz. Beverly Hills. Sage, 1979. 303 pp. Cloth, \$16.00, paper, \$7.95.

Reviewer: J. HERMAN BLAKE, Oakes College, University of California, Santa Cruz

One of the distinctive features of the 1970s was the introduction of continuous inflation and high levels of unemployment into American life. This seemingly contradictory phenomenon has now become commonplace, presenting very special challenges to most American families. Some have been little bothered by it, while

for others it has raised the haunting spectre of the Depression. Few have been able to ignore it.

*Making Ends Meet* is an analysis of how American families have faced the pressures of inflation and recession, the strategies they have developed to cope with them, and the impact of the economic strains on various aspects of family life. During May and June of 1976, almost 2,000 families were interviewed in four Standard Metropolitan Statistical Areas, in four major areas of the country: the Northeast (New York), the South (Atlanta), the Midwest (Detroit), and the far West (San Francisco). The sample was stratified to ensure that a variety of families were interviewed; in each city approximately 15 percent of the respondents were to be those with incomes averaging below \$7,000, while 15 percent were to be retired persons. The remaining 70 percent were to be evenly divided among blue- and white-collar families.

Caplovitz divides the book into four major sections, analyzing the impact of inflation and recession, their consequences, mitigating factors, and finally the effect on the mind. Some of his findings are what one might expect to come out of such a study: those in the lowest class groups, those with the least education, and minority groups have the greatest difficulty coping with inflation.

Other findings, however, are not as predictable, and all of his results have implications for sociological analysis and social policy. For example, the continued high level of inflation tends to proletarianize the middle class: as they begin to cope with the challenge, they adopt the same strategies as those in less fortunate circumstances. In addition, Caplovitz shows that welfare and public assistance are not the salves many people consider them, and may even exacerbate the hostilities of the poor. In a number of places he demonstrates that recipients feel humiliated by the welfare system and prefer to avoid any form of public assistance. His respondents repeatedly expressed a desire to obtain employment in order to free themselves from a degrading system. However, these are the same people most affected by high unemployment levels; consequently, they expressed the view that the larger society was engaged in a conspiracy to prevent welfare recipients from obtaining jobs. Public assistance clearly is not a buffer against the harsh economic realities, and indeed may engender social hostility.

Equally as interesting are data indicating that victims of inflation-recession see illegal activities as justified in coping with their circumstances. Many of Caplovitz's respondents speak of turning to crime to obtain necessary resources or goods.

As compared to the Depression of the 1930s, the current inflation-recession spiral has produced very little protest or social unrest. Caplovitz concludes that one of the key factors in this quiescence is the high proportion of families who own their own homes. He argues that such families can weather the strains by using their homes to generate additional cash, through refinancing, or by simply taking comfort in the rising value of their property. In either case, Caplovitz feels, homeownership is much more important than public assistance in preventing social turmoil.

Policymakers should take note of the data showing that, when asked about the causes of inflation-recession, almost half of all the respondents placed the primary blame on government or politicians.

Caplovitz has presented an impressive array of findings in a very interesting

manner. His statistical indexes and multivariate analyses are complemented by generous quotations from unstructured interviews. As a result, the book is very readable and its conclusions are persuasive.

**Professionalization and Democratization in Sweden.**

By Gunnar Boalt, Ulla Bergryd et al. Stockholm. Almqvist & Wicksell International, 1980. 264 pp. Sw kr 199.00.

Reviewer LARS BJORN, *University of Michigan-Dearborn*

Gunnar Boalt is one of Sweden's most prolific sociologists and a "neo-positivist" (his own words), representative of Swedish sociology before the late 1960s. In his latest work, Boalt and his collaborators continue in this empirical tradition with a study of social interaction patterns ("social contacts") among the Swedish professional elite. Since this study is concerned with changes in these patterns over the last century, Boalt frames his data within the theoretical context of professionalization and democratization.

The data collected for the study comprise all memoirs and biographies published after 1904 by or about Swedes alive in 1904 or later. Not surprisingly, these "human documents" provide information about males in the upper class, with class defined in terms of present occupation as well as social background. The study's units of analysis are 6,204 instances of face-to-face interaction ("social contact") between 1,079 persons reported in these documents. Boalt is interested in contacts between occupations and those within occupations. He finds that contacts between occupations have decreased, which the authors believe supports their hypothesis about increased professionalization in Swedish society. It should be noted, however, that the study only provides data on a single dimension of professionalization. More valuable is the analysis of changing interaction patterns within particular professions: clergymen/preachers, politicians, diplomats, military officers, authors, and artists. The authors of this study make good use of the existing literature on these professions to delineate ideological subgroups within them. Within more bureaucratized organizational settings, such as those of diplomats and military officers, ideological subgroups are more likely to overlap with successive age cohorts and thus contribute to relatively incremental forms of social and cultural change. On the other hand, among clergymen, authors, and artists, Boalt and Bergryd depict a more conflictual pattern of change that also reaches beyond professional boundaries. Here the authors find more frequent interactions based on shared political ideology across professions than interactions across ideological lines within professions. This pattern holds after statistical controls for age cohort are entered into the data analysis. The political groupings are conservatives versus liberals, among those born 1841-50, and liberals versus Marxists, among those born 1881-90. Readers familiar with Swedish political history will recognize that these ideological divisions correspond quite closely with those found within political institutions before and after the rise of the Swedish Social Democratic party.

The concept of democratization is used in a narrow sense to refer to changing relations between the class structure and the professions, more specifically the

declining share of members from the upper class who are recruited into the professions. The change turns out to be rather modest, with 47 percent of those born before 1881 coming from upper-class homes, compared to 42 percent among those born after that date. There is also considerable variation across occupations, with journalists, authors, and politicians the most democratized, and four occupations becoming less democratic (e.g., industrialists and actors). The authors freely acknowledge that there are problems of validity, but these problems might have been less if they had presented a careful comparison with other data on mobility patterns in Sweden.

The major shortcomings of this work, however, are theoretical rather than empirical. Reference is made several times to "alternative hypotheses systems" (worked out by Ulla Bergryd) that should provide links between theory and data, but unfortunately these hypotheses are presented as unconnected to any other body of theory. The data's support of one of these systems is consequently left without implications for other theories about professionalization or democratization. The authors implicitly acknowledge this, when they do not relate these hypotheses to other chapters in the book discussing changes within professions, chapters in which the book makes its major empirical contributions. The theoretical vacuum is also not filled by an introductory chapter by two Americans, Herman Lantz and Martin Schultz. Their chapter deals almost exclusively with the U.S. literature on vertical mobility (indicating "democratization of economic life"), at the expense of studies of professionalization.

The book would also have benefitted from more careful editing. Many sentences read as if literally translated from Swedish, and there are also many grammatical and typographical errors.

### **Marginality.**

*By Gino Germani. New Brunswick. Transaction Books, 1980. 98 pp \$5.95.*

*Reviewer: PAT LAUDERDALE, University of Minnesota*

This book focuses on the impact of social, political, and economic development. Germani is interested in broadening the theoretical study of stratification and addressing some of the basic issues in the study of modernization. He initially refers to marginality as the condition of the "persistent segregated, excluded" sector of society, not in terms of any trendy sociological concept of deviance. He proposes to examine marginality at the descriptive, explanatory, and causal levels. His examination produces mixed results.

The concept of marginality has a fairly long research history, primarily grounded in work emerging from Latin American scholars. Many sociologists and political scientists will no doubt be surprised to hear of this book since many argue that the concept of marginality should have been abandoned over a decade ago. In fact, Perlan's *The Myth of Marginality: Urban Poverty and Politics in Rio De Janeiro*, while a valuable contribution to the study of urbanization and poverty, was chastised for using the concept in a social-psychological fashion. Germani, however, presents the concept as "the lack of participation of individuals and groups in those spheres in which, according to determined criteria, they might be expected to



participate." Unfortunately, not until the last section of the book does he clearly explicate the concept and suggest what the criteria might be. In general, the criteria rely heavily on the classic categories of power, prestige, property (wealth for Germani), and his subsequent theoretical outline should prove fruitful for sociologists concerned with economic dependency and stratification. On the other hand, dependency researchers will probably find the methodological outline to be somewhat primitive, and in serious need of modification.

Specifically, some discussions in this book are especially heuristic. For example, the distinctions among poverty, modernization, and marginality are analytically clear, with the added advantage of being grounded in concrete observations in a variety of societies (cf. the author's analysis of the existence of a hidden economy in Southern Italy, or his more general discussion of superfluous employment in socialist countries). In addition, Germani's comparison of marginality in developing and advanced societies suggests that meaningful analogies can be created if we focus on such concepts as labor-intensive technology, surplus labor, distribution/consumption of products, and design typologies using the Weberian dimensions of economic class, strata based on prestige, and differentiation based on power. Obviously, this is no easy task. Germani does provide some important first steps by schematically delineating the categories of marginality in terms of the degree and form of insertion in the productive, consumption, cultural, educational, political, and public services subsystems. Most importantly, his examination of these categories in Argentina goes well beyond the well-worn treatises on the causes of disequilibrium that focus on accelerated demographic growth, insufficient development, and inadequate distribution of income.

While Germani's analysis of Argentina (and Buenos Aires, in particular) is incisive, all of the data reported in his tables are restricted to that one country (and to 1960 census data). Considering the level of discussion prior to the presentation of the tables, it is disappointing to find such a limited empirical sample (Table 1 is a theoretical outline on degree of participation; Tables 2-6 present marginality measures for Argentina, Tables 7-9 also contain indicators of marginality, but only for Buenos Aires). Also, the integration of the tables into the theoretical discussion is weak and there is no reference in the book dated after 1971. While the analytic part of this work still goes beyond most contemporary prattle, there is no doubt it could have been much better if the same care had been extended here as in Germani's earlier *Authoritarianism, Fascism, and National Populism*. *Marginality* would have profited by taking into consideration some of the recent work in immigration (e.g., Alejandro Portes' research) and economic dependency (e.g., Wallerstein's ideas and the methodological advances of some of his followers).

However, despite the fact that this small book ignores recent advances that are central to the idea of marginality, it cannot be ignored. *Marginality* presents some of the most important dimensions for any research project; it identifies most of the central analytic concepts; it provides an agenda for answering core questions; it begins the process of empirical investigation. Put simply, Germani exhibits the rare ability to separate important from trivial issues and that, in itself, is noteworthy. Let us hope that this work on marginality is prolegomenon.

**Identity and Authority: Explorations in the Theory of Society.**

Edited by Roland Robertson and Burkart Holzner. New York: St. Martin's Press, 1980. 311 pp. \$27.50.

Reviewer: ALVIN ROSE, University of Miami

As a sociological specialization, "civilizational sociology" can in some respects be contrasted with traditional "culture-and-personality." Culture-and-personality studies have been concerned with the small primitive society, seen idiosyncratically through the lenses of anthropology and psychoanalysis. But civilizational sociology focuses on the global society, covering a full comparative sweep of the world's civilizations, treated in terms of the most "upper-range," universal propositions that transdisciplinary studies permit.

Robertson and Holzner, were for some years convinced that one of the most severe, widespread crises in civilizations has been in the relation between identity and authority, and that there was far too little theoretical explication and clarification of the nature of this relation—partly because identity had been the province of psychoanalysis, while authority was left to political sociology. They therefore organized a conference on identity and authority at the University of Pittsburgh in 1977. This seven-chapter volume by seven scholars is an outgrowth of that conference.

In this volume the ubiquity of the identity–authority relationship is sometimes argued, sometimes assumed. But analysis of the sources and nature of both identity and authority and of the multivariied forms of their relationship, studied across civilizations and epochs, is what the seven essays in this book are about.

The first three essays, by Holzner and Robertson, by Vytautas Kavolis, and by Rainer C. Baum, conceptualize the relation between identity and authority at the highest possible level of abstraction and generality. The second three essays, by Richard K. Fenn, John H. Marx, and Guy E. Swanson, are limited to Western societies (for Fenn and Marx, to the U.S.). The last chapter, by Robertson, seeks to harmonize the first six essays more closely with the sociological tradition of Durkheim, Weber, Marx, Parsons, and, to some extent, Simmel.

Very much as years ago, Parsons and Shils, in *Toward A General Theory of Action*, provided a systematic frame-of-reference for the study of social action, Holzner and Robertson, in their introductory essay, "Identity and Authority: A Problem Analysis of Processes of Identification and Authorization," seek to map the terrain for the study of the relation between identity and authority.

Their theory of identity is essentially a restatement, in different phraseology of Cooley's and Mead's portrayal of the relation between human nature and the social order, between mind, self, and society. While only the phenomenological experiencing of self can supply the subjective identity requisite called "authenticity," identity (a more or less stable conception of self persisting in time and space) is essentially an objective product of the social structure with its grid of social measurements, based on valuational standards that comprise "the folk mathematics of social measurement." These "folk mathematics" for the social determination of identity provide for each civilization and its subsystems multiple models of identity and varieties of modalities or modes of conduct.

Authority likewise is constrained by and expressed in terms of organiza-

tional styles or codes for the management of power. Swanson's earlier associational/social control (heterarchic) versus his social system/socialization (commensural) regimes as typologies for conceptualizing societal authority are extended by the authors with the concepts of vertical and lateral modes of identification. The various types of relationships between modalities of identity and the forms of power or authority are then derived.

Kavolis, in his "Logics of Selfhood and Modes of Order," interprets the historical record of civilizations as revealing four major modes of identity (as he calls them, "cultural logics of selfhood") and four major forms of order or authority. The four cultural logics of selfhood are (1) a unique patterning of enduring internal coherence (the heroic self), (2) a coincidence of the subjectively experienced self and the social structure or larger society to which the individual is oriented (the "god-in-me" or the spiritual self), (3) a "submerged luminosity" or "hidden savagery" or "real-me" revealed in pure peak experiences (the intensely emotional self), and (4) the casual encounter logic of selfhood (the other-directed, "unturnished" quality-less self). The four forms of order are (1) lawful nature, (2) spontaneous nature, (3) the factory model of order, and (4) "work-of-art".

The life histories of individuals and of civilizations reveal a cycle in the logics of selfhood—from (1) casual encounter, to (2) peak experience, to (3) coincidence logics, to (4) unique pattern. But a particular logic of selfhood is a derivative of specific forms of order (authority). Kavolis proceeds to suggest the several forms which this relationship between identity and authority can take.

Rainer Baum, in "Authority and Identity: A Case For Evolutionary Invariance," uses Parsonsian structural-functionalism to accomplish three important theoretical objectives. In the first place, he reviews the literature of modernization research in order to demonstrate a continuity, an unchanging invariance in both the meaning codes of authority (a political invariance that transcends even such basic trends as economic modernization) and the fundamental forms of individualism, of autonomy, of identity. Second, he derives two invariant forms of authority from Swanson's associational (a negotiated social order flowing from constituent interaction) versus social system or societal (a transcendent solidarity representing the common interests and general will) forms of social control. "Ex parte" and "ex toto" codes of authority are suggested as designations for Swanson's associational and social system forms. These ex parte and ex toto codes are expressed as political regimes in four types of authority relations: consultative authoritarianism (absolute totalitarianisms are unstable charismatic aberrations), corporatism, competitive democracy, and consociational democracy (the Netherlands or Switzerland). Finally, Baum elaborates types of identity as concomitants of the ex parte and ex toto authority codes.

Thus, in Baum's view, except possibly because of conquest or peaceful merger, this closeness-of-fit between codes of authority and forms of identity assures continuity and invariance, a relatively stable social order since "these matched conceptions of obligation to society and to self function in the maintenance and enhancement of societal identity over time."

The essays by Fenn ("Religion, Identity and Authority in the Secular Society"), by Marx ("The Ideological Construction of Post-Modern Identity Models In Contemporary Cultural Movements"), and by Swanson ("A Basis of Authority and Identity in Post-Industrial Society") are delightful, if provocative, reading.

following the broader, more comprehensive macro-analyses. Space limitations preclude even the synopsis each surely warrants.

Robertson's concluding essay, "Aspects of Identity and Authority In Sociological Theory," indicates a sensitivity to the methodological questions that will be raised concerning this volume, among them: (1) aspects of several sociological orientations (phenomenology, structural-functionalism, symbolic interactionism, cultural determinism, etc.) are deliberately and explicitly employed simultaneously, opening the way for charges of eclecticism; (2) multiple-disciplinary frameworks (sociology, attribution theory, ego psychology, history, philosophy, Freudian psychoanalysis, economics, etc.) are employed for unitary explanations; (3) movements across levels of analysis (individual traits, personality systems, social systems, cultural systems, civilizations) are unrestrained; (4) a preoccupation with Weber, Durkheim, Marx, and Parsons may obscure a wider and richer array of sociological alternatives for dealing with civilizational phenomena; and (5) discussion of empirical reality, sociological theories of reality, and sociology-of-knowledge analysis of the sociocultural contexts of sociological theories are intertwined and presumed to be the minimal ingredients of scientific social explanation. These may indeed prove to be serious reservations.

However, if in this kind of inquiry we manage even partially to break through intellectual parochialisms, to move across the boundaries of sociological nationalisms into the genuinely international social science that Robertson calls an inter-civilizational science of society—well, these problems may pale in significance.

Three last comments. The contributors here are seven marvelous scholars, comfortable with movements of thought and with the history of ideas. Their seven essays are seminal, imaginative without the loss of rigor. As both an introduction to civilizational sociology and a contribution to the theory of society, this is a good book.

**Power and Politics in Organizations: The Social Psychology of Conflict, Coalitions, and Bargaining.**

By Samuel B. Bacharach and Edward J. Lawler. San Francisco: Jossey-Bass, 1980. 249 pp. \$15.95.

**Power in Organizations.**

By Jeffrey Pfeffer. Marshfield, Ma.: Pitman, 1981. 391 pp.

*Reviewer:* PETER V. MARSDEN, *University of North Carolina at Chapel Hill*

To judge by the titles, one might expect these two books to have quite a bit in common. In general terms this is so. Both advocate a "political model" of organizations, focusing on purposive activity within organizations by individual actors (persons or subunits) or coalitions composed of such actors. Both are based on the premise that there is a good deal to be learned about organizations by viewing activity within them as political activity, and adapting ideas from political sociology to the study of that activity. Both undertake the considerable service of

synthesizing rather scattered literatures. Some of the propositions the authors arrive at through their analyses are quite similar.

The contents of the books, however, are quite distinct. The subtitle of Bacharach and Lawler's book is more descriptive of its content than the title; they state that their efforts "are directed toward integrating the social psychology of politics with the structural analysis of organizations in order to develop the dimensions of intraorganizational politics." They synthesize an extensive and disparate literature in social psychology on power, influence, bargaining, and coalitions, arriving at over 100 propositions. Many of these hypotheses are framed at a rather abstract level, however, and it is not always clear how studies of organizations are to be informed by them.

Pfeffer's book falls more squarely within organization studies. He indicates that the book was stimulated by a lack of suitable materials for teaching the topic of power in organizations. Pfeffer adopts a "basically sociological" perspective, with the objectives of (1) synthesizing existing knowledge about power in organizations; (2) identifying significant gaps in empirical research; and (3) exploring why people are uneasy with the idea of organizations as political, rather than rational, systems, and making clear the potentially beneficial contributions of power and politics to organizational functioning.

Pfeffer's task is somewhat broader than what Bacharach and Lawler set out for themselves. He begins by contrasting the political model with competing approaches: rational choice models emphasizing the efficient adaptation of means and structures to attain externally determined goals; bureaucratic models emphasizing precedent and procedural rationality; and decision process or "organized anarchy" models that deemphasize the preferences of actors in explaining choice. He then suggests several research tasks necessary for the understanding of organizational choice from a political standpoint. These include setting boundaries, specifying unit actors, and ascertaining the distributions of interests and resources among unit actors, understanding the conditions under which the political model is most likely to be applicable and the bases of interest differentiation among actors, discovering the environmental and structural conditions that provide actors with potential power, and analyzing the aspects of the decision process that enhance or limit the convertibility of such power into the decision outcomes preferred by actors. Extended illustrations of research on organizations from Pfeffer's point of view are given throughout. He closes by considering institutionalization processes that tend to reinforce existing power arrangements, and the implications of a political analysis for organizational performance, organizational design, and the training of managers.

Pfeffer's perspective owes a great deal to the resource dependence view he has used elsewhere in studies of organizations and organization-environment relations. He emphasizes environmental conditions as foundations for the distribution of power within organizations. Such conditions create opportunities for actors to make others dependent on them, to provide critical resources, to cope with uncertainties, or to monopolize knowledge or information, and Pfeffer sees such opportunities as the primary underpinnings of power. To be sure, environmental conditions are not seen as entirely deterministic: manipulating definitions of importance, obfuscating or legitimizing the use of power, building coalitions, using symbols, language, and rituals—all of these also play a part in transforming the

potential power conferred by the environment into successful control over decisions. Nonetheless, Pfeffer states that while "[d]efinitions of the situation are part and parcel of power strategies. . . the realities of the environment ultimately intrude, even if with some delay."

The focus of Bacharach and Lawler's book is more on the social-psychological processes involved in politics than on structural features giving rise to power. They counterpose their emphasis on intraorganizational decision structures and processes to the comparative studies of organizational macro-structures undertaken by Blau, Schoenherr, and others, arguing that such studies embody an apolitical view of organizations. In their early chapters, they review conceptual and empirical work on power, and conclude that power is best treated as a sensitizing construct. As such, it has relational, dependence, and sanctioning formal aspects, and two distinct contents, authority and influence. Authority is rooted in large part in structural position, while knowledge is the principal basis for influence.

Bacharach and Lawler's central contributions lie in five chapters concerned with coalitions and bargaining. They review theories of coalition formation emphasizing size, payoff to members, and ideological consistency, and conclude that coalition formation and politics are most apt to appear in decentralized organizations. (Pfeffer reaches a quite similar conclusion.) They also consider the conditions under which actors will pursue objectives individually ("interest group politics") rather than collectively ("coalition politics"): coalitions are likely when the environment is certain, interests of coalescing actors are consistent across issues, issues are specific rather than general, communication opportunities are ample, and the retaliatory capacity of potential countercoalitions is limited.

Bargaining in organizations, according to Bacharach and Lawler, occurs in a mixed-motive context, i.e., one having both competitive (zero-sum) and cooperative (positive-sum) elements. They distinguish two dimensions of bargaining relations; the balance of competitive (distributive) versus cooperative (integrative) elements and the extent to which the mode of bargaining is formal (explicit) rather than informal (tacit), and conclude that conflicts are most easily resolved when bargaining is integrative and explicit. Their discussion of bargaining tactics considers conditions under which a "tough" bargaining stance will be effective, and approaches to altering the dependency relationship between actors, as well as the alternative process of coercion in bargaining: this occurs largely within the authority content dimension of power, while manipulation of dependence relations is more a matter of influence. Hypotheses suggest that coercion and threats are most effective in formalized, but decentralized organizations.

At the end of their book, Bacharach and Lawler briefly refer to the study of inter-coalition influence networks and argue, I think correctly, that network analysis offers promise as a medium through which their micro-level concerns can be brought up to the macro-level of analysis; they make the important additional point that relational networks must be studied in conjunction with patterns of interest differentiation in order to produce an informative political analysis. The discussion of networks is "cursory" (to use the authors' term), taking up only seven pages, and it does not deal with any of the recent work on networks that has begun to develop the connection between levels of analysis (much of the relevant literature is listed in the appendix of references, which suggests that an earlier version of the book may have given more extensive attention to networks).

Bacharach and Lawler have taken some important first steps toward their goal of integrating insights from various social-psychological theories of politics with the structural analysis of organizations, but there is some distance yet to be travelled. Much of their effort is devoted to translating formal theories or experimental results based on contrived conditions into a form where their applicability to organizations is more clearly evident, but there are only occasional efforts to draw this connection tightly (e.g., the concluding discussion of collective bargaining). For the most part, readers will need to convince themselves that the propositions are useful. Bacharach and Lawler also exhibit rather limited concern with problems of operationalization. Their coalition models, for instance, make predictions based on distributions of resources and (in some cases) preferences among interest groups. There is little discussion, though, of how one is to go about identifying interest groups or empirically assessing distributions of resources in order to put the models to a test outside the laboratory.

Pfeffer has provided a more complete discussion of problems to be dealt with in applying a political perspective to the study of formal organizations, and a better sense of the sorts of studies that might be undertaken within the framework of such a perspective. Both academics and practitioners should find material of interest in Pfeffer's book, his conclusion that "[i]n situations in which technologies are uncertain, preferences are conflicting, perceptions are selective and biased, and information processes are constrained, the model of the effective politician may be an appropriate one both for the individual and for the organization in the long run" will certainly interest proponents of rational or bureaucratic models of organization. There are, however, limitations to this book as well. Evidence for many of its propositions is at present limited, and what is available is based for the most part on studies of public sector organizations. Pfeffer's coverage of politics in organizations is not as theoretically grounded as that of Bacharach and Lawler, and his focus on dependency relations as the primary source of power probably leads to underemphasis on authority structures and coercion.

The idea that organizations can be productively viewed as political orders has been available for some time, certainly since the publication of *A Behavioral Theory of the Firm*, by Cyert and March (1963), and *The Bureaucratic Phenomenon*, by Crozier (1964). It was only in the 1970s that systematic research on power in organizations was initiated, and only recently have analysts attempted book-length treatments of the subject. In *Power in Organizations*, Pfeffer has given a helpful progress report on the development of a political model for organizations from this viewpoint. Those who wish to emphasize the political model in intermediate-level courses on organizations will find the book quite useful. Bacharach and Lawler's book, on the other hand, would be more attractive as part of a course concerned with political sociology in general, because little of its content is specific to organization studies, it is pertinent to the study of coalitions and bargaining in general. Taken together with an increased emphasis on the study of relational networks linking unit actors, these two books provide a promising foundation for the study of organizations as political systems.

***Machine Politics in Transition: Party and Community in Chicago.***

Thomas M. Guterbock. Chicago: University of Chicago Press, 1980. 324 pp. \$20.00.

Reviewer: JOSEPH GALASKIEWICZ, *University of Minnesota*

*Machine Politics in Transition* takes us back to Chicago for an update on Mayor Daley's Democratic political organization. The monograph is a case study of a Northside ward organization. The research began in 1972 and was completed in 1975, just before Daley's death and well before the ascendancy of anti-mayor Jane Byrne. Guterbock lived in the area, worked as an assistant to the Democratic alderman, and conducted interviews with 160 ward residents. As Morris Janowitz notes in the Foreword, the book contributes not only to our understanding of the transformation of urban politics but to a rich and valuable tradition of community-based research which began and continues at the University of Chicago.

The monograph's intent is to test models of machine politics that view the relation between politician and voter as basically an exchange of services or friendship for votes. Surprisingly, Guterbock's data do not lend much support to either exchange model. In the interviews with ward residents, he attempted to document the characteristics of those who were most loyal to the party (in terms of voting behavior) and those who used the services of the ward committeeman, alderman, or precinct captains. To summarize his findings, there was little if any overlap.

As one would expect, loyal Democrats tended to have lower occupational and educational status, to be renters, to have lower bureaucratic competence, to participate less in politics, and to be less involved in the social life of the community. In other words, loyal Democrats tended to be the poorer, more disadvantaged voters in the ward. However, a majority of the people who requested and received services were employed in white-collar occupations and identified themselves with high-status ethnic groups. Users were more likely than nonusers to be homeowners, members of locally based voluntary associations, long-time residents, readers of the community press, and persons in the middle years of the life cycle.

To ferret out his findings, Guterbock presents a lengthy statistical analysis examining the relation between use of services and loyalty to the Democratic organization. He takes into account how satisfied users were, voters' feelings of political efficacy, voters' political participation, type of services used, locus of service (precinct versus ward), respondents' religion, organization membership, personal acquaintance with club officials, etc., and the results are basically the same. Those who used services most were not those who were loyal Democrats.

There are problems with the monograph, but most reflect the fact that this was the author's dissertation research. For example, since this is a case study of one specially selected ward in one city at one point in time, few general inferences can be drawn from its findings. Another problem is that there are very few illustrations, considering that it is an ethnography: e.g., there is little concrete description of the internal structure of the ward club or of how the alderman and committeeman conduct "politicking" among the people of the ward. I suspect that this is due to a pledge of anonymity for respondents. A third problem is that the narrative is parochial at times: the reader needs to be familiar with Chicago politics to



appreciate the subtleties of some arguments. These problems, however, are not very serious, at most, they are a nuisance.

The only truly disturbing (and the most provocative) part of the book is Guterbock's attempt to offer an explanation for why the exchange model did not work. He spends some 46 pages trying to explain why the Organization expends so much effort to provide services to ward residents, while the people who receive the services are not likely to vote the party ticket. He argues that the provision of services demonstrates to voters that the ward club is a legitimate public servant looking out for the public interest. Essentially the services and the attendant publicity win the respect of a constituency increasingly concerned about the provision of public goods rather than private goods. There is support for this thesis in the work of Banfield and others, and Guterbock may have a case.

But let us go back to his data. First, the people who used the services but were not loyal Democrats were mostly middle-class, while those who did not use the services but were loyal Democrats were primarily lower-class. If Guterbock's explanation is correct, then the lower classes voted for the Organization because the alderman and his cronies were doing wonderful things for the middle classes! I always thought Banfield and Wilson had their heads on backward, and now I am convinced. It is the "urban masses" who are willing to sacrifice for the sake of democratic values and the public interest, and the middle class who enjoy the free ride. Personally, I have little trouble with this interpretation, but I doubt that the author and many of my colleagues will feel the way I do.

All in all, this is a provocative monograph and raises some interesting questions for students of urban politics. I believe that it could touch off an interesting debate. The findings present a major challenge to the accepted wisdom on machine politics and should receive careful consideration.

### **Crime and Deviance: A Comparative Perspective.**

*Edited by Graeme R. Newman. Beverly Hills: Sage, 1980. 335 pp. Cloth, \$20.00, paper, \$9.95.*

*Reviewer: GARY LA FREE, University of New Mexico*

Although early criminologists such as Tarde, Lombroso, and Ferri emphasized the necessity of verifying the social facts of crime across cultures and nations, few twentieth-century researchers have taken this task seriously. Newman's edited volume demonstrates at once the importance of comparative criminology and the complexity of the methodological and theoretical obstacles to its development.

In the introductory chapter, Newman and Ferracuti argue that the major tasks of comparative criminology are to apply criminological theories cross-culturally, and to explain diversity among cultures and nations. Unfortunately, they do not provide a comprehensive description of how the eleven essays in the book relate to these major tasks—a problem that is exacerbated by the theoretical and conceptual diversity of the essays they include.

In evaluating the book, I divided the essays into three groups. In the first, the authors address problems of interpreting comparative crime statistics and explore some of the difficulties created by cultural variations in perceptions of

*crime. In the second group, the authors attempt to apply theories of crime and deviance cross-culturally. And in the third, the authors examine issues like terrorism and political crimes where the subject matter more or less requires a comparative focus.*

Essays by Wilkins, Scott and Al-Thakeb, and Salas all examine validity problems in cross-cultural comparisons of crime rates. Wilkins argues that international crime statistics drawn from official sources are of limited utility and suggests instead a broader focus that would attend to the subjective definitions of crime held by members of different cultures. Scott and Al-Thakeb collected data on cross-cultural variations in perceptions of crime, examining public attitudes toward 22 criminal offenses in eight countries. Generally, they found substantial similarity in the perceived seriousness of crimes across nations. But the generalizability of their findings was reduced by the fact that, with the exception of Kuwait and Finland, the countries examined were all Western democracies. Finally, the essay by Salas explores some of the conceptual and methodological difficulties of collecting valid crime data in Marxist countries

The next five essays are all attempts to apply criminology theory cross-culturally. Friday reviews the research literature on youth and delinquency for evidence of cross-cultural commonalities. He argues that youth unemployment and the quality of school and family conditions precipitate delinquency in a wide variety of nations and cultures. Relying mostly on data from Japan, DeVos argues that the cultural background of minority groups interacts with majority group discrimination to produce deviance and crime. Haft-Pieker looks specifically at the cross-cultural appearance of feuds. She interprets feuds as a social response to external threat. Krase and Sagarnn examine the neglected issue of informal social control of deviance. They argue that this topic has special relevance to cross-cultural studies because cultures clearly differ in the extent to which they rely on informal crime controls. Finally, Shoham presents a general, cross-cultural theory of deviance based on relationships between individual personality and social environment

The last three essays in the book address issues that relate crime to emerging international economic and political relationships. Los examines economic crime from a comparative perspective, Ianni and Reuss-Ianni examine organized crime, and Georges-Abeyie examines political crime and terrorism.

The study of comparative criminology has long been neglected by sociologists and the appearance of works such as this one may help to stimulate further research interest in the area. But this book has two flaws that seriously reduce its potential contribution to the field. First, most of the essays are based largely on reviews of prior research or on informed conjecture by the authors. In fact, eight of the ten essays provide no new empirical data at all. DeVos includes data, but they are all taken from his earlier published work. Scott and Al-Thakeb provide original data but their utility is reduced by a design based more on convenience than theory. The non-empirical tone of the book is exemplified by Los, who states that her essay "is bound to be basically of a qualitative nature, since the available statistics and other quantitative data are scarce, unsystematic, and grossly unreliable." But in fact, Los' approach is less qualitative than speculative. She draws her conclusions primarily on the basis of a partial review of the literature and on her own assumptions about crime. A serious shortcoming in the area of

comparative criminology is a general scarcity of solid empirical research, either qualitative or quantitative. This book does little to improve the situation.

A second problem with the book is that despite the title, many of the essays are not really comparative. For example, the article by Salas draws almost entirely on Cuba, DeVos draws mostly on Japan, and Ianni and Reuss-Ianni draw entirely on the United States. It is probably telling that the essays with the broadest comparative focus (e.g., those by Halt-Picker and by Shoman) are also the most speculative.

Overall, the book promises more than it delivers. Granted, the comparative study of crime presents complex empirical and theoretical problems. But this book makes only a half-hearted attempt to define these problems and to address them squarely (a fact that was symbolized for me by a vexing number of typos in the text). Because of these shortcomings, it will probably be of interest only to persons working specifically in the area of comparative criminology. And even here, much of the material (e.g., as in the essays by DeVos, Shoham, and Ianni and Reuss-Ianni) is presented more comprehensively in the original writings of the authors.

In the introductory chapter, Newman and Ferracuti justify the book by claiming that the "research findings and innovative theory from diverse settings, perspective and disciplines [will] add a few more pieces to the complex puzzle that is comparative criminology." The volume does include essays from a wide variety of perspectives and disciplines. Unfortunately though, while it adds more "pieces" to the puzzle, it contributes little to the more important task of beginning to put the pieces together.

### **Hookers, Rounders, and Desk Clerks: The Social Organization of the Hotel Community.**

*By Robert Prus and Stylianos Irimi. Toronto: Gage, 1980. 279 pp. \$9.95*

*Reviewer: CHARLES A. SUNDHOLM, San Jose State University*

The tradition of ethnographic research that was begun at the University of Chicago and carried on by such top-rate ethnographers as Sherri Cavan, Fred Davis, Erving Goffman, John Lofland, and Jacqueline Wiseman has been continued in this fine work. *Hookers, Rounders, and Desk Clerks* provides an intriguing sociological account of an illicit hotel community, as well as an ethnographic description of the social worlds of the prostitutes, pimps, strippers, thieves, barroom staff, and hotel personnel who live out their lives in this setting. Weaving field notes from observations and interviews into clear descriptive generalizations, the authors provide a window on this world that demonstrates the interrelatedness of its inhabitants' roles, their career patterns, and the strategies that they use in routine interactions and in dealing with trouble. Like anthropologists in a primitive community, Prus and Irimi paint an honest picture of the sometimes grim, sometimes violent, and sometimes humorous incidents that make up the underlife of the hotel community.

The book's opening chapter provides a general introduction to the commu-

nity settings under study, most of the material drawn from over two years of participant observation in two similar hotel-lounge buildings in what is described as a quasi-slum area of a large mid-eastern North American city. This was supplemented with observations and interviews at over fifty other bars and hotels, and the authors believe that the cross-section of the activities of prostitutes, pimps, strippers, and "rounders" (who are generalized criminals or jack-of-all-trades "hustlers"), is fairly representative.

The chapters which follow are primarily organized around different kinds of roles and the activities that they include. Chapter 2 focuses on prostitution and examines such related topics as the economic interdependence of the hotel-lounge and the prostitutes; how the women's careers are initiated, maintained, and terminated; how they relate to pimps, customers, hotel staff, and police; and how these hotel and bar prostitutes compare to other kinds of prostitutes such as street, massage parlor, escort service, and call girl prostitutes. Some data begin to move beyond the present sociological literature, while other data confirm present knowledge.

Chapter 3 describes the occupational world of the hotel staff, especially the desk clerk. The focus is on how he keeps order, deals with trouble, protects the prostitutes, and handles the police, as well as dealing with more mundane aspects of his job.

Chapter 4 explores the social situation of the strippers who work the bars adjacent to the hotel area. This chapter provides an analysis of such career problems as getting work, balancing intimacy and social distance with the customers, handling troublesome people in the audience, relating to hotel and bar staff, drifting into prostitution (for some), and dealing with disrespectability. The chapter ends with a comparison to other kinds of stripping—in burlesque theaters and at stag parties, for instance—and a section on other kinds of entertainers—comedians, singers, and musicians.

Chapter 5 describes the bar setting: how employees manage their jobs and attempt to get tips, how customers are handled, and how deviance and fights are dealt with. The chapter also examines various forms of theft that the staff engages in.

Chapter 6 describes the different kinds of patrons of the bar: regulars, "rounders," and "heavies" (someone who is a violent type). Most informative is the discussion of "rounders," generalized criminals who may become involved in pimping, drug-dealing, burglary, theft, con games, and other "hustles." This is an increasingly common type of criminal, and deserves more attention from criminologists.

The final chapter provides a statement of interactionist assumptions and employs them in a brief analytic summary of the prior chapters, ending with a discussion of the illicit hotel as a deviant community which integrates a number of subcultures.

This book offers much in the way of new material for criminology and the sociology of deviance, especially for the topics of deviant communities, prostitution, stripping, and thievery. In addition, it shows how all of these deviant pursuits coalesce and are supported in a symbiotic relation with the legitimate hotel and bar establishment. Any serious student of deviance and good "street ethnography" should read *Hookers, Rounders, and Desk Clerks*.

**Adolescent Sexuality: A Study of Attitudes and Behavior.**

By Helen F. Antonovsky. Lexington, Ma.: Heath, 1980. 162 pp.

Reviewer: J. RICHARD UDRY, *University of North Carolina at Chapel Hill*

This book reports data from a survey of adolescent sexual behavior in Israel. The sexual attitudes and behavior of Jewish Israeli children ages 14–18 and attending secular schools in 1972–73, are described and cross-tabbed by several social variables. Self-administered classroom questionnaires were used, using the classroom and the school as sampling units. All told, more than 5,000 questionnaires were amassed. The main comparisons are between males and females, younger and older children, those from traditional and modern families, and those from kibbutzim compared to non-kibbutzniks. The effects of peers and parents are examined.

The findings are mostly the expected, but there are some surprises. Younger children are more conservative in their attitudes, and have less experience, girls likewise; traditional compared to modern family background likewise—but traditionalism of the family had an effect only on girls' behavior, not boys'. Age for age and sex for sex, the kibbutz respondents had much more liberal attitudes, much more sexual experience, and did not show sex differences in the extent of sexual experience.

The author approached the question in much the same way as previous studies of adolescent sexual behavior have, so she found it possible to provide frequent points of comparison to American and English studies. Her large sample enabled her to use contingency tabular presentation beyond the point where smaller samples would have disintegrated. But, in the tradition of such studies, nothing beyond tabular analysis was attempted. Kinsey may be forgiven because his work antedated the availability of computers. Other students of sexual behavior might want to consider why the questions of interest about adolescent sexual behavior can always be answered by cross-tabulation.

One of the most interesting features of modern Israel is its ethnic diversity. It is therefore somewhat frustrating to find that this book says nothing about ethnic differences in sexual behavior. Arabs were omitted because they go to separate schools. Ethnicity was obscured by a family traditionalism index which compounds parental origin with father's education and the presence of family religious observances. Not a single table examines ethnic patterns specifically.

The faults of this book are the faults of nearly every study of the sexual behavior of young people. How do you find out whether an adolescent's peer group influences his sexual behavior? You ask the adolescent what he thinks most of his friends are doing, you compare what he tells you he is doing with what he tells you his friends are doing, you find that the answers he gives for himself are remarkably similar to those he gives for his friends. (Never mind that his friends might not be doing what he thinks—it's what he thinks they're doing which influences his behavior, *v.e.* are told.) This leads quickly to the conclusion that adolescents are strongly influenced by their peer groups. This conclusion may be true, but you can't prove it with this kind of data.

The same kind of problem exists in the analysis of parent-child communication. Since everybody is in favor of more parent-child communication, it is

comfortable to believe that children will behave more as their parents would like (less sexual behavior) if only their parents would communicate with them more. And some American studies have purported to show this to be the case (at least for girls). So this study's finding that the more communication older teenagers have with their parents the more sexually active they are may be somewhat unsettling. But without some kind of data from the parents, both the comforting and disquieting finding need to be taken with caution.

It is good to have this descriptive study available. It presents useful comparative data. It adds a little to our understanding of the phenomenon of adolescent sexual behavior. But it provides data which others may eventually explore further for that purpose.

### **Rape and Woman's Identity.**

By William B. Sanders. Beverly Hills. Sage, 1980. 184 pp. Cloth, \$18; paper, \$8.95

Reviewer. BARBARA F. RESKIN, *Indiana University*

During 1977 William Sanders accompanied sex-crime detectives while they investigated 100 sexual assaults. He observed interviews with victims and suspects and was privy to investigators' reasoning regarding arrest decisions. Sanders provides 50 brief case excerpts and 13 percentaged tables to support his findings. An introductory chapter reviews the history of rape laws and approaches to rape research. A "dramaturgical perspective" is employed in subsequent chapters on circumstances surrounding the assault, victim-assailant interaction, and the investigative process.

Readers schooled in Goffmanesque analysis may find this application interesting, but its utility depends on whether it yields insights conventional analysis misses. Applying the concept of "breaking frame" to instances in which rapists revealed their initially disguised intentions to victims may explain certain victim responses, but dramaturgical translation more often obscured the data, occasionally masking fallacious reasoning. For example, sex "without proper introductory ritual" supposedly subjects women raped by strangers to charges of promiscuity<sup>1</sup>

The description of how detectives work (e.g., how they classify victims as "good" or "bad") is informative, although related literature is often ignored. But several aspects of design and analysis are disturbing. We are not told how the sample compares to the almost 500 reported assaults during 1977 in the same jurisdiction, although its generalizability affects the validity of policy recommendations. Tables are, with two exceptions, univariate, and questions of spuriousness and causal order are never addressed. Two chapters are reprehensible. In the chapter on the impact of rape on women's identity ("A Woman's Reputation"), Sanders argues from stereotypes ("silly, . . . hysterical women," for instance, or the expression, "a man of his word,") that because women are seen as lacking attributes of "character"—courage, integrity, composure, etc.—their reputations depend largely on their self-control in sexual relations, "the tiny aspect that society has given women as a center of self." But because most women do not vigorously resist assailants, Sanders concludes, rape robs victims of their sexual control and hence

the small claim they might lay to "character." He does not blame victims, but this does not render his thesis more scholarly, based as it is on sexist stereotypes and unsupported assumptions.

In the penultimate chapter, Sanders musters his case for resistance. In a psychological analysis of rapists that reads like a Dr. George Crane column, he concludes (from an Australian study that purportedly found that the type of men who frequent brothels resembled rapists) that "the rapist is a wimp, since he has to go to a prostitute for sex." Readers are also provided a cross-tabulation of victim resistance by physical harm. Although it lacks marginal totals, percentages of 33 and 67 justifiably arouse suspicion regarding the Ns, and, in fact the experience of three women who fought back without serious physical injury provides the basis for Sanders' plea for victim resistance. (No explanation is offered for the absence of two additional victims classified earlier as having fought, at least one of whom incurred substantial injury.) From a flawed interpretation of the table and despite a significant (although inappropriate) chi-square test, Sanders concludes that resistance and harm are unrelated. But the relationship is not at all clearcut, relevant factors such as weapon use or number of assailants are not considered, and the question of causal order is ignored.

These are serious problems because the book's major thesis is that resistance reduces rape without increasing the risk of injury. Related—but uncited—research on rape avoidance (that of Pauline Bart, for example) has reported associations between resistance and *both* rape avoidance and injury. Studies carefully appraising the conditions under which resistance is most effective and least dangerous are essential. But scientists who use their data to offer advice on what are literally life and death issues should be on very solid ground. This study is not, and to the extent that it convinces readers to resist, it does a disservice. Sage editor Gilbert Geis is correct in his Foreword, that "support for some of Sanders' views may be arguable" and that, because of their importance, these matters demand thorough consideration and "some better conclusions." I wonder, then, why Geis and Sage did not wait for a responsible, scholarly examination of this important topic.

### **Unequal Care: A Case Study of Interorganizational Relations in Health Care.**

By Murray Milner, Jr. New York: Columbia University Press, 1980. 180 pp. \$22.50.

Reviewer: DENNIS MILETI, Colorado State University

On what has become too rare an occasion, something comes along to remind me why I became interested in sociology. Each of these experiences evokes a common emotion—a deep-seated excitement over reading a statement of what seems intuitively obvious—after the reading is done—that is being stated in sociological jargon for what could be the first time. Milner has produced my most recent rejuvenation, I encourage others to give themselves a chance to share my enthusiasm about his work.

In simple terms, the text offers answers to some pretty basic questions: Why are some organizations linked while others are not? Why do links vary in terms of coordination and conflict? Why is inequality among organizations perpetuated? The answers are innovative and contribute much to basic understanding. In this

regard, the book sells itself quite short. Although the title suggests, correctly, that it should be of interest to those concerned with health care, it is just as important for all students of interorganizational relations. More than that: it is worthwhile reading for any sociologist. The basic conclusion is simple. There have to be poor (low quality hospitals) for there to be rich (high quality hospitals), and the latter have a vested, selfish interest in keeping the former around as a way to remain the elite. Milner labels the phenomenon "symbiotic inequality." The idea is familiar—didn't Marx say it about classes? Who was it that said it about nations? The point is that the application of the perspective to interorganizational relations works beautifully. I'd wager that in a few years someone will produce a path model to support the view quantitatively.

Milner's method is qualitative, and in some places even literary. There are interviewee responses quoted throughout the work. The skill and care exercised to implement a sociological perspective and analysis are exemplary. The system under analysis is placed squarely in its broader social context, and the conclusions are pertinent to understanding how and why inequality is perpetuated, despite all attempts to eliminate it.

**From Student to Nurse: A Longitudinal Study of Socialization.**

By Ida Harper Simpson. New York. Cambridge University Press, 1979. 266 pp. Cloth, \$14.95; paper, \$4.95.

Reviewer: MADELINE H. SCHMITT, *University of Rochester*

The main objective of this monograph is to develop a model of professional socialization that synthesizes the induction approach, typified by Merton's *The Student Physician*, and the reaction approach, typified by Becker, Geer, Hughes, and Strauss' *Boys in White*. In the induction approach students are eager incorporators of professional roles and attitudes pervading the educational program and role modelled by the faculty in it. In the reaction approach the student is an active participant responding to conflicting pressures within and beyond the professional school. The author applies the model to a case study of the socialization experience of several classes of nursing students at Duke University during the early 1960s.

Making use of questionnaires, the author measures student "orientations." These include the extent to which students are inducted into the nursing faculty's perspective, measured with four scales comprised of questions on holistic perceptions of patients, individualized patient care, collegialism, and identification with the roles of supervisor and administrator. They also include the students' reactions as measured with questions about occupational commitment, occupational attraction, and identification with the status of nurse.

A key part of the monograph is a discussion of the effects of major shifts in the curriculum from year to year. The findings, in the form of mean scores, show overall increases in orientations to collegialism and administration and supervision, and in status identification and commitment to the role of the nurse, and overall declines in student orientations toward holistic views of patients, individualized patient care, and attraction to nursing from the time of entry to one year past graduation. Not all of the changes occur together. It is argued that the timing of the



changes in specific orientations most strongly reflects particular shifts in the curriculum. For example, students are described as initially sharing the ideals of faculty, showing positive orientations toward holism and individualized care. The author argues that exposure to bureaucratically organized nursing care in the hospital leads to a decline in these orientations.

Making use of Pearson correlations, the author examines the relations between a number of other variables and students' orientations. Very few significant correlations are found. Some of the most important and unconvincingly interpreted findings focus on anticipated future work and family plans. Understandably, the 23 percent of the students who never expect to work after graduation scored lowest on most of the orientation questions. Those who expected to work full time (8 percent) had the highest mean scores on collegialism and administration and supervision. However, they also showed the most dramatic decline in commitment and attraction to the profession from freshman year to one year after graduation. Most of the sample expected to alternate family life and work. The questionnaire data do not permit exploration of these very different types of response to the occupation.

The author concludes that the most significant influence on the crystallization and continuity of professional orientations is the exposure to the skills and knowledge of the profession learned in the clinical training. These orientations may contradict the official ideology—for instance, of holism and individualization—while it may reinforce others such as collegialism. Parts of this book are a disappointment. The synthetic model and the longitudinal study design promise new insights into the relative impact of professional education and the work environment on professional orientations. However, the choice of a nursing educational program as the empirical focus complicates the analysis of this relationship, since the analysis of women's occupations necessarily includes motivational factors unrelated to professional work per se. This issue is not fully confronted in the conceptualization or application of the model.

The study is weakened further by problems of measurement, analysis, and interpretation. An important example of this are the measures of holistic views and individualized views of patient care. Although the full list of items is not provided, examples of questions used raise questions about validity. "Holism" is defined by phrases such as "acting friendly and understanding." However, in nursing it means seeing sickness in the context of patients' developmental, psychological, social, physiological, and environmental situation. "Individualized care" is represented by questions focusing on personal service (e.g., getting the patient a glass of water, walking with the patient). However, in nursing "individualized" has a conceptual relation to "holistic." It means fitting the nursing care delivered to the total situation of particular patients.

The author's lack of understanding of these ideal role images is reflected in her assumption that the ideals held by the faculty are the same as entering students' undifferentiated aspirations to help people through close personal relationships. Other accounts of the renewed interest in the "tenderness" dimension of the nurse's role and its incorporation into the curriculum via extensive application of social science ideas during the 1960s properly treat this issue more complexly.

A further complication involves the confounding of the measurement of holism with reactions of nursing students to the work situation. Because of the

wording of each item students are forced to make mutually exclusive choices between holism and commitment to bureaucratic routine. Declines in holism, which reflect increasing agreement with "the need to follow prescribed routine" and "instructions in the chart" are interpreted as a loss of student idealism: its simultaneous continuance is not a possibility open to testing.

The fluctuations in the data on holism and individualized care which, to this reviewer, reflect a lack of reliability and validity, are the bases on which the author makes key interpretations of student movement away from the role ideals espoused by the faculty.

The exclusive reliance on questionnaire data prevents discovery of these kinds of error in measurement, analysis, and interpretation, and precludes a richer understanding of the pushes and pulls undoubtedly felt by nursing students.

While the book is flawed, its approach does suggest new possibilities for studies of nursing student socialization, taking into account important changes in nursing since these data were collected in the 1960s. A new emphasis on technical proficiency has been integrated into the ideology of nurse educators. Even more important, organizational experiments such as the "unification" and primary nursing models are underway which purport to close the gap between educational ideology and nursing practice. Examination of the orientations of students exposed to this educational-clinical milieu compared to the more traditional education would go a long way toward unraveling the whole set of relationships. Such studies would, of course, have to address the impact of renewed feminism on nurses' work aspirations and orientations.

### **Community Development in America.**

*Edited by James A. Christensen and Jerry W. Robinson, Jr. Ames. Iowa State University Press, 1980. 245 pp. \$8.50*

### **Community Development: Theory and Method of Planned Change.**

*Edited by Dan A. Chekki. New York: Advent Books, 1979. 258 pp. \$18.00*

### **Participatory Democracy in Action: International Profiles in Community Development.**

*Edited by Dan A. Chekki. New York: Advent Books, 1979. 306 pp. \$20.00*

*Reviewer: CARLTON R. SOLLIE, Mississippi State University*

The first of these books is described as "the first comprehensive assessment of theory, research, and the practice of community developing in America." Its preparation was sponsored by the Community Development Society, to celebrate its first ten years of existence.

Beginning with a chapter that deals with the major concepts and a definition of community development as a science and profession, the book moves through several chapters to an essay by Spiegel on new directions, which identifies one of the major problems in community development, the fact that community development cannot by itself redirect macrosocial trends. Intervening chapters provide information about the history of community development in America, various

themes found in a content analysis of articles appearing in the *Journal of the Community Development Society*, professionals in community development, teaching community development, research issues, evaluation, and theory

Perhaps the most informative chapter, by Christensen, uses a content analysis of journal articles to identify three themes of community development: self-help, technical assistance, and conflict. Each theme is then dealt with in a separate chapter, the result being detailed descriptions of each approach to community development.

Brief essays by seven authors are found in Chapter 7, where the question of what community development professionals do is answered in various ways. Warner, in Chapter 8, discusses the teaching of community development and identifies some of the concepts and skills that should be included in a community development educational program.

Three areas of activity in need of research are also identified. These, as discussed in Chapter 9, are needs assessment, community services, and economic analysis focused on the impacts of industrialization and population change.

Evaluation of community development is very difficult, because the process is extremely complex. However, evaluation research is becoming more and more important. This complexity and this importance lead Voth, Bothereau, and Cohen to suggest an eclectic approach.

The complex nature of community development is also one of the main reasons for the lack of clearly articulated theory. Composed of several social, economic, and behavioral perspectives, the field of community development has no single disciplinary core. Development of theory, therefore, should begin with a model or conceptual paradigm, and Blakely suggests three: a "social facts" paradigm, a "social definition" paradigm, and a "social behavior" paradigm.

The second book reviewed here focuses on theory and method of planned change. Contributors write from the perspective of community development as a process of planned change.

Following an informative introduction, the editor presents a brief survey of theoretical formulations to begin the section on theory. Cary examines the present state of community development theory, describing it as "inadequate, out of date, and incomplete." The state of practice is in no better shape than the state of theory, and one of the main reasons for this is a communication gap between the two.

Concepts of community work as practiced in Britain are discussed in a chapter by Muriel Smith, and her essay is followed by a description of three models of community development by Dasgupta. Two of the models refer to developing and developed countries while the third projects the idea of a "community society" in which the "clients of welfare will also be the decision-makers in the system; the producers of welfare will also be its consumers; and third, the client of welfare, or of development, will not be the individual but a conglomeration of individuals."

Kaufman's "activity field" is offered as a perspective for community development. The activity field is seen as "structure in progress." Mookerjee's essay is in part an elaboration of one of Kaufman's three main concepts, association. Mookerjee writes of "balanced development," i.e., an integrated development of community life that grows out of the felt needs of individuals in the local society.

The theory section of this book concludes with an assertion by Head that one



of the most pressing needs in community development theory today is a vision of a humane and meaningful future.

Six essays are found under the heading of methods-strategies. Dimock identifies the two major problems of community development in Canada as that of outside workers' making decisions about what would be best for the community, and that of organizations' losing touch with programs they sponsor, finding them threatening, and abandoning them. "Systems improvement research" is suggested as a strategy to overcome these problems. In a similar vein, Stinson argues for "action research" as a means of increasing the acceptability and utilization of research in community action.

Baldock's concern is the achievement of "popular growth" through community work and action, believing that local problems cannot be solved without change in the way the rest of society operates. Shragge's argument is of the same nature. Campfens argues for "centers of power" at the local level, encouraged by government, as a means of facilitating local participation.

The third book focuses on the interrelatedness between democratic participation and community development. There can be no doubt about the philosophical orientation of the book, it is a strong argument for "rejuvenation of institutions, policies, and programs" through grass-roots involvement in social processes. Community development is seen as the most appropriate context for this kind of action.

The significance of voluntary associations in community development is seen in the fact that some 600 or so INGO's (International Non-Government Organizations) have consultative status with the Economic and Social Council of the United Nations.

Divided into seven sections, including the Introduction and an Epilogue, the book contains cross-national profiles, case studies, and commentaries. Specific topics dealt with by the various contributors include community planning organizations, community leisure planning, professions and the community, the emergence of executive ombudsmen to counteract government unresponsiveness, mass media in development, community self-help, citizen participation, and issues and trends.

For those interested in acquiring basic knowledge about community development these books would be useful. Each is a collection of essays contributed by people from such varied backgrounds as government, private enterprises, and university teaching, research, and service. An apparent commitment of the editor and contributors is "keeping participating democracy alive," as Francis J. Bregha puts it in the Foreword to *Participating Democracy in Action*.

Those familiar with the literature will be disappointed if they read these books expecting to find anything significantly new with respect to theory, method or practice. Nevertheless, all three books should be considered by students of community development. Together, they make a timely contribution, a comprehensive survey of the state of the art in community development.

**Racism and Political Action in Britain.**

*Edited by Robert Miles and Anne Phizacklea* London & Boston: Routledge & Kegan Paul, 1979. 246 pp. Cloth, \$18.00, paper, \$9.00.

*Reviewer* JOHN STONE, Goldsmith's College, University of London

Class, not race, has been the single most important division in British politics throughout the twentieth century, but this does not mean that the "racial factor" has been insignificant: race relations have often occupied a prominent place in political debate and conflict. Such arguments have usually focused on the question of "immigration," beginning with the agitation against Irish and Jewish migrants in the late nineteenth century. This was followed by the antics of the various fascist groups during the 1930s and, in the postwar period, the debate over Commonwealth immigration, the speeches of Enoch Powell (the first major politician since Mosley to make an open appeal to the racist vote) and the activities of the National Front.

Miles and Phizacklea's book consists of a collection of ten essays, based loosely on a conference held in September 1977 under the auspices of the S.S.R.C. Research Unit on Ethnic Relations, and brings together a variety of perspectives on the question of racism and political action in British society. In their introductory chapter, the editors emphasize the degree of historical continuity from the xenophobia of the nineteenth century to the racism of contemporary Britain. They also develop one of the major themes of the volume, and a critical issue in the analysis of race relations: the interplay between class consciousness and race consciousness. The potential conflict between these two types of identification was appreciated by Marx and Engels in their discussion of the impact of the Irish on the English working classes, as well as by Liberal writers who anticipated the present day concern with whether there is a distinctively *working-class* racism. How racism and nationalism have cut across class affiliations is explored in many of the contributions to this volume.

Two early chapters are concerned with the historical role of the extremes of the political spectrum in the development of racial politics. Nugent and King demonstrate how ethnic minorities have been used as convenient scapegoats by a succession of fascist and neo-fascist organizations—the British Brothers League before the First World War, the British Union of Fascists in the interwar years, and the National Front in the postwar period. Jews have gradually changed from "low level" to "high level" scapegoats for the extreme Right, as they have moved from the status of a poor refugee group into the solid middle classes. Their place in the front line of racist abuse has been taken over by the West Indian and South Asian immigrants who arrived in significant numbers from the 1950s onwards. The authors are rightly critical of the thesis advocated by certain political sociologists, like Lipset and Raab, who suggest that Britain is "immune" from outbreaks of irrational extremism because of an allegedly tolerant political culture and a deferential working class. Caroline Knowles examines divisions within the Left over the interpretation of anti-Semitism during the critical years of 1934–36, the height of Mosley's fascist activity in the East End of London. While the official Labour party adopted a pluralistic standpoint, regarding the Jews as a separate ethnic group that needed to be protected against persecution, the Left Wing of the

party (and the Communist party, too) defined Jews, not as a separate ethnic category, but simply as a section of the working class. Defense of the working-class Jew was an integral part of the defense of the working class as a whole against fascism. Thus, anti-Semitism was seen as a mechanism by which the ruling class sought to divide and rule the working class by generating "false consciousness," and it should not be interpreted as a distinctively working-class phenomenon.

John Rex continues this exploration of ethnic separatism and class conflict in relation to recent black immigrant minorities in Britain. Rejecting oversimple analogies with the United States, Rex argues that the "inclusion process" in Britain involves, for the great majority of immigrant groups, an attempt to join the central institutions of the indigenous working classes. As a result, membership in the trade unions and access to public housing takes on a significance that is quite different from comparable circumstances in America. However, blacks do suffer from extensive discrimination, and this has resulted in the growth of black power organizations which act, according to Rex, as the "functional equivalent of the working class movement." Rex asserts that "immigrant politics is not simply the politics of . . . class struggle within capitalism [but] the political formation of an immigrant underclass." This politics of "defensive confrontation" suggests that blacks are "structurally distinct" from the majority of the working class, a conclusion that will raise eyebrows in vulgar Marxist circles, but will provoke little controversy elsewhere. Miles and Phizacklea continue the focus on working-class racism with some empirical research based on the deprived, inner-city area of Willesden in northwest London. Their small sample produces convincing evidence of the pervasiveness of working-class racism and demonstrates—what might be called the Myrdal mistake—that "racist beliefs can happily coexist with even high levels of class consciousness." This leads to a classical Orwellian situation where some workers, the white ones, are "more equal" than others.

In a chapter on the National Front, Taylor shows that its electoral support is related less to the absolute number of blacks in a constituency and more to subjective fears of black "invasion" of particular neighborhoods. This "threat" hypothesis is explored in further detail by Husbands in an interesting cross-national comparison between National Front voters and the supporters of George Wallace. A case study of the development of race and local politics in Bradford is followed by Robin Ward's paper describing a situation of racial cooperation during a slum clearance program in Manchester. Setting out the conditions of interracial partnership is an important corrective to the general obsession with the politics of racial conflict. The book concludes with a masterly survey by Michael Banton outlining the nature of English nationalism and emphasizing the considerable local variations in the process of racial categorization. Banton's article shows how the racial predictions of Gilbert Murray, a Glasgow University Professor of Greek at the turn of the century, proved to be incorrect. It remains to be seen whether the prophecies of another classical scholar, Enoch Powell, turn out to be equally false. In any event, Miles and Phizacklea's useful collection provides a better understanding of the dynamics of racial politics in Britain.

**Jackson, Mississippi: An American Chronicle of Struggle and Schism.**

By John R. Salter, Jr. Hicksville, N.Y.: Exposition Press, 1979. 248 pp. \$10.00

Reviewer: JAY WEINSTEIN, Georgia Institute of Technology

The tragic murder of Medgar Evers in Jackson, Mississippi, on June 11, 1963, marked a turning point in the civil rights movement, for Jackson, the state, the South, and, indeed, the entire nation. In the years between the 1954 Supreme Court school desegregation decision and Evers' death, the course of the movement could be described as one of slow but steady acceleration, consolidation of effort and purpose, and a broadening of the base of participation to include more whites and blacks of all socioeconomic strata, urban and rural dwellers, and residents of virtually every state. After that June day, fission occurred along many fault-lines: a proliferation of groups and organizations serving special interests and constituencies, often in overt competition (if not conflict) with one another; segregation along racial lines within the movement, and (especially with Martin Luther King's Poor People's March late that summer) a shift in focus from the local level to the national—including the active involvement of the federal executive and judicial branches and the national media, and national control at the grass roots.

In *Jackson, Mississippi*, John Salter provides a sympathetic, carefully reasoned, and highly readable first-person sociological account of the events surrounding Evers' murder and its actual and symbolic connections with this transition in the civil rights movement. Salter skillfully details the roles played by key actors and organizations—including his own role as participant, observer, and, for a time, lightning rod for the bad feelings of local political leaders, Jackson's notorious "hoodlum element," and even some people within the movement itself. The book's uniqueness as a chronicle and a document of the times, and also the valuable sociological observations it contains (often implied in a narrative rather than underscored as law or generalization), make it of considerable value to all who have an interest in the 1960s, social movements, and U.S. social history. It is certainly not written as a textbook, but I suspect that it would prove an immensely popular addition to undergraduate or graduate courses.

Salter, whose father was a Micamac Penobscot Indian, clearly (and self-consciously) appreciates the oral, story-telling tradition in social science. He has kept students fascinated with his anecdotal lectures in colleges and universities throughout the country—at Goddard College, Coe College, and the University of Iowa, among others—and in this, his first book, he has succeeded in the very difficult task of putting part of "his" story on paper. The fact that Salter entered the scene at Jackson when, in 1961, he came to Tougaloo College as a sociologist, ought to spark the interest of all who were trying to understand society in a dispassionate, scientific manner during the anything-but-dispassionate era of the 1960s. All the issues of social commitment versus truth-seeking, neutrality, of *Verstehende* versus natural-science models of social science method, are raised and argued in Salter's *post-hoc* analysis. This is especially clear when Salter recounts his actions and reflections during the black boycott of Jackson's downtown, the sit-in in Woolworth's, the massive demonstration on the day of Evers' funeral, and the territorial battles between NAACP, SNCC, SCLC, and CORE which crystallized on that day.

It was also in the early 1960s, it may be recalled, that C. Wright Mills was

instructing his students that "no social study that does not come back to the problem of biography, history, and of their intersection within society has completed its intellectual journey." Though many social scientists since that time have professed to endorse this vision of their work, few have been able to put it into practice. In John Salter's *Jackson, Mississippi*, we have a rare instance in which an intellectual journey into some of the most beautiful and some of the most ugly regions of U.S. and southern society has at least the semblance of the completeness of which Mills spoke. John Salter happened to be "where it's at," in all of the glorious and frightening senses of that movement expression. His biography intersected with history, at least for a moment, in an incredibly dramatic way: he is sociologist enough to want to record this fact, and human enough to realize that he had to act, react, learn, and grow with it. The result is, as the book's subtitle puts it, a chronicle of struggle and schism, one in which we not only learn about the structure of the Jackson movement (and, by implication, of the movement in general), but meet the "little" people involved—like the farmer in the Delta region whose family of twelve shared one pancake for dinner and the now well-known sociologist Joyce Ladner. We also meet the "big" people: James Meredith, Dick Gregory, William Kuntzler, Floyd McKissick, Ross Barnett, Mayor Alan Thompson, John and Robert Kennedy, Dr. King, and of course Evers, all of whom converged on once-sleepy and entrenched Jackson (the president and attorney-general by long-distance phone) during a few intense weeks in the hot summer of 1963. The fact that the last four of these men were all killed within a few years of that summer for reasons America does not yet understand is enough (but not the only) justification for the appearance of this chronicle nearly two decades later. John Salter is to be congratulated for his efforts, and potential readers are to be encouraged to engage—and, when necessary, to take issue with—the views of a uniquely situated commentator on a uniquely poignant period in our history.

#### **Anti-Semitism in America.**

By Harold E. Quinley and Charles Y. Glock. New York: Free Press, 1979. 237 pp. \$9.95.

Reviewer: CELIA S. HELLER, Hunter College

The authors have admirably fulfilled the goals they set for themselves in producing this important volume. As they explain the origin of the study, it followed a postwar period when not only scholars but also the Anti-Defamation League (founded in 1912 to fight anti-Semitism) largely ignored anti-Semitic attitudes and behavior. By the end of the 1950s, "ADL leaders began to feel that violence against Jewish persons and property had become a thing of the past." Then came the rude awakening. At the beginning of 1960 more than 600 anti-Semitic incidents occurred in the U.S.: "Synagogues . . . suddenly bore the crude epithets of bigotry and the ubiquitous swastika." Jewish cemeteries, stores, and homes were subject to anti-Jewish vandalism.

These incidents led the ADL to return to some of the old questions about *anti-Semitism in the U.S.* It resulted in the ADL's commissioning a new social science inquiry. The handsome figure of half-a-million dollars was provided, and the California Research Program on Patterns of American Prejudice was set up to



conduct a series of research projects, interrelated, but each designed to illustrate a different facet of the phenomenon of anti-Semitism. These were to result in a series of technical and scholarly publications. Fortunately, the project also called for a "wrap-up" volume, summarizing the findings of these studies (conducted from January 1963 to August 1975), and written in a form that would attract a larger reading public. That volume, this book, discusses the nature, extent, social location, and causes of anti-Semitic prejudice. The weakest part is that dealing with causes, since that question does not lend itself to the questionnaire method which dominates the studies.

Let us then begin with the findings. Before World War II, anti-Semitism was both virulent and open (prominent spokesmen attacked Jews viciously and found support among millions of Americans) but this was no longer true in the 1960s and the early 1970s. (I specify these years because we do not know to what extent this is still true today, a period of inflation, high rates of unemployment, and frightening violence.)

It is noteworthy that very few non-Jews favored discrimination against Jews in housing, employment, college admissions, or hotels and resorts. Still, the studies show that many Americans held the stereotypic images of Jews traditionally associated with anti-Semitism. Jews are deceitful, dishonest in business, conceited, clannish, pushy, and they control the mass media and banks. Some harbored newer stereotypes, such as that American Jews are more loyal to Israel than to the U.S. Non-Jews also resented it when Christmas carols were not allowed in the public schools.

A concern with anti-Jewish stereotypes should not obscure the fact that most Americans held positive attitudes toward Jews. They thought them friendly, warm, intelligent, hard-working, ambitious, successful, religious, and family oriented. I doubt that the same results would be found if the study were replicated in some European democracies, such as France, let alone Germany.

Like many other attitudes, negative and positive stereotypes of Jews vary with class. In the words of the authors, "anti-Semitism is found among all kinds of Americans, the rich and the poor, the young and the old. However, it is most prevalent among the lower socioeconomic levels" and among older people. The authors show that this is due to lower educational attainments. When education is held constant, the differences between working-class and middle-class, as well as between young and old, disappear. No evidence was found to support the psychological theories of prejudice, including those which attribute prejudice to "authoritarian personality."

Since prejudice varies with education, the authors give much attention to education as an explanatory variable. Unfortunately, they do not point to the fact that this is a distinctively American pattern. In my own work on Jews in inter-war Poland, I have found that anti-Semitism was stronger in the middle class than in the working class and that the universities were hotbeds of the most vicious anti-Semitism. The educated were more anti-Semitic than the lower strata, among whom illiteracy was very high.

This leads me to the criticism that this study would have profited from a comparative perspective; it completely lacks one. My other major criticism is that the authors' discussion of political anti-Semitism is confined to rightist groups and completely omits the left-wing anti-Semitism which often parades under the label

of "anti-Zionism." From my own study of this phenomenon, I would venture that—while it is possible to be anti-Zionist without being anti-Semitic—in most cases the so-called anti-Zionism of the political left is camouflaged anti-Semitism. Its birthplace was the Soviet Union, from which it moved to Soviet satellites. In Poland, for instance, an organized anti-Zionist campaign was launched in 1968; its propaganda contained all the poisonous themes that characterized Polish anti-Semitism of the 1930s.

Attempting to predict the future of anti-Semitism in the U.S., Quinley and Glock plausibly state that "Among the conditions that might generate political anti-Semitism are a severe economic recession or a war in the Middle East." I consider preposterous, however, their assertion that the nomination of a Jew as a presidential candidate "would also be likely to produce political anti-Semitism." Their own data, on the contrary, tell us that the majority of the American people would not object to a Jewish candidate.

**In Gods We Trust: New Patterns of Religious Pluralism in America.**

*Edited by Thomas Robbins and Dick Anthony. New Brunswick: Transaction Books, 1981. 338 pp. \$8.95.*

*Reviewer:* WILLIAM M. NEWMAN, *University of Connecticut*

The reigning scenario among sociologists of religion is that a general breakdown of normative institutions in American society is measurable within religious institutions in at least two ways. Traditional mainstream institutions and the "civil religion" they allegedly support have declined in their social importance; ultra-conservative versions of the mainstream and new forms of religion have emerged to fill this normative vacuum. *In Gods We Trust* is a collection of seventeen essays, a dozen of them reprinted from *Society* magazine, that attempt to illustrate, examine, and explain these trends. An introductory essay by Anthony and Robbins outlines these basic themes and does an admirable job of explaining how each of the essays in the volume fits into the issues being debated in the field today. In terms of both themes and authorships this volume is genuinely representative of the debates that have filled the pages of professional journals, the forums at annual meetings, and the pages of major case studies over the past decade or so.

The book is divided into five major sections. The first section, on "Religious Ferment and Cultural Transformation," contains general theoretical essays by Robert Bellah, Benton Johnson, and Charles Glock and Thomas Miazza. Johnson's essay is one of the few in this book not previously published. Together, these three essays examine the place of religion in the cultural developments of the late 1960s and the 1970s. The second section, on "Disenchantment and Renewal in Mainline Traditions," contains four essays that focus on the softening of liberal Christianity and the move to orthodoxy and fundamentalism. Contributors are W. Clark Root, Andrew Greeley, Samuel Heilman and co-authors James Richardson, Mary W. Stewart and Robert Simmonds.

Part Three contains an assortment of five case studies illustrating the "new pluralism" in the book's subtitle. The respective authors and topics are Irving L. Horowitz on the Unification Church, John Hall on the Jonestown tragedy, Robbin

and Anthony on Meher Baba, Donald Stone on the Human Potential Movement, and Robert Wuthnow on the political aspects of membership in such groups. The fourth set of essays in the book explores brainwashing as an explanation of why people join these types of movements. Contributors are Anthony and Robbins, Shupe and Bromley, and Judy Solomon. Finally, two essays address the "Decline of Community" in American life as a causal element in the popularity of these new religious movements. Irvin Doress and Jack N. Porter discuss "Kids in Cults" and George Hillery provides a general theory of organizational types. The book concludes with an extensive bibliography, serving as a reference bibliography for all the essays in the volume.

As I have already indicated, this collection is genuinely representative of the authors and issues that have absorbed much of the energy of sociologists of religion over the past decade or so. The editors' introduction is first-rate, and the essays maintain a fairly consistent level of quality throughout. A volume such as this makes an excellent reference work for smaller academic libraries and even public libraries. It provides a great corrective for so much of the trash that has been published on "cults" over the past decade. (The absence of an index weakens the potential reference function of the book.) As a classroom Reader it obviously lacks some of the more familiar "work-horse" essays that examine basic issues in the field, and which are essential for a first course in the sociology of religion. Thus, while the book is of high quality and was thoughtfully assembled, I'm not entirely sure how to use it. At the very least sociologists of religion can take pride in this volume as a statement to the discipline about what many of us have been doing for the past decade.

### **The New Vigilantes.**

By Anson D. Shupe, Jr., and David G. Bromley. Beverly Hills: Sage, 1980. 267 pp. Cloth, \$18.00, paper, \$8.95.

Reviewer: WILLIAM SIMS BAINBRIDGE, *University of Washington*

This is a companion to *"Moonies" in America* from the same authors and publisher. While the earlier book examined The Unification Church, the cult founded by the Rev. Sun M. Moon, this book scans the attempt to form a movement in opposition to the Moonies and other controversial new religious groups. Unfortunately, the sequel reads like the residue from the first book, constituting neither a theoretical essay nor an empirical study of consequence in its own right.

Perhaps because the authors relied on a "snowball" technique, working largely from an anti-cult entrepreneur who happened to live near their university, whole sections and aspects of the anti-cult crusade are missed, for example the federal government's war with Scientology. Since the anti-cult "movement" was in fact a series of failed attempts to organize a true movement out of spontaneous collective behavior, questions of the representativeness of the data collected are important. The authors say their methods included participant observation, formal interviews, collecting organizational documents, and trips to the library. But this reader, at least, is left with the strong impression that both the kinds of methods

and the amount of work invested in them fell far short of the requirements of a good scientific study of the topic.

One lesson that can be drawn from this book and other recent volumes like it is that there is no such thing as a sociological methodology of "participant observation." There exists no agreed-on set of criteria for conducting field research in sociology. Anthropologists do have a methodology of "ethnography"—a set of at least implicit rules for documenting an alien culture. Indeed, cultural anthropology is only possible to the extent that the "natives" have developed a coherent culture: for example, a standard language, kinship structure, corpus of myths, or cycle of rituals. The anthropologist can document major segments of this coherent culture through extended but limited observation and interviewing. This is true because even a single native speaker—or a single enactment of the wedding ritual—may be typical of all. But American anti-cult activity (like so many other phenomena sociologists wish to observe) fails to meet this criterion. It is incoherent and unstable. Merely hanging around an anti-cult headquarters, observing a few events, and interviewing convenient informants, does not add up to a scientific methodology. Sociologists must stop pretending to be anthropologists, unless they are prepared to invest the time and care of professional anthropologists and unless the topic they wish to study is in fact sufficiently coherent to permit successful ethnography. We should declare an absolute moratorium on "participant observation."

Shupe and Bromley might have achieved a much more convincing study had they employed the standard methodology for documenting incoherent mass phenomena—that is, survey research. Clearly, they could have sent questionnaires to members of local anti-cult groups, and could have conducted uniform interviews with enough properly sampled respondents to permit real analysis. Throughout this book we are treated to conclusions which could only have been derived properly from quantitative survey research on well-chosen samples. Yet the authors give no scientific support for their claims beyond their personal impressions. What they tell us may often be correct, but we have no way to evaluate the quality of their insights because they have followed no convincingly systematic methodology.

We are told that the anti-cult movement developed alternative ideologies, describing cult recruitment either as *deception* or *possession*, and that each of these models has seven components. But where is the analysis of fourteen survey questions to prove that the two ideologies were really held by different persons as unified ideological positions? We are told that the fundamentalist Christian churches were more aggressive than the liberal churches in attacking the cults, because they experienced a greater threat, because of their own doctrines and sources of faith. Perhaps. But where are the numbers to document these allegations and support the authors' interpretations against alternative explanations? Often we are given quasi-quantitative statements about what most of the parents of cult members felt, thought, or did. Yet never do we get the kind of evidence appropriate for supporting such claims.

Except for a couple of useful term-paper-like sections based on pure library work (for example, a description of legal decisions on deprogramming), this book fails utterly as an empirical report. The methods do not fit the questions, and the questions are not developed in a sufficiently rich way to turn *The New Vigilantes* into an essay in theory. This book further debases and dilutes qualitative sociology, ar

inherently valid approach which has recently been disgraced through mistreatment by too many lazy writers. Publishers like Sage deserve much of the blame: apparently they are more concerned with whether they can turn a profit on superficially academic books than with whether the books meet minimum standards of science and scholarship.

***Internal Migration Systems in the Developing World: With Special Reference to Latin America.***

*Edited by Robert N. Thomas and John M. Hunter. Boston: Hall, 1980, 176 pp. \$19.95.*

*Reviewer: JOACHIM SINGELMANN, Population Division—United Nations*

This book, according to its Introduction, is the outcome of a course on "Population, Development, and the Environment" that was organized around the migration process and given in the mid-1970s. The authors of these papers were invited to participate in that course, and their contributions make up this volume. The first chapter briefly discusses some models and methods for research on migration. The next four chapters present various empirical findings about the migration process in Colombia, Venezuela, Guatemala, and Peru. Two subsequent chapters address spatial policies and regional disparities of population distribution, and the final two chapters discuss environmental perception in rural Argentina and the integration of village migrants into urban areas in Taiwan, respectively. While these contributions may have made for a successful course, the same, unfortunately, cannot be said for this book. There are four major reasons for my negative evaluation.

First, the list of topics is eclectic and far from a systematic presentation of internal migration in Latin America. This would be acceptable if the papers were major research pieces or substantial reviews of the literature, but none qualifies for such a description. Moreover, hardly any of the major studies of internal migration in Latin America are discussed—not the Monterrey and Mexico City projects, not the research by IPEA and CEBRAP on migration in Brazil, not the main analyses in Argentina. And although there have been many migration studies in Colombia, the nine-page chapter on rural depopulation in that country gives no evidence of this wealth of information.

A second shortcoming is that the many topics represented require more than a two-page Introduction: they should have been placed in their proper context by a thorough review of internal migration in Latin America. Such an integration of the research findings is particularly important for those chapters that are based on very specialized and atypical migrant samples (e.g., high-school seniors in Venezuela and Volga Germans in Argentina), and it would also help to relate the two chapters on Africa and Asia to the mostly Latin American focus of this book. A more comprehensive Introduction would also have afforded the opportunity to bring up important migration issues not discussed by the individual contributions, such as return and circulatory migrations (becoming very important in the transformation of rural economies from a peasant to a capitalist mode of production), the effects of different class structures and state policies on urbanization and migration patterns (evident from a comparison of Peru and Brazil); or the myth of marginality of migrants in cities.

A third problem is that much of the discussion in this volume sounds dated, for the manuscripts apparently did not take account of any research published after 1975 (to judge from the publication dates of the references).

Finally, I am not certain what the audience for this volume is intended to be. It is too fragmentary for undergraduates and does not present sufficient in-depth research for graduate students; and the migration research specialist will not find much that is new in these papers.

Aside from these general remarks about the overall organization of this volume, I should like to comment on several specific issues brought up in the various chapters.

Griffin and Williams, in their chapter on Colombia, address the process of rural depopulation that "has begun on a massive scale and is accelerating." Since rural decline is a general occurrence in countries that are mostly urban (and can also be observed in other highly urbanized Latin American countries such as Argentina, Chile, and Venezuela), I am not certain as to the importance of this general trend. So far, the rural depopulation has been very small—less than 200,000 people during 1964–73 and about 500,000 during 1970–1980. Moreover, if the rural decline mostly implies a shift to *cabeceras* across many *municipios* (a *cabecera* is the administrative center of the *municipio* and somewhat similar to a county seat in the U.S.), one should not attribute too much significance to this trend. If, on the other hand, rural depopulation is concentrated in a few areas leading to a regional imbalance, it would have more crucial policy implications. Thus, in order to evaluate rural depopulation one must relate it to total population growth in *municipios*, regions, and the urban system in a more systematic way than this analysis does. I should also note that since Colombia has a more balanced distribution of its urban population than most other Latin American countries, rural depopulation is likely to have a different meaning there than elsewhere.

Both Jones' chapter on Venezuela and Thomas and Mulvihill's chapter on Guatemala assume the existence of separate value and behavioral systems for urban and rural areas, a view that implies that rural-urban migration causes many problems, since migrants are ill-prepared for life in the modern city. This view of urban-rural dichotomy, however, was challenged by Robert Redfield back in the 1950s, and it has since been rejected by most major studies of internal migration. Investigations of so-called "marginal" urban populations (like the *biscateros* in Brazil) have further refuted this distinction. Instead of emphasizing separateness, more insights would be gained from investigating the linkages between urban and rural populations.

The book also suffers from various definitional and methodological problems. For example, Jones' chapter on Venezuela states that his study is based on a sample of 191 senior university students, yet the reader is told that they were between the ages of 16 and 23, with a median age of 17—perhaps the author mistakenly translated *bachillerato* as "bachelor's degree" (in fact, it refers to high school graduation). Thomas and Mulvihill designed a stratified random sample of 2,500 family heads in Guatemala City, yet their reported results are based on only 1,200 individuals—the discrepancy is not explained. Thomas and Mulvihill's data analysis shows that with increasing distance from Guatemala City, migrants are more likely to come from a provincial capital rather than from rural areas, a finding consistent with the theory of stage migration, which states that rural migrants

to a large city bypass an intermediate center in favor of the metropolis, whereas in areas further away they migrate first to the intermediate center. But Thomas and Mulvihill also show that the correlation coefficients are smaller for more recent arrivals and conclude "that today a greater proportion of migrants arriving in a primate city have come directly from a rural area than in the past." That conclusion may be correct, although it cannot be derived from their correlations. Since they had migration histories for all individuals, however, they could have easily presented some data on the origins of migrants over time. Finally, Gallin and Gallin, in an interesting chapter on Taiwan, note that the percent urban in Taiwan increased from 10.5 to 24.4 during 1920-66, and go on to say, "Natural increase accounts for part of this growth, but migration has played the major role, for fertility rates are lower in the cities than in the countryside." While this statement may be true for *urban population growth*, it is incorrect with regard to *urbanization* (i.e., the increasing proportion of the population living in urban areas). If fertility rates are lower in the cities, Taiwan's urbanization must have been the exclusive result of migration.

In sum, this book is not very helpful for either students or researchers with an interest in Latin American migration and urbanization, but given the solid research tradition in this area, a number of good alternatives—research monographs as well as introductory surveys of the literature—are readily available.

**Symlog: A System for the Multiple Level Observation of Groups.**

By Robert F. Bales and Stephen P. Cohen. New York: Free Press, 1979. 537 pp. \$24.95.

Reviewer: JOHN STEPHEN BRENNAN, *University of Kansas*

SYMLOG is a set of methods for the study of natural groups, the focus is on personalities and relations among individuals. SYMLOG is a flexible system of parts. It can be used for a single person's retrospective adjective ratings of group members, or for a number of observers using interaction scoring. The latter would involve recording detailed observations of acts, scoring captures information on the behavioral, content, and attitude levels. Group members may do the ratings themselves, or researchers can assign ratings that reflect what they imagine group members would make of each other. In addition to the rating and scoring (i.e., measurement) parts of SYMLOG, the system offers an extensive theory (itself composed of a set of parts).

The "Systematic Multiple Level Field Theory" is a series of what Bales' calls heuristic hypotheses, concerning the basic dynamics of groups of interacting persons. The theory is implemented via numerical and statistical methods, and the results are summarized in graphic form for use in analysis and possibly for feedback to group members.

SYMLOG is based on Bales' earlier *Personality and Interpersonal Behavior* (1970) and *Interaction Process Analysis* (1950). The key theoretical insight reflected in SYMLOG is the use of the same three-dimensional space to describe both the definition of the situation in content terms and the behavior of the participants. The SYMLOG system uses a "field diagram" to show the location of members in a particular two-dimensional plane of the three-dimensional space. A clear plastic

overlay is used to determine the dynamic structure of the field. This "polarization-unification" overlay reflects a number of theoretical assumptions concerning the psychological processes that could have resulted in the pattern of images in the field diagram.

The dimensions of the three-dimensional space are: dominant versus submissive, friendly versus unfriendly, and instrumentally controlled versus emotionally expressive. Observation is focused on what the individual *does* (nonverbal behavior and overt action) and what the individual *says* (content is classified as descriptive of self, another member, the group, the external situation, the societal environment, or fantasy images). The SYMLOG system also focuses on attitudes toward the content classes. Thus the system is multiple level: behavior, content images, and value judgments are the major levels.

The book does not attempt to develop detailed links with the various theoretical perspectives on which the system is based. Instead, it introduces the observation method and the techniques for analysis, and suggests areas for further research. Bales does acknowledge a number of influences, however. Psychoanalytic theory contributes the idea of motivation. Gestalt psychology and theories of group dynamics give the concept of field. The concepts of polarization and unification come from balance theory, congruity theory, dissonance theory, consistency theory, and attribution theory. Personality theory, clinical psychology, individual and group therapy, and family therapy have all contributed to the creation of SYMLOG. On a sociological level, symbolic interaction theory leads to the emphasis on communication of meaning as opposed to the physical substrate of behavior. Social exchange theory contributes the concepts of behavior and content, seen as rewards and punishments. The theory of SYMLOG leaves as an empirical question the relative importance of consensus or conflict in a particular group.

The book is divided into five parts. Part One, by Bales, uses ratings of members of a single group to illustrate the theory and to exemplify the power of SYMLOG for producing useful, graphic information about the dynamics of group interaction. Part Two, by Bales and Cohen, discusses the more detailed *interaction scoring method* component of SYMLOG. This section presents the "SYMLOG message," a written communication between observer and the group member designated as "actor" in the message. In addition, the message indicates observer group identification, date, time, receiver, whether the act is overt, the direction (in the three-dimensional space) of the act, a brief description of behavior or content, attitude (pro or con) toward the content image, the direction of the image, and the level of the image. The SYMLOG interaction scoring method is designed for use in settings where a self-analytic group and an observer group cooperate in a learning process. Members of the self-analytic group get feedback that allows them to monitor their behavior over time. Observers get information about their perceptions and biases. SYMLOG gives detailed, democratic, and systematic feedback.

Part Three, by Cohen, is a study of SYMLOG adjective ratings and SYMLC Interaction Scoring. Each of these is analyzed for reliability, and the factor structure of adjective ratings and the relations between rating and scoring are presented. The results indicate that the rating method and the scoring method tend to converge and that the theoretical three-dimensional SYMLOG space is supported by both measurement methods.



Part Four demonstrates the use of SYMLOG for training in observation and feedback to group members. Part Five comprises technical appendices that offer detailed instructions for the various parts of the SYMLOG system. All of the information necessary to use the SYMLOG system is presented. SYMLOG should be useful for small groups (e.g., teams or committees) that want feedback about individual behavior and values. Diagrams will allow group members to pinpoint and solve group problems.

# Take Note

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**Agnew, John A. (ed.).** INNOVATION RESEARCH AND PUBLIC POLICY. *Ann Arbor: University Microfilms International, 1980. 286 pp. \$19.75.* Eight papers by geographers examine the origin and influence of public policy on the diffusion of innovations. Four are general discussions, four are case studies.

**Ahmed, Akbar S.** PUKHTUN ECONOMY AND SOCIETY: TRADITIONAL STRUCTURE AND ECONOMIC DEVELOPMENT IN A TRIBAL SOCIETY. *London & Boston: Routledge & Kegan Paul, 1980. 406 pp. \$35.00.* This comprehensive study of the tribes on Pakistan's Northwest frontier presents detailed observations of the Mohmand ecological and administrative framework, lineage organization, domestic and economic structure and the encapsulation of the tribal area by the surrounding political systems. The importance of ideology is stressed, specifically the role of Islamic religious symbolism. Social behavior and organization tend toward the ideal as expressed in Islam, Ahmed argues, so long as external influence is minimized.

ALCOHOL AND ALCOHOLISM: THE REPORT OF A SPECIAL COMMITTEE OF THE ROYAL COLLEGE OF PSYCHIATRISTS. *New York: Free Press, 1979. 162 pp. \$10.95.* Responding to alarming statistics, the (British) Royal College of Psychiatrists appointed a special committee, whose report provides a cogent summary of available evidence on alcohol abuse and the complex mental, social, and physical problems that stem from it. A prevention program involving education of the public and a recommended maximum daily consumption are proposed.

**Blasi, Joseph Raphael.** THE COMMUNAL FUTURE: THE KIBBUTZ AND THE UTOPIAN DILEMMA. *Norwood, Pa.: Norwood Editions, 1980. 277 pp. \$17.50.* This monograph opens a new series from Harvard's Center for Jewish Studies. (See Cherns, below.) It is primarily a study of a single kibbutz, pretty much a traditional ethnography, with special emphasis on personality and mental health. There is, in addition, comparison throughout of the practical exigencies facing contemporary kibbutznim and the utopian hopes forming kibbutz ideology.

**Blu, Karen I.** THE LUMBEE PROBLEM: THE MAKING OF AN AMERICAN INDIAN PEOPLE. *New York: Cambridge University Press, 1980. 276 pp. Cloth, \$24.50, paper, \$6.95.* The Lumbee Indians are concentrated in eastern North Carolina, set amid the better known black and white ethnic contestants of the American South. This is a political and legal history, set in a wider context, as well as an ethnography. In addition to her empirical study, Blu offers some general thoughts on the concept and importance of ethnicity.

**Bourne, Larry S.** THE GEOGRAPHY OF HOUSING. *New York: Wiley, 1981. 288 pp. Cloth, \$37.95, paper, \$19.95.* A clearly written introduction to a body of literature badly in need of systematic presentation. Focuses on variation in the distribution of hous-

stock between and within areas, with most attention to the English-speaking advanced capitalist countries, although Sweden, Poland, and the Third World are briefly considered.

**Camberwell Council on Alcoholism. WOMEN AND ALCOHOL.** *London: Tavistock, 1980. 207 pp. Cloth, \$22.00; paper, \$9.95.* Ten professionals associated with the Camberwell Council on Alcoholism (London) have produced this sourcebook for people professionally, or personally involved with women problem drinkers. The content ranges from discussion of the causes, patterns, and consequences of drinking problems among women to more detailed examination of particular features of women's drinking and provision of treatment services.

**Cherns, Albert (ed.). THE QUALITY OF WORKING LIFE AND THE KIBBUTZ EXPERIENCE** *Norwood, Pa: Norwood Editions, 1980. 287 pp. \$17.50.* The second volume in the Harvard Center for Jewish Studies series on the Kibbutz, Communal Society and Alternative Social Policy (see Blasi, above) considers topics ranging from the choices facing kibbutzim, to general issues about working and communal life, to the relations between kibbutzim and political institutions, to the role of trade unions in British industrial producers' cooperatives, to the quality of working life on the kibbutzim. Relatively few of the contributions are full-fledged research papers.

**Coxon, Anthony P. M., and Charles L. Jones. MEASUREMENT AND MEANINGS: TECHNIQUES AND METHODS OF STUDYING OCCUPATIONAL COGNITION.** *New York: St. Martin's Press, 1980. 305 pp. \$22.50.* This methodological appendix to *The Images of Occupational Prestige* (1978) and *Class and Hierarchy* (1979) provides detailed descriptions of the sampling strategies, methods of data collection, and methods of analysis employed in those studies of occupational cognition and evaluation. The authors seek methods that retain subjective meaning.

**Enloe, Cynthia. POLICE, MILITARY AND ETHNICITY. FOUNDATIONS OF STATE POWER** *New Brunswick: Transaction Books, 1980. 179 pp. \$14.95.* Seeks to demonstrate the importance of the state-ethnicity relationship, examining state institutions, particularly the military and police, in terms of ethnic composition, and the role that ethnicity plays in maintaining elite power. Several states are treated, particularly those of the Third World, although a very interesting section deals with Britain and Ulster.

**Fredericks, Marcel A., Ralph R. Lobene, and Paul Mundy. DENTAL CARE IN SOCIETY: THE SOCIOLOGY OF DENTAL HEALTH.** *Jefferson, N.C.: McFarland, 1980. 180 pp. \$15.95.* Described as the first published attempt to inform the dental profession about societal influences on health care delivery, preventive practices, and disease rates, this is intended as a standard sociological text for dental and dental auxiliary educators and health professionals in general.

**Greenberg, Paul D., and Edward M. Glaser. SOME ISSUES IN JOINT UNION-MANAGEMENT QUALITY OF WORKLIFE IMPROVEMENT EFFORTS.** *Kalamazoo: Upjohn Institute for Employment Research, 1980. 85 pp.* Representatives of 20 international unions discuss labor-management cooperation to improve quality of worklife (QWL). Attempts to clarify the concept of QWL, to identify workable structures for implementing QWL programs, and to analyze union and management roles in such programs. Materials based on the conference proceedings are supplemented by the

reactions of ten management personnel, and the authors suggest implications for public policy.

**Hamer, John, and Jack Steinbring (eds.).** ALCOHOL AND NATIVE PEOPLES OF THE NORTH. *Lanham, Ma.: University Press of America, 1980. 323 pp. Cloth, \$19.75; paper, \$11.50.* Noting the variations in drinking styles and in the meanings attached to alcohol use by Canadian Native peoples and those of European ancestry who introduced them to alcohol, this volume brings together original essays by leading anthropologists which should be of interest both to professionals in the area of alcohol studies and to a wider lay audience.

**Hill, Samuel S., Jr.** THE SOUTH AND THE NORTH IN AMERICAN RELIGION. *Athens: University of Georgia Press, 1980. 152 pp. \$12.50.* America's most distinguished student of southern religion looks historically at how his subject matter came to be distinct, and at whether it is likely to remain so. These chapters were the Lamar Memorial Lectures at Mercer University.

**Madison, Bernice Q.** THE MEANING OF SOCIAL POLICY: THE COMPARATIVE DIMENSION IN SOCIAL WELFARE. *Boulder: Westview Press, 1980. 333 pp. \$35.00.* Striving for geographical as well as topical comprehensiveness rather than focused and detailed analysis, Madison provides a review of the welfare policies and programs of capitalist and socialist, developed and underdeveloped societies. She reviews a great deal of social science research, and treats problems of definition and methodology. Topics include social security, programs for special groups like the handicapped and children, the organization of programs, and social welfare planning. A selected bibliography is provided.

**Manning, D.J. (ed.).** THE FORM OF IDEOLOGY: INVESTIGATIONS INTO THE SENSE OF IDEOLOGICAL REASONING WITH A VIEW TO GIVING AN ACCOUNT OF ITS PLACE IN POLITICAL LIFE. *London: Allen & Unwin, 1980. 136 pp. Cloth, \$19.50, paper, \$8.95.* Six diverse essays deal with ideology in various contexts of political life. All center around the critique that ideology offers no real information, and should not be conceived as a defective theory, but rather as a justificatory framework for action.

**Mansbridge, Jane J.** BEYOND ADVERSARY DEMOCRACY. *New York: Basic Books, 1980. 398 pp. \$20.00.* American democracy seeks to resolve conflicts among ends rather than to achieve unitary ends. Such, at least, is the thesis of this provocative and interesting book. Interviews and descriptive material range widely, but include case studies of a New England town meeting and a self-governing urban crisis center. The best part, however, is the book's discovery of the hidden operation of "unitary democracy" and its arguments concerning possibilities for its extension.

**Meadow, Robert G.** POLITICS AS COMMUNICATION. *Norwood: ALEX, 1980. 269 pp. \$24.95.* This attempt to clarify and to establish political communication as a subdiscipline gives special effort to extending its study beyond consideration of mass media effects. Meadows links communication concepts to the study of such political phenomena as conflict, integration, participation, power, and political socialization, and also looks at reciprocal relations: language as a political issue, regulation of communication, and the government as communicator.

**Pepitone-Rockwell, Fran (ed.).** DUAL-CAREER COUPLES. *Beverly Hills: Sage, 1980. 294 pp. Cloth, \$18.95; paper, \$9.95.* Reports research on families units where both husband and wife are engaged in occupations requiring continuous and high commitment. Issues for the family include the impact of wives' employment on the family's role structure, the benefits and costs of dual-career marriage, and role strain for individuals. Career issues include job search problems, salary and job performance differences, and the idea of the "coordinated career couple," in which work activity overlaps to some extent. The impact of equal opportunity laws is also reviewed.

**Phizacklea, Annie, and Robert Miles.** LABOUR AND RACISM. *London & Boston: Routledge & Kegan Paul, 1980. 236 pp. \$30.00.* Data from northwest London are employed in an examination of black migrant labor as a racialized fraction of the working class. Observational work and interviews with English and West Indian men and women in both a factory and a residential area are the basis for a discussion of working-class consciousness, racial consciousness, trade unionism, issues concerning migrant women, and possible political actions.

**Rule, James, Douglas McAdam, Linda Stearns, and David Uglow.** THE POLITICS OF PRIVACY: PLANNING FOR PERSONAL DATA SYSTEMS AS POWERFUL TECHNOLOGIES. *New York: Elsevier North-Holland, 1980. \$14.95.* Professional concern over the implications of new data management technologies is mounting, yet the public has only a slight appreciation of most of the issues. Privacy is one of the most important of these concerns, and this book is the authoritative treatment of the issue. The authors consider not only ethical problems but technological alternatives, not only political difficulties but possibilities for effective planning.

**Sanders, Irwin T., Walter C. Bisselle, and Roger Whitaker.** EAST EUROPEAN PEASANTRIES: SOCIAL RELATIONS—AN ANNOTATED BIBLIOGRAPHY OF PERIODICAL ARTICLES, VOLUME 2. *Boston: Hall, 200 pp. \$16.00.* A follow-up to a similar bibliography compiled in 1976. Articles concerning the social relations of the countryside and rural-urban relations are documented in this volume for Eastern Europe generally, and Hungary, Greece, and the Slavic nations individually. Particularly valuable for its summaries of articles written in languages other than English or French.

**Spindler, George D. (ed.).** THE MAKING OF PSYCHOLOGICAL ANTHROPOLOGY. *Berkeley: University of California Press, 1980. 679 pp. \$10.75.* Many of the most important living psychological anthropologists grace this volume, which is neither simply a retrospective nor a manual, but bits of both mixed with a good deal of autobiography and several cautionary tales. Most authors attempt to convey something of their personal approaches to issues in fieldwork and analysis. The editing is unusually careful and thorough.

## Referees\*

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Kitano, Harry H. L.  
Klemmack, David L.  
Kluegel, James  
Kobrin, Frances E.  
Kohn, Melvin  
Krohn, Marvin D.  
Kutner, Nancy G.  
LaFree, Gary D.  
Land, Kenneth C.  
Lavin, David  
Lebowitz, Barry D.  
Lee, Everett  
Leggon, Cheryl B.  
Leghorn, Rex Y.  
Leik, Robert K.  
Leiter, Jeffrey  
Lemert, Edwin  
Lerman, Paul  
Levin, Martin L.  
Levinson, Richard  
Lewis, Jerry  
Lincoln, James R.  
Lipset, Seymour M.  
Liska, Allen E.  
Lizotte, Alan J.  
Loftin, Colin  
Logan, John R.  
Long, J. Scott  
Long, Larry H.  
Lyman, Stanford M.  
McClendon, McKee J.  
McInnis, Kathleen  
MacKinnon, Neil J.  
Macmillan, Alexander  
McNamara, Patrick  
McPhail, Clark  
McTavish, Donald  
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Maris, Ronald  
Marsden, Lorna R.  
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Marshall, James  
Marshall, Susan  
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Mason, Karen  
Mazur, Allan  
Meier, Robert F.  
Messner, Stephen  
Michelson, William  
Michener, H. Andrew  
Mileti, Dennis S.  
Miller, Ann R.  
Minor, William  
Moberg, David O.  
Moen, Phyllis  
Molm, Linda D.  
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Morrison, James  
Moxley, Robert L.  
Mueller, Carol  
Mueller, Daniel P.  
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Nagel, Ilene  
Nelsen, Hart M.  
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Newman, Graeme R.  
Nolan, Patrick D.  
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Oliver, Pamela  
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 Orum, Anthony M.  
 Osmond, Marie  
 Otto, Luther B.  
 Palmore, Erdman  
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 Petersen, William  
 Pfeffer, Jeffrey  
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 Pitcher, Brian L.  
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 Poston, Dudley L.  
 Presser, Stanley  
 Pullum, Thomas  
 Quarentelli, Enrico L.  
 Ragin, Charles C.  
 Randall, Susan  
 Rankin, Joseph H.  
 Reasons, Charles E.  
 Reilly, Mary Ellen  
 Rhodes, Lewis A.  
 Richards, Pamela J.  
 Ritterband, Paul  
 Robinson, Robert V.  
 Rockwell, Richard  
 Rohe, William M.  
 Rokach, Milton  
 Roof, W. Clark  
 Rose, Peter  
 Rosen, Bernard  
 Rosenbaum, James  
 Rosenberg, George S.  
 Rothman, Barbara Katz  
 Rubinson, Richard  
 Rushing, William A.  
 Rytina, Nancy F.  
 Saltzman, Linda E.  
 Sanders, Joseph  
 Savitz, Leonard  
 Scanzoni, John  
 Scarpitti, Frank R.  
 Schaefer, Richard T.  
 Schneider, David J.  
 Schneider, Joseph W.

Schneidman, Edwin S.  
 Schoenherr, Richard A.  
 Schulman, Michael D.  
 Schwartz, Barry  
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 Schwartz, Richard  
 Schwirian, Kent P.  
 Scull, Andrew  
 Segal, David  
 Seidler, John  
 Sell, Ralph  
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 Shin, Eui-Hang  
 Short, James F., Jr.  
 Shostak, Arthur B.  
 Sills, David L.  
 Simmons, Roberta  
 Simpson, Ida Harper  
 Skipper, James K.  
 Skvoretz, John  
 Slatin, Gerald T.  
 Sly, David F.  
 Smith, Clagett  
 Smith, Joel  
 Smith, Richard L.  
 Smith, Ronald W.  
 Smith, Thomas E.  
 Smith-Lovin, Lynn  
 Solomon, David N.  
 Spanier, Graham B.  
 Speare, Alden  
 Spilerman, Seymour  
 Stack, Steven  
 Stebbins, Robert A.  
 Steffensmeier, Darrell J.  
 Steiber, Steven R.  
 Stephan, Cookie White  
 Stephenson, Richard M.  
 Stern, Alan  
 Stewart, Phyllis L.  
 Stewart, Robert L.  
 Stolzenberg, Ross M.  
 Stone, Leroy O.  
 Stone, Philip J.  
 Sudman, Seymour  
 Sutton, L. Paul  
 Sykes, Gresham  
 Tallman, Irving

Taylor, Patricia  
Tayman, Jeffrey M  
Tedeschi, James T.  
Telles, Joel  
TenHouten, Warren D.  
Tessler, Richard C  
Thornberry, Terence P.  
Tickamyer, Ann R.  
Tittle, Charles R.  
Tolbert, Charles M.  
Touhey, John C.  
Tropman, John E  
Tucker, Charles W.  
Turk, Austin T  
Turk, James L  
Turner, Jonathan H.  
Twaddle, Andrew  
Ugalde, Antonio  
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VanValey, Thomas  
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Webster, Murray  
Weisman, Carol  
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Williams, Robin  
Willigan, J. Dennis  
Wilson, John  
Wilson, Kenneth L.  
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Zeisel, Hans  
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# Tolerance of Free Speech in the United States and West Germany, 1970–79: An Analysis of Public Opinion Survey Data\*

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## ABSTRACT

*In previous research on political tolerance in West Germany I found that respondents' level of education had no statistically significant effect on a number of variables measuring tolerance. This result seemed to contradict standard findings for populations of democratic countries. Therefore, this article explicitly compares tolerance of free speech for nonconformists in America using replications of Samuel Stouffer's benchmark questions (National Opinion Research Center surveys in 1972 and 1977) with very similar—but not identical—questions asked in West German national surveys in 1970 and 1979. These questions concern tolerance for a communist, an atheist, and a neo-Nazi/militarist speaker. Using log-linear models to analyze scales of these questions and of the disaggregated tolerance items, the initial findings were confirmed: education has little effect on tolerance in West Germany, but considerable effect in the U.S. The influence of generational cohort, occupation, left–right ideological self-placement, party preference, country, and time are also tested. Semantic and historical explanations for the findings are briefly discussed.*

A thirty-year accumulation of empirical research in liberal democracies has impelled observers to the conclusion that higher levels of education are positively related to higher levels of liberalism—especially political and social tolerance. This relationship has often been interpreted as showing the influence of educational institutions in instilling the dominant (liberal) norms of the society. Although there are several other possible interpretations (which will be briefly discussed), this one provides a reasonable starting point for treating the phenomenon in a comparative and historical perspective, as will be done here.

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The seeming consistency of these findings in several Western countries, above all the United States, has led some to generalize this relationship. One of the clearest statements of this view is given by Lipset:

Data gathered by public opinion research agencies which have questioned people in different countries about their beliefs on tolerance for the opposition, their attitudes toward ethnic or racial minorities, and their feelings for multi-party as against one-party systems have showed that the most important single factor differentiating those giving democratic responses from the others has been education. The higher one's education, the more likely one is to believe in democratic values and support democratic practices (39-40).

And Lipset's view from twenty years ago is still widely held today. A recent study of tolerance in America in the 1970s cites this passage and concludes that "this assessment continues to describe research evidence accurately in 1977" (Nunn et al., 58).

To substantiate his claim, Lipset makes an explicit comparison between the U.S. and Germany. In his well-known (and still controversial) chapter on "Working-Class Authoritarianism" in *Political Man*, he cites evidence on tolerance for the rights of dissidents to speak publicly in the United States and evidence on support for a more-than-one party system in Germany to illustrate the theory that the better educated are more democratic, more liberal, or less authoritarian than the rest of the population (100-3, cf. Tables 3 and 4).

It is well known, however, that the use of different indicators to measure the same phenomenon across time or place is a tricky business. In this case, one could argue that these two items—dissidents' right of free speech and approval of multi-party systems—measure different things, acceptance of liberal values and support for democratic institutions. The distinction is not trivial, although it may not appear obvious from the perspective of a long-established liberal democracy. Especially in newer democracies, adherence to the regime form and its institutions may simply be part of an otherwise undemocratic citizen's loyalty to the state: liberal values like tolerance may not be part of this ideological package. Nor need this distinction necessarily be limited to new regimes which aspire to liberal democratic status. For example, it is theoretically possible to have an illiberal democracy of a Rousseauist or communist type—a danger clearly recognized by Tocqueville and J. S. Mill in their discussions of the "tyranny of the majority"—in which a majority allows no deviations or dissent from its decisions; and although we may feel that the definition of democracy is thereby stretched, balanced defenses of this distinction have also been made with reference to contemporary empirical cases (Lindblom) <sup>1</sup>

If this distinction is correct, then there need be no necessary correspondence in the pattern of association between democratic and liberal measures; and there is no necessary basis for concluding that if the better

educated in Germany support a more-than-one party system, they will also tend to be more tolerant of the speaking rights of dissidents. Indeed, Almond and Verba's 1959 multi-nation survey failed to find positive associations between higher education levels and certain deeper aspects of liberal democratic political culture in West Germany (105, 151-3); and Muller et al. found on the basis of a 1974 cross-national survey that West Germany deviated from several other Western democracies in its lack of association between level of education and support for the freedom of assembly (cf. Olsen and Baden). To my knowledge, no investigator has seen a consistent pattern of West German exceptionalism in these results or been prepared to offer more than a tentative explanation when the finding is noted in isolated cases. As I will suggest in the concluding discussion, however, there may be important historical reasons why better educated Germans are no more tolerant than worse educated Germans.

The data on tolerance in America which Lipset cites, as do a great many other writers on the topic, derive from Samuel Stouffer's 1954 survey, *Communism, Conformity, and Civil Liberties*, perhaps the earliest such investigation whose results are still comparable to present investigations (cf. Adorno et al.). Stouffer's central conclusions have formed a benchmark from which to build our expectations about the structure of tolerance in a western population: "The data showed that the older generation was less tolerant of nonconformists than the younger generation; also, that within each group the less educated were less tolerant than the better educated" (107). And several independent American replications of all or part of Stouffer's surveys in the 1970s (Davis, a; Nunn et al.) have established that (1) Stouffer's findings about generation and education still hold true, and (2) there has been a considerable aggregate increase in levels of measured tolerance in the American population since the 1950s, partly due to the replacement of older, less tolerant generations by younger, more tolerant generations, who are also better educated and therefore more tolerant; and partly due to a residual historical change in popular attitudes which cannot be accounted for by generation replacement and/or higher levels of education.<sup>2</sup>

## The Data

Because there are indications that the results for West Germany may diverge in an important way from a well-established pattern for the United States and perhaps other Western democracies, it was considered worthwhile to attempt as exact a cross-national comparison as possible to see whether the difference is real or only apparent, and if real, what the probable causes of the divergence are. An extensive search of survey data for West Germany suggested that Stouffer's items had not yet been asked

there; a set of very similar items was found, however, which are almost exact translations of questions used in the Detroit Area Studies of 1958 and 1971 (Duncan et al.; Steiber). However, since the Detroit Area texts are substantively so close to the Stouffer items and since they are restricted in the U.S. to one northern urban area at one time point in the 1970s, the Stouffer items (which were also asked in U.S. national surveys in the 1970s) are compared here to the text available for West Germany.<sup>3</sup> Here are the exact texts of these questions.

TEXTS OF TOLERANCE QUESTIONS ASKED IN THE U.S. AND GERMANY

*Germany*

"In our country, the Basic Law—that is, the Constitution—guarantees the right of freedom of expression for everyone. I have a couple of questions about this. There are always these six answers possible:

*Yes, absolutely.*

*Yes, probably.*

*No, probably not.*

*Certainly not.*

*Undecided.*

*Don't know.*

"... if someone publicly attacks religion or Christianity, does the right of freedom of expression permit this?

"And does the right of free expression of opinion permit somebody to advocate communism, to call for a communist world revolution?

"And if someone publicly advocates the founding of a new National Socialist [Nazi] party, does the right of free expression permit this?"

*U.S.*

*Asked in 1972 and 1977 (also 1954):*

"Now, I should like to ask you some questions about a man who admits he is a communist. Suppose this admitted communist wants to make a speech in your community. Should he be allowed to speak?

*Yes.*

*No.*

*Don't know.*

"There are always some people whose ideas are considered bad or dangerous by other people. For instance, somebody who is against all churches and religion. If such a person wanted to make a speech in your city (town, community) against churches and religion, should he be allowed to speak?

*Yes.*

*No.*

*Don't know.*

Asked in 1977 only:

"Consider a person who advocates doing away with elections and letting the military run the country. If such a person wanted to make a speech in your community, should he be allowed to speak?

Yes.

No.

Don't know.

Thus, in 1970 in Germany and in 1972 in the U.S. there are parallel questions about tolerance for the public speaking rights of an atheist and of an advocate of communism; and in 1979 in Germany and in 1977 in the U.S. these two questions were re-asked together with a question about tolerance for a neo-Nazi speaker in Germany and for a speaker who advocates banning elections and letting the military run the country in the U.S. These data thus allow us to compare tolerance in two countries at two times in the 1970s.

The surveys used in these analyses include a 1970 survey from the Institut für Demoskopie of Allensbach (survey number 2066) and the 1979 ZUMABUS survey conducted by Infratest of Munich for the Zentrum für Umfragen, Methoden und Analysen of Mannheim (the questions were included for the present study), and also the 1972 and 1977 American General Social Surveys conducted by the National Opinion Research Center (NORC) of Chicago. The 1970 survey is a quota sample<sup>4</sup> of the German population (including West Berlin) aged 16 and older; and the 1972, 1977, and 1979 surveys are multistage probability samples of the German (without West Berlin) or American populations aged 18 and older. There are 1,662 cases in the 1970 survey, 1,613 cases in the 1972 survey, 1,530 cases in the 1977 survey, and 2,012 cases in the 1979 survey.

It was desired to test the effects on tolerance of generation, education, social class, and political orientation on tolerance. I define generations as birth cohorts, and for this analysis, four cohorts are used, those who reached age 21 (an arbitrary age of "political maturity") before 1933, those who became 21 in 1933–48, those who became 21 in 1949–65, and those who have become 21 since 1965. Level of education was defined by comparable levels of certification—a middle level and a high level, yielding three groups—those with less than a *mittlere Reife*/high school diploma, those with this certificate but not an *Abitur* or college diploma, and those with an *Abitur*/college diploma or higher. Occupation of the head of household was used as a measure of social class, with manual workers representing the working class; professionals together with the self-employed (for Germany) or managers (for the U.S.) representing the old middle class; and other white-collar workers representing the new middle class; those in the agricultural sector were excluded from the analysis. Variables for party-political orientation were used for both countries—in the U.S. a party

identification question and for Germany a hypothetical vote intention if there were an election next Sunday. The right was represented by American Republicans and German Christian Democrats (CDU-CSU), the left by the Democrats in the U.S. and the Social Democrats in Germany, and the center or residual group by Independent identifiers in America and by FDP voters and those who chose no party in Germany; supporters of other parties in Germany (mainly radical and ecology parties; a very small percentage) were excluded from the analysis. Finally, the response categories for the German tolerance items were dichotomized to correspond to the American formulation (a tolerant and an intolerant response). The two items available at both times were combined to make a scale, and a tolerant response to *both* items was considered necessary to code a tolerant response; other responses were classified as less tolerant. A three-item scale was also tested for the late 1970s (for both countries) and a six-item Guttman scale was tested for Germany alone (for both time points), but the results were very similar to the two-item scales and will not be reported here. Those expressing no opinion on any tolerance item were excluded from the analyses. The sample distributions of the variables used here are shown in Table 1.

## Analyses

We can systematically investigate the relationships among the variables used here by making complete cross-tabulations of them for each period and for each country, combining the multivariate contingency tables and treating time and country each as variables, and analyzing the resulting contingency tables with a series of log-linear models. Models were thus fitted to two basic kinds of contingency tables, those with the tolerance items combined into scales, and those with each separate tolerance item included in the table. (Since these contingency tables are very large and virtually impossible to inspect visually, they will not be presented here.) The procedure was followed of first considering the ideology variable(s) as dependent with the contingency table collapsed over party preference, and then including party preference as the ultimate dependent variable. Thus, the model-building assumes a causal ordering of the variables and proceeds in a recursive fashion from the beginning to the end of the causal chain; but since we will not be concerned with the relationships among the predictor variables, we concentrate only on the last and next-to-last steps of the chain. Following Goodman's modified path analysis technique (ch. 11), the predictor variables are controlled by including an interaction term of all possible associations among them at each modeling step. Finally, although standard methods of hierarchical model-building are used (Fienberg, a; Goodman), two criteria are used to determine goodness of fit:



**Table 1.** POLITICAL TOLERANCE THE UNITED STATES 1972-77 AND WEST GERMANY 1970-79  
SAMPLE FREQUENCIES OF VARIABLES USED IN ANALYSES

	<u>United States</u>		<u>West Germany</u>	
	1972	1977	1970	1979
<u>Percent Tolerant (incl. "Don't Knows")</u>				
Communist speaker	52%	55%	57%	69%
Atheist speaker	65	62	75	86
Neo-Nazi/militarist speaker	--	50	(46)	52
Scale score	40	42	41	45
<u>Cohort (year became age 21)</u>				
Post 1965	21	32	27	31
1949-65	35	34	28	28
1933-48	19	17	22	24
Pre 1933	25	17	23	17
<u>Education</u>				
Volksschule + grade school	40	35	77	65
Mittlere reife + high school	49	51	18	24
Abitur + univ. + college	11	14	5	11
<u>Occupation</u>				
Worker	56	58	51	37
Employee	16	14	38	54
Self-employed + professional	28	28	12	9
<u>Party Preference</u>				
CDU/CSU + GOP	23	22	34	38
SPD + Democrat	50	45	38	42
FDP + Independents	27	33	28	20

whether the model as a whole provides an adequate fit, and whether individual interaction terms are statistically significant (i.e., add significantly to the overall fit) when tested at the appropriate modeling stage. Since the  $X^2$  statistics have been divided by two to allow for clustering in the sample designs, the overall fit tends to yield overly parsimonious models when judged in relation to the test of significance of individual interaction terms, which in this case are probably more reliable (see Fienberg, b, 25). The processes of model fitting will not be shown in full here in order to save space, but the results are available from the author on request: although model fitting occupies the bulk of the methodological literature on log-linear models, the reader of substantive research is usually most interested in the final results, and that will be the focus here.

#### THE TOLERANCE SCALES

The effect parameters for the final fitted models for the contingency table using tolerance scales are shown in Table 2: the first column of Table 2

**Table 2.** POLITICAL TOLERANCE THE UNITED STATES 1972-77 AND WEST GERMANY 1970-79  
SELECTED EFFECT PARAMETERS FROM FINAL LOG-LINEAR MODELS TOLERANCE SCALES\*

	Germany 1970-79/ U.S. 1972-77	G-70	G-79	US-72	US-77
<u>Occupation</u>					
Worker	-.09	-.16	--	--	-.19
Employee	.03	.06	--	--	.09
Self-employed	.06	.10	--	--	.09
<u>Cohort</u>					
Post 1965	.28	.22	.26	.33	.33
1949-65	.12	.07	.18	.10	.11
1933-48	-.06	-.03	-.07	-.06	-.07
Pre 1933	-.34	-.27	-.37	-.37	-.37
<u>Education</u>					
Volksschule + grade	-.34	--	-.23	-.63	-.40
Mittlere reife + high school	-.03	--	-.02	-.04	-.05
Abitur + univ. + college	.37	--	.25	.67	.45
<u>Party</u>					
CDU/CSU + GOP	--	--	--	--	--
SPD + Democrat	--	--	--	--	--
FDP + Independents	--	--	--	--	--
<u>Country</u>					
Germany	.05				
U.S.	-.05				
<u>Year</u>					
1970 + 1972	--				
1979 + 1977	--				
<u>Country-Education</u>					
Germany					
Volksschule	.18				
Mittlere reife	.01				
Abitur + univ.	-.18				
U.S.					
Grade school	-.18				
High school	-.01				
College	.18				

\*Positive values indicate greater tolerance, as against lesser tolerance.

Dash (--) indicates interaction statistically insignificant (at .10): parameter set to zero. All other interactions significant at least at .05.

shows the parameters for the pooled contingency table, and the last four columns show the parameters for similar tables disaggregated by year and country (see Goodman, ch. 12, on this sort of disaggregation).

We are most interested to know whether there is a cross-national difference in the relation of education to tolerance, that is, whether a country-education-tolerance interaction is statistically significant. We directly test the null hypothesis that this interaction is *not* statistically significant by comparing appropriate "adjacent" models—which differ only

in that one includes and the other excludes this interaction—but tests show that it is highly significant ( $X^2 = 27.41$ , with 2 *df*) and that it explains at least an additional 6 percent of the variation relative to the baseline model (in which none of the predictor variables has an effect on tolerance). We therefore reject the null hypothesis and note that the relationship of education to tolerance differs between the two countries. We can investigate the *nature* of the education–tolerance variation by country by examining the directions and magnitudes of the significant interaction parameters, shown in Table 2. A glance at these results reinforces the conclusion that while the better educated in the United States are much more tolerant than the less well educated, virtually no such relationship exists in West Germany. This can be seen by adding the appropriate country component of the country–education–tolerance interaction term to the base education–tolerance effect parameter: for the U.S. this procedure yields a spread about zero above .50, while for Germany the spread is under .20; and by examining the education–tolerance interactions directly for each year and country in the last four columns: for the U.S. the figures remain about .50, but for Germany we again see that there is no significant association between education and tolerance in 1970, although a statistically significant interaction emerges in 1979 with a spread about zero of .25. A direct test of the hypothesis that the education–tolerance association is converging in the two countries over time in the 1970s, however, showed no statistically significant effect. On the other hand, tests I conducted on other dependent variables have also indicated the possible emergence of an education–tolerance association in Germany in the late 1970s (tests not shown here). Thus, we must perhaps modify the above conclusion because although educational level had virtually no effect on measured tolerance in Germany in the early 1970s, a small relationship appears to emerge in the late 1970s along the lines of the American pattern.

The results in Table 2 also indicate that the magnitude of the cohort effect on tolerance is roughly the same as that of education, but that there are no significant differences between the two countries nor changes during the 1970s. The younger generations are more tolerant than the older. Other testing, however (not shown), indicates that this generational effect has possibly become stronger in the United States since the 1950s. In a test using a similarly constructed scale of tolerance for the speaking rights of nonconformists (Stouffer data), the cohort–tolerance effect has a spread about zero of .20 in 1954 and about .40 in 1972–74; but since there is no significant year–cohort–tolerance interaction effect, this change is probably due to the historical entry of new generations with different levels of tolerance, rather than differential changes in the tolerance levels of the cohorts measured at both times. These findings parallel a number of my findings for Germany (not shown), where longer time series allowed me to investigate generational change in liberal attitudes since the early 1950s.<sup>5</sup> They

suggest that if a longer time series for the present tolerance items were available, we might also find an increase in generational correlations in Germany since the 1950s.

A few final features of Table 2 deserve mention. The first is that a small but statistically significant association appears between occupation and tolerance in the pooled table (first column), with the higher social class levels more tolerant than the workers, and this association also appears irregularly in the disaggregated tables (last four columns). We will later be able to locate the source of this association in one of the tolerance items in the scales; but for now we simply observe that this result is weakly at variance with a previous analysis of the German data alone (using a six-item scale; not shown) where no such significant association was found, but that it follows roughly the same pattern as in an analysis of the American data above for the period 1954 to 1972-74 (not shown). This result clearly supports Lipset's well-known theory of "working-class authoritarianism" (ch. 4), but when education and cohort are taken into account, it is also clear that social class (as measured by occupation) has only a weak residual relationship to tolerance. Under this test, working-class authoritarianism, while observable, is only a marginal phenomenon.<sup>6</sup>

Second, once all other factors in the table are taken into account, there are only very small differences in levels of measured tolerance between the two countries: the greatest part of the apparent difference between countries in the aggregate percentages in Table 1 can be accounted for by differential relationships of education to tolerance. That is, it would seem that Germans give more tolerant aggregate responses on the scale because the less well educated there respond about as tolerantly as do the better educated in both countries. Third, there is no significant change during the 1970s in the level of tolerance as measured by the scales: we find no evidence at this point of a posited rise in neoconservatism understood as a decline of liberal tolerance (cf. Davis, b). Finally, these tolerance scales are statistically unrelated to party preference. The effects of left-right or liberal-conservative self-identification were also tested (not shown; results available from the author), and although there was a tiny association, it did not appear to be ideological, for the centrists expressed least tolerance (cf. Sullivan et al., a).<sup>7</sup>

#### DISAGGREGATED TOLERANCE ITEMS

Up to this point we have analyzed a *scale* of tolerance items in order to summarize a larger body of information and also to tap an underlying dimension of tolerance, if possible, by referring to nonconformist speakers of varying ideologies. We now disaggregate this scale into its constituent parts to see whether any of the correlates of tolerance thus far determined can be localized within one or another subrealm. We thus compute and

analyze a second contingency table for both countries and for both time points with occupation, cohort, and education, items measuring tolerance for an atheist speaker and for a communist speaker, and in some tests, party preference as a dependent variable; and also a third contingency table for both countries, but only for the later 1970s, including occupation, cohort, and education, items measuring tolerance for an atheist speaker, for a communist speaker, *and* for a neo-Nazi speaker, and in some tests, left-right self-placement as an intervening variable. (Remember that these contingency tables are not shown here, as noted at the beginning of the "Analysis" section.) The effect parameters for the final fitted models are shown in Table 3. (The first six columns of the table refer to the second contingency table, and the last three columns of the table refer to the third contingency table.)

We again proceed thematically, examining the impact of each independent variable in turn. First, however, we should note that the separate dependent tolerance items are themselves highly related. In fact, the associations between them are the strongest observed: the association between the atheist and communist items in the second contingency table alone explains 47 percent of the variation relative to the base model, and the associations among the three tolerance items in the third contingency table explain about as much. There is also evidence that the association between the atheist and communist items is stronger in the U.S. than in Germany, but the effect is of small magnitude (.05) and it explains less than 1 percent additional variation relative to the base model. Second, we should also note that left-right self-placement is never importantly related to any of the tolerance items in the third contingency table: the strongest such association, with tolerance for a communist speaker, is only statistically significant at the .10 level. Since model fitting on this table used so much computer time, the table was collapsed over left-right self-placement in order to obtain a smaller less expensive table, and testing proceeded from the basis of the results obtained to this point. (This procedure is justified by the so-called collapsibility theorem, see Bishop et al., 47.)

Beginning again with the relationship between education and tolerance, we recall that although education had a large impact on the tolerance *scale* in the U.S. samples, it had virtually none in the 1970 German sample, but a small effect in the 1979 German sample did appear in the direction of the American results. A glance at the results for the scale *items* in Table 3, however, suggests that these relationships are unevenly distributed in the different realms of tolerance tested: tolerance in the religious realm differs somewhat from tolerance in the political realm, and these relationships vary from country to country. Since the results are fairly complex, we examine them item by item.

**Table 3.** POLITICAL TOLERANCE THE UNITED STATES 1972-77 AND WEST GERMANY 1970-79  
SELECTED EFFECT PARAMETERS FROM FINAL LOG-LINEAR MODELS TOLERANCE ITEMS\*

	G-70,79/US-72,77		G-70/US-72		G-79/US-77		G-79/US-77		
	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)	(c)
<u>Occupation</u>									
Worker	-.11	--	-.13	--	-.14	--	-.13	--	--
Employee	-.01	--	-.02	--	.00	--	.01	--	--
Self-employed	.12	--	.15	--	.14	--	.12	--	--
<u>Cohort</u>									
Post 1965	.35	.13	.33	.12	.39	.11	.37	.07	.12
1949-65	.05	.08	.08	.03	.01	.14	.00	.10	.08
1933-48	-.07	-.03	-.07	-.00	-.08	-.05	-.07	-.02	-.05
Pre 1933	-.33	-.18	-.34	-.15	-.32	-.20	-.29	-.14	-.14
<u>Education</u>									
Volksschule + grade	-.18	-.28	-.26	-.25	--	-.30	--	-.23	-.13
Mittlere reife + high school	.02	-.04	.03	-.02	--	-.07	--	-.05	-.07
Abitur + univ. + college	.16	.32	.23	.27	--	.37	--	.28	.20
<u>Country</u>									
Germany	.20	.00	.16	-.06	.24	.07	.31	--	-.16
U.S.	-.20	.00	-.16	.06	-.24	-.07	-.31	--	.16
<u>Year</u>									
1970-72	.03	--							
1979-77	-.03	--							
<u>Atheist</u>									
More tolerant	x	.53	x	.50	x	.57	x	.49	.26
<u>Communist</u>									
More tolerant	x	x	x	x	x	x	x	x	.47
<u>Country-Education</u>									
<u>Germany</u>									
Volksschule	--	.14	--	.23	--	--	--	--	.15
Mittlere reife	--	.03	--	.05	--	--	--	--	.04
Abitur + univ.	--	-.17	--	-.28	--	--	--	--	-.19
<u>U.S.</u>									
Grade school	--	-.14	--	-.23	--	--	--	--	-.15
High school	--	-.03	--	-.05	--	--	--	--	-.04
College	--	.17	--	.28	--	--	--	--	.19
<u>Country-Atheist</u>									
Germany-More Tolerant	x	-.05	x	--	x	-.09	x	--	--
U.S.-More Tolerant	x	.05	x	--	x	.09	x	--	--
<u>Country-Year</u>									
Germany-1979	.07	--							
U.S.-1977	-.07	--							

\*Positive values indicate greater tolerance of (a) an Atheist, (b) a Communist, (c) a Neo-Nazi speaker, as against lesser tolerance.

Dash (--) indicates interaction statistically insignificant (at .10): parameter set to zero. All other interactions significant at least at .05.

### *Tolerance of an Atheist Speaker*

There are no significant differences between the countries here, although an examination of the *insignificant* interaction effects (not shown) suggests that the relationship between education and the atheist item is slightly stronger in the U.S. than in Germany. Likewise, in the second contingency

table there is no significant temporal change in this association in the final model, although it attains statistical significance at an earlier stage of model-fitting: more importantly, however, the results for the second contingency table disaggregated by year suggests that in the early 1970s the interaction had a spread about zero of .26, but by the late 1970s the interaction had become statistically insignificant (see the third and the fifth columns in Table 3). In the third contingency table for the late 1970s, the association between education and the atheist item is also statistically insignificant. Again, however, an examination of the insignificant interaction term (not shown) suggests that the association did not shrink so much in the U.S. as it did in Germany during the 1970s.

#### *Tolerance of a Communist Speaker*

Here we see a significant difference between the countries in the early 1970s but not in the late 1970s; that is, there is a process of *convergence* in the impact of education on tolerance of a communist speaker. By adding the appropriate effect parameters in Table 3, we find that this interaction has a spread about zero of .55 for the U.S. in 1972, .02 for Germany in 1970, and about .30 for both countries in the late 1970s: if we take the statistically insignificant interaction term (not shown) into account in the late 1970s, these figures would become about .45 for the U.S. in 1977 and about .20 for Germany in 1979.

#### *Tolerance for a Neo-Nazi Speaker*

This item is available for both countries only in the late 1970s. There is a statistically significant difference in the association with education between the two countries, and by performing the same sort of additions in Table 3, we find that the interaction has a magnitude of .39 for the U.S. in 1977 and .02 for Germany in 1979.

Thus, (1) the association between education and tolerance is somewhat stronger in the political than in the religious realm, (2) the associations remain fairly constant in the U.S. throughout the 1970s, and (3) the associations are rather low in Germany during the 1970s, but a relationship emerges with the communist item in 1979 which probably accounts for the small effect in the *scale* scores noted above. This latter finding has more than methodological significance, however: it suggests that if orthodox Western political culture is spreading in Germany, it is beginning with the extension of political tolerance among the better educated to left extremists. Tolerance of the speaking rights of right-wing extremists among the better educated may take longer because of still-unresolved problems in German political culture in dealing with the Nazi past.<sup>8</sup>

Examining generational effects next, we see that cohort has its greatest impact on tolerance for an atheist speaker, somewhat less impact on tolerance for a left or right radical political speaker.

always more tolerant than the old; however, unlike the association with education, there are no cross-national differences in the relationship between cohort and tolerance. In the second contingency table, for instance, the interaction between cohort and the atheist item explains an additional 15 percent of the variation relative to the base model, while the interaction between cohort and the communist item only explains an additional 2 percent of this variation. In general, the effect parameters for the atheist item in Table 3 have a spread about zero of at least .35, while the parameters for the political items mostly remain under .15 in magnitude. It is, of course, usual to find that the effects of secularization appear strongly across generations, but on the basis of testing other variables (not shown), I rather expected an equally strong impact of cohort on political tolerance: perhaps generational effects on political tolerance emerge most clearly when the test is a more difficult one, like a scale which requires expressed tolerance of both ends of the political spectrum simultaneously.

A handful of observations remain to be made about the correlates of the tolerance items in the second and third contingency tables. The first is the association between occupation and tolerance. The disaggregation of the scales shows clearly that, at least in these data, any working-class intolerance is not political authoritarianism, but at most a kind of clericalism or fundamentalism, for there are only significant associations between class (occupation) and religious tolerance, none with political tolerance. Furthermore, these interactions for the tolerance items explain even less of the variations (1 or 2 percent) relative to their base models than did the interaction for the tolerance scale.

The second observation concerns party preference. In contrast to the tests of the tolerance scale, which found no significant association between tolerance and party preference, a very small but highly significant association was found for the communist item when the scale was disaggregated into its constituent parts (these effects not shown in Table 3). Social Democratic supporters in Germany and political Independents in the U.S. are somewhat more tolerant than average of a communist's right to speak publicly, but the whole interaction accounts for only 2 percent of the variation over the base model, and the effect parameters are tiny in magnitude. One should not make much of such a small association, but we might note that while there seems to be some partisan solidarity on the left in Germany, the higher levels of tolerance, or if one will, liberalism, appear among the Independents rather than the Democrats in the U.S. (For discussions of the neo-liberalism of Independents in American politics, see Burnham; Ladd and Hadley.) It would perhaps be better to ask why there is *no* association between Christian Democrats and (lack of) tolerance for an atheist speaker.

The last observation concerns the effects of time and place. While changes over time in the levels of tolerance, net of changes in related



variables, are either entirely insignificant or near zero, the two countries differ from each other in levels of tolerance in a most interesting way. First, there is much less tolerance of an atheist speaker in the U.S. than in Germany, and the gap actually widened through the 1970s: this difference fully supports the view that secularism is more widespread in Germany (and perhaps in Europe) than in the U.S. (cf. *Public Opinion*, 38–9) and that there has been a revival of religiosity in the U.S. in the last decade, but none comparable in Germany (as yet) (but cf. Gallup and Poling, Appendix). Second, there is considerably more tolerance for a neo-Nazi (actually, militarist/antidemocrat) speaker in the U.S. than in Germany: this difference gives no support to the view that the Germans still have protofascist leanings compared to the Americans, but it does suggest that they may be exceedingly cautious due to their recent history. (One should, of course, note that this item may have caused some confusion in Germany simply because the German constitution not only guarantees rights of free speech [Art. 5], but also prohibits advocacy of anti-democratic doctrines [Art. 18]. Compare the exact question texts used here.) Third, there are *no* significant differences between the two countries in tolerance for a communist speaker, net of the effects of the other variables: this lack of difference contradicts any simple theory of a prolonged cold-war atmosphere in Germany relative to the U.S. or of an increased repressive potential, aimed primarily at the left, since the early 1970s, said to manifest itself especially in discriminatory hiring practices for public service employment (*Berufsverbot*; see the 1978 Russell Tribunal) or perhaps, in some of the measures taken against terrorist activity.

## Summary

Some analyses of West German data suggested that this country deviated from the standard patterns found in the literature on liberal democratic values, especially in the U.S. A more systematic comparison of very similar, but not identical, tolerance data for the two countries in the 1970s has substantially confirmed this impression and raised several questions, which will be addressed in the next section. The basic findings are these:

### *Education*

Education is very strongly related to tolerance in the U.S., with the better educated more tolerant. This relationship holds uniformly true for each of the items of the tolerance scale, but the associations are stronger for the political than for the religious items. In contrast, there are virtually no such associations in the German data, although there is some evidence that they may have slightly emerged in the latest survey. However, this lack of association is differentially distributed among the it-

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